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SCIENCE PROGRESS  
IN THE TWENTIETH CENTURY  
A QUARTERLY JOURNAL OF  
SCIENTIFIC WORK  
& THOUGHT

EDITOR

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VOL. XXI

1926—1927

LONDON

JOHN MURRAY, ALBEMARLE STREET, W.

1927



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# SCIENCE PROGRESS

## RECENT ADVANCES IN SCIENCE

**PURE MATHEMATICS.** By F. PURYER WHITE, M.A., St. John's College, Cambridge.

THE first part of the new publication of the London Mathematical Society, the *Journal*, contains two papers of an historical kind, which should be read by everybody interested in the development of mathematics during the nineteenth century. The first is an account, by Prof. H. F. Baker, of the work of Felix Klein, an honorary member of the Society, who died in 1925, and whose collected papers, recently published in three volumes, with a running personal commentary by Klein himself, form an enduring monument to their author. The second is an account of the early history of the society delivered by Dr. J. W. L. Glaisher as an informal lecture after the dinner with which the Society celebrated the sixtieth anniversary of its foundation. Readers of De Morgan's *Budget of Paradoxes* (Open Court Edition, vol. 1, pp. 376-83) will rejoice that his lament, "Not a drop of liquor is seen at our meetings, except a decanter of water," was at any rate not justified on this occasion.

*Analysis.*—G. H. Hardy (*Proc. L.M.S.*, 24, 1925, 1-11) adds several examples to those already known in which the repeated integrals

$$\int_a^b dx \int_c^d f(x, y) dy \text{ and } \int_c^d dy \int_a^b f(x, y) dx$$

both exist and have different values. The same author (*ibid.*, xxx-xxxi) gives two different proofs of Schuster's conjecture that the value of the definite integral  $J = \int_0^\infty (C^2 + S^2) dx$ ,

where  $C = \int_x^\infty \cos t^2 dt$  and  $S = \int_x^\infty \sin t^2 dt$ , which turns up in a problem of physical optics, is  $\frac{1}{2}\sqrt{(\frac{1}{2}\pi)}$ ; one proof is based on the theory of Fourier transforms, the other is formally very simple but ignores any difficulties in changing the order of the integrations, and Prof. Hardy remarks that a strict analytical proof in this way would be rather troublesome to write out.

A. E. Ingham (*Journal L.M.S.*, **1**, 1926, 34-5) has since given a proof which avoids all difficulties, formal or theoretical, and which further leads to the results :

$$\int_0^\infty C^2 dx = \frac{1}{8}\sqrt{\pi}, \int_0^\infty S^2 dx = \frac{1}{2}\sqrt{(\frac{1}{2}\pi)} - \frac{1}{8}\sqrt{\pi}, \int_0^\infty CS dx = \frac{1}{4}\sqrt{(\frac{1}{2}\pi)} - \frac{1}{8}\sqrt{\pi}.$$

C. Pólya (*Proc. L.M.S.*, **24**, 1925, lvii) gives, on half a page, a proof of the inequality, due to Carleman,

$$\sum_{n=1}^{\infty} (a_1 a_2 \dots a_n)^{1/n} > e \sum_{n=1}^{\infty} a_n,$$

if  $a_n \geq 0$  and the series on the right-hand side converges to a sum which is greater than zero ; also  $e$  cannot be replaced by any smaller constant.

By means of the theory of Fourier series, N. Lusin showed, in 1913, that if  $f(x)$  is any function of integrable square then the integral

$$\int_0^1 \frac{f(x+t) - f(x-t)}{t} dt,$$

considered as  $\lim_{t \rightarrow 0} \int_t^1$ , exists for almost all values of  $x$ . A

direct proof, without using Fourier series, has recently been given by A. Besikovitch (*Fundamenta Mathematicæ*, **4**, 1923, 172-95). Moreover, A. Plessner has shown that the condition that the square of  $f(x)$  should be integrable is not essential ; he has extended the result to all functions which have a Lebesgue integral, whether their squares are integrable or not. E. C. Titchmarsh (*Proc. L.M.S.*, **24**, 1925, 347-58) has now proved the following results in this connection :

(1) The function  $\frac{1}{t}$  cannot be replaced by any function which tends to infinity more rapidly as  $t \rightarrow 0$  ; further, the integral may diverge in a set of positive measure, even if the function  $f(x)$  is continuous.

(2) The convergence of the integral is due not to the smallness of  $f(x+t) - f(x-t)$  for small values of  $t$ , but to the interference of positive and negative values ; for the integral :

$$\int_0^1 \frac{|f(x+t) - f(x-t)|}{t} dt$$

may diverge in a set of positive measure.

(3) The integral  $\int_0^1 \frac{f(x+t) - f(x)}{t} dt$ , although apparently similar, may diverge in a set of positive measure.

S. Beatty (*Proc. L.M.S.*, **24**, 1925, 339-46) develops a new method of procedure for expressing a polynomial in one variable.

of which the coefficients are power series in other variables, as the product of factors which are linear in the one variable. The method is applicable if some of the linear factors are repeated and the method of effecting the factorisation in the case of  $N + 1$  additional variable flows naturally from and employs the result of that for  $N$ .

G. Pólya (*Journal L.M.S.*, 1, 1926, 12-15) gives the following theorem which, he remarks, "exhibits an essential characteristic of the notion of order." If  $g^{(*)}$  and  $h^{(*)}$  are integral functions and  $g(h^{(*)})$  is an integral function of finite order, then there are only two possible cases: either (a) the integral function  $h^{(*)}$  is a polynomial and the external function  $g^{(*)}$  is of finite order; or else (b) the internal function  $h^{(*)}$  is not a polynomial but a function of finite order and the external function  $g^{(*)}$  is of zero order. Dr. Pólya has not yet been able to find a proof which does not involve some rather elaborate result connected with the theorem of Picard.

M. Felsete (*Journal L.M.S.*, 1, 1926, 15-19) proves the following inequality for the order of magnitude of the number  $N(\tau)$  of zeros of the Riemann zeta-function  $\zeta(s) = \zeta(\sigma + it)$  on the critical line  $\sigma = \frac{1}{2}$ , for which  $0 < t < \tau$ ,

$$\liminf_{\tau \rightarrow \infty} \frac{N_o(\tau)}{\sqrt{\tau}/\log \tau} > 0.$$

This is not the best inequality that has been proved, as Hardy and Littlewood have shown that

$$\liminf_{\tau \rightarrow \infty} \frac{N_o(\tau)}{\tau} > 0,$$

but the author considers that the simple method of proof which he uses may be of theoretical interest in itself and may be useful for analogous problems.

An anonymous writer, in a letter to Prof. L. J. Mordell (*Journal L.M.S.*, 1, 1926, 66-8), proves that the Diophantine equation

$$y^2 = a_0 x^n + \dots + a_n \quad (n \geq 3)$$

has only a finite number of solutions in integers  $x, y$ , if the polynomial  $a_0 x^n + \dots + a_n$  has only simple zeros—a result conjectured by Mordell, but only proved by him for  $n = 3$ .

L. J. Mordell (*Journal L.M.S.*, 1, 1926, 68-72), by a method which he had developed for the evaluation of Gauss's sums, proves very simply the approximate functional formulæ for the theta-function in the form: If  $0 < \omega < 2$ ,  $0 \leq x < 1$ , then

$$\sum_{n=0}^{\infty} e^{\pi i n^2 \omega + 2\pi i n x} = \sqrt{\left(\frac{i}{\omega}\right)} \sum_{r=0}^{\infty} e^{-\pi i (x - \frac{r}{\omega})^2 / \omega} + O\left(\frac{1}{\sqrt{\omega}}\right),$$

where  $O(1/\sqrt{\omega})$  denotes a function of  $n$ ,  $\omega$ , and  $x$  which is in absolute value less than a constant multiple of  $1/\sqrt{\omega}$ .

In the sixth of their series of papers, "Some Problems of Partitio Numerorum" (*Math. Zeits.*, **23**, 1925, 1-37) Hardy and Littlewood proved the following result for the number  $N_{k,s}(x)$  of distinct numbers  $n \leq x$  which are expressible as the sum of  $s$  (or fewer) positive  $k$ th powers ( $k > 2$ ,  $s \geq 2$ );

$$\lim_{x \rightarrow \infty} \frac{N_{k,s}(x)}{x^{as-\epsilon}} > 0,$$

where  $a_s = 1 - (1 - 2k^{-1})(1 - k^{-1})^{s-2}$ , and  $\epsilon$  is any positive number.

E. Landau (*Journal L.M.S.*, **1**, 1926, 72-4) now proves that if  $k$  is even, more than this is true, viz.:

$$\lim_{x \rightarrow \infty} \frac{N_{k,s}(x) \log x}{x'^s} > 0.$$

C. H. Hardy and J. E. Littlewood (*Journ. L.M.S.*, **1**, 1926, 19-24) generalise a theorem on Fourier series, due originally to Fatou and proved under more general conditions by themselves. Fatou's original theorem is that if

$$\frac{1}{2} a_0 + \sum (a_n \cos nt + b_n \sin nt) = \frac{1}{2} A_0 + \sum A_n$$

be the Fourier series of an integrable function  $f(t)$ , and

$$a_n = o\left(\frac{1}{n}\right), b_n = o\left(\frac{1}{n}\right),$$

then the necessary and sufficient condition that the series should converge, when  $t = x$ , to the sum  $s$  is that

$$\frac{1}{2t} \int_0^t \{f(x+u) + f(x-u) - 2s\} du$$

should tend to zero when  $t \rightarrow 0$ . They now show that the theorem is true, provided only that  $n A_n > -C$ , where  $C$  is a positive constant and they also prove the corresponding theorem for the conjugate series.

P. R. Ansell and R. A. Fisher (*Proc. L.M.S.*, **24**, 1925, liv-lv) draw attention to a previously unexpected connection between the derivative of a Bessel Function

$$\frac{\partial}{\partial \nu} J_\nu(x)$$

and the sine and cosine integrals, which enables the value of the former, when  $\nu$  is the half of an odd integer, to be calculated directly from tables of the latter.

R. C. Cooke (*Proc. L.M.S.*, **24**, 1925, 381-420) establishes conditions for the truth of certain inversion formulæ involving

Bessel functions which have been given recently by Hardy and Titchmarsh.

C. Fox (*Proc. L.M.S.*, **24**, 1925, 479-93) investigates, by the methods of contour integration, a class of Schlömilch series (for which see Watson, *Theory of Bessel Functions*, chapter 19), which represent a null-function.

The differential equation

$$\frac{d^2y}{dx^2} + \xi \sin 2x \frac{dy}{dx} + (\eta - p\xi \cos 2x) y = 0$$

possesses, for certain discrete values of  $\eta$ , a solution of period  $2\pi$  expressible in a finite form. E. L. Ince, to whom this result is due, has now (*Proc. L.M.S.*, **25**, 1926, 53-8) investigated the real zeros of those periodic solutions which are of finite form.

He shows that, when  $m < p$ , those periodic solutions of finite form which reduce respectively to  $\cos mx$  and  $\sin mx$  when  $\xi = 0$  have each  $m$  distinct zeros in the interval  $0 \leq x < \pi$ . When  $\xi = 0$  the zeros are equally spaced in the interval; as  $\xi \rightarrow +\infty$  they tend to cluster in a small interval  $(\frac{1}{2}\pi - \delta, \frac{1}{2}\pi + \delta)$ , and as  $\xi \rightarrow -\infty$  they tend to cluster in the two intervals  $(0, \delta)$  and  $(\pi - \delta, \pi)$ , where  $\delta$  is a positive number depending upon  $\xi$ , and tending to zero as  $\xi$  tends to infinity.

M. J. M. Hill, R. C. Cooke, and F. M. Wood, in a paper entitled "On Borel Intervals" (*Proc. L.M.S.*, **24**, 1925, 319-34), examine the overlapping of the intervals

$$\left(\frac{p}{q} - \frac{\alpha}{\beta} \frac{1}{q^n}, \frac{p}{q} + \frac{\alpha}{\beta} \frac{1}{q^n}\right),$$

in which  $p, q$  are positive integers prime to one another ( $p < q$ ),  $n$  is a positive integer not less than 3, and  $\alpha, \beta$  are positive integers prime to one another, such that  $\beta^{n-1} > \alpha^n$ , including the case  $\alpha = \beta = 1$ . They find that the overlapping is subject to four laws: (1) If the centre of an interval be inside another interval, then the whole of the first interval lies inside that half of the second interval in which the centre of the first interval lies; (2) If the centre of one interval be at an end-point of another, then that half of the first interval which lies inside the second interval does not extend as far as its centre; (3) If the centre of one interval be inside a second interval, then either the first interval is wholly inside the second or else the first interval contains the second in one of its halves; (4) Two intervals cannot have a common end-point. The paper had its origin in an attempt to determine approximately what part of the unit length is covered by the intervals

$$\left(\frac{p}{q} - \frac{1}{q^n}, \frac{p}{q} + \frac{1}{q^n}\right).$$

Borel, who called attention to these intervals in his *Leçons sur la Théorie des Fonctions*, p. 39, showed that the total length does not exceed

$$2 \sum_{q=2}^{\infty} (q^{-2} - q^{-3}) = 0.885754 \dots,$$

but this includes an infinite number of lengths counted many times over. In the paper cited it is found that the length covered lies between 0.620705 and 0.623476; but, as the authors say, "the interest [of the four laws] overshadows that of the original quest."

*Geometry*.—H. W. Richmond (*Proc. L.M.S.*, **24**, 1925, xvi–xix) has a preliminary note on algebraic surfaces containing five systems of conics. They are rational surfaces and the mapping on a plane may be done by means of polynomials, in five parameters, which are such that their ratios are unaltered when every parameter  $\theta_r$  ( $r = 1, \dots, 5$ ) is replaced by  $(p\theta_r + q)/(m\theta_r + n)$ , whatever  $p, q, m, n$  may be. Such "invariantive functions," as they are called by Elliott, are products of differences of the parameters in which each appears the same number of times, or sums of such functions. For five parameters there are twelve simple invariantive functions in which each parameter occurs to the second degree, twelve like

$$(\theta_1 - \theta_4)(\theta_4 - \theta_2)(\theta_2 - \theta_3)(\theta_3 - \theta_5)(\theta_5 - \theta_1)$$

and ten like

$$(\theta_1 - \theta_3)^2(\theta_2 - \theta_4)(\theta_4 - \theta_5)(\theta_5 - \theta_2),$$

but it is found that only six of the twenty-two are linearly independent. The surface of which the co-ordinates are proportional to sums of these functions must, therefore, lie in space of five dimensions or less. All such surfaces have five systems of conics, obtained by letting each parameter in turn vary, the other four remaining constant; one conic of each system passes through every point of the surface, and any two conics of different systems have one common point. The surfaces (of order five) in space of five dimensions, from which the others may be derived by projection, are all projectively identical; they contain ten straight lines, obtained by making two parameters equal.

Mr. Richmond refers, in passing, to the invariantive functions of six parameters; by using functions of the type  $(\theta_1 - \theta_5)(\theta_3 - \theta_6)(\theta_2 - \theta_4)$  we obtain Segre's cubic variety (of three dimensions) in space of four dimensions, with six families of straight lines—this was discussed by Castelnuovo in 1891. For more than six parameters little or nothing is known of the invariantive functions; the matter is very complicated, but should repay investigation.

Reference has already been made in SCIENCE PROGRESS to a paper by A. E. Jolliffe (*Proc. L.M.S.*, **23**, 1924, 250-78) in which he shows that, for a plane quartic curve, the twenty-four inflexional tangents and the twelve bitangents of a Steiner complex touch a curve of class six. Jolliffe's treatment is analytical; W. P. Milne (*ibid.*, **24**, 1925, 335-8) has now studied the problem from the standpoint of synthetic geometry and invariants and obtains further properties, among which may be mentioned: (1) There is a curve of class eight which touches the twenty-four inflexional tangents of a quartic curve and has the twelve lines of a Steiner complex for bitangents; (2) there is at least one configuration of sixty-three conics, each of which touches the harmonic envelope of the quartic curve at six distant points.

In view of the well-known connection between plane quartic curves and cubic surfaces, W. P. Milne (*Journal L.M.S.*, **1**, 1926, 7-12) has been led to consider analogous problems for the cubic surface, and in particular the relationship of the harmonic and equianharmonic envelopes, *i.e.* the surfaces (of class six and four, respectively) enveloped by planes which cut the cubic surface in cubic curves whose invariant cross-ratio is harmonic or equianharmonic, to the straight lines of the surface. He obtains a number of interesting results from which Jolliffe's results for the plane quartic may be obtained in the familiar way, by considering the enveloping cone from a point of the surface. These results of Milne's are based on the following fundamental theorem, which is believed to be new: The twenty-four tangent planes to a cubic surface at the twenty-four parabolic points of a double-six touch a quadric. The parabolic points of a line of the surface are of course the double points of the involution of pairs of points of contact of the surface with planes through the line; for a double-six, the twenty-four parabolic points are its intersections with the corresponding Schur quadric. This theorem Milne obtains by rather heavy algebra, which is suppressed in the printed paper; it would be of the greatest interest to obtain a synthetic proof, and there clearly remains a great deal to be done on a subject, the cubic surface, on which so much has been written since Cayley observed, in 1849, in a letter to Salmon, that a definite number of straight lines must be on the surface.

H. P. Hudson and T. L. Wren (*Proc. L.M.S.*, **24**, 1925, xxviii-ix) prove that in the general quadro-quadric Cremona space transformation, *i.e.* the birational transformation of space in which to a plane there corresponds, in either sense, a quadric surface, there are two involutory point-pairs; this corrects erroneous statements by Aschieri and Doehleemann, that there are no such pairs. Following on this, C. G. F. James

(*Journal L.M.S.*, 1, 1926, 3-7) calculates the number of cycles of  $i$  points in a general Cremona transformation of space of any number of dimensions. By a cycle of  $i$  points is meant a group of  $i$  points  $A_1 A_2 \dots A_i$  such that to  $A_i$  corresponds in the direct sense  $A_{s+1}$  ( $s = 1, 2, \dots, i-1$ ), while to  $A_i$  corresponds  $A_1$ . For a birational transformation of space of  $r$  dimensions there are  $r-1$  orders,  $m_1, m_2, \dots, m_{r-1}$ , the orders of the varieties corresponding in the direct sense to linear spaces of dimensions  $r-1, r-2, \dots, 2, 1$ ; it is easily seen that in the inverse sense to a linear space of dimensions  $t$  there corresponds a variety of order  $m_t$ . Mr. James proves, by means of a correspondence principle due to Schubert and Zeuthen, that the number of united points ( $i=1$ ) is  $\sum_{j=1}^{r-1} m_j + 2$  (there is an obvious misprint on p. 7), the number of involutory pairs ( $i=2$ ) is  $\sum_{j=1}^{r-1} \frac{1}{2} m_j (m_j - 1)$  and the number of cycles of  $i$  points is

$$F_i(m_1 \dots m_{r-1}) = \frac{1}{i} \left[ \sum_{j=1}^{r-1} (m_j^i - m_j) - \sum p F_p(m_1 \dots m_{r-1}) \right],$$

where the summation is over all factors  $p$  of  $i$  ( $1 < p < i$ ). The result of Miss Hudson and Mr. Wren is included as a particular case ( $r=3, m=n=2$ ).

C. G. F. James has recently published two long papers dealing with enumerative results in line geometry. In one (*Trans. Camb. Phil. Soc.*, 23, 1925, 201-34) he obtains a number of results relating to scrolls (ruled surfaces) in ordinary space, in particular with regard to the multiple tangents of the scrolls. The method depends upon what the author calls "a new principle relating to their degeneration," which he states as follows: In any permissible degeneration of a scroll the double curve is represented by the system of double curves and curves of intersection, residual to the common generators, of the scroll into which the given scroll degenerates. This principle makes possible the use of the functional method of Cayley and Severi, which has already been found of enormous value in determining enumerative properties of curves. The principle is applied in the first four sections to obtain some known results, namely, the number of triple points and the genus of the double curve on a scroll of general type; but most of the results are new. A second part applies the same principle to the determination of the order of the double curve, and so on, on the scroll of lines common to a given congruence and a given complex.

The other paper (*Proc. L.M.S.*, 24, 1925, 359-80) investigates line systems in higher space. A system of characteristics is defined, in terms of which the orders of the constructs generated

by the lines of a system which satisfy simple incidence conditions can be expressed. The order of the construct common to two line systems of suitable dimension is then obtained in terms of these characteristics. From these formulæ all the characteristics of the system of intersection of any two systems can be read off when required.

**METEOROLOGY.** By E. V. Newnham, B.Sc., Meteorological Office, London.

IN *The Quarterly Journal of the Meteorological Society* (vol. 51, No. 216, Oct. 1925, pp. 357-62) there is a paper entitled "Gustiness of Wind in Particular Cases," by A. H. R. Goldie. A few remarks may not be out of place concerning the exact meaning attached to the word "gustiness" by professional meteorologists in this country, and the distinction between gusts and squalls, before passing on to consider this paper in detail. In a publication of the British Meteorological Office, entitled *The Meteorological Glossary*, gusts are defined as rapid fluctuations of the wind, the time elapsing between a minimum and a maximum in the wind-speed amounting generally to a few seconds. These fluctuations are attributed to eddies set up by obstacles—trees, buildings, etc.—in the path of the wind, and are not of purely meteorological origin. Squalls, on the other hand, are sudden increases of wind which last for some minutes and then suddenly subside, and are believed to be entirely of meteorological origin.

Goldie's paper deals only with a particular class of gustiness, namely, that produced by sea-waves. One might expect, from the regular character of sea-waves, that the gustiness to which they give rise will be more regular than the gustiness due to obstacles on land, and this is found to be the case.

Taking the sea and wind to be in a steady state, the mean hourly velocity of the wind being  $V^1$ , while the waves have the ideal form:

$$y = a \sin k(x - Vt) \dots (1).$$

Where  $y$  is the vertical displacement of a particle of water at time  $t$ ,  $V$  the speed of travel of the waves,  $x$  is distance measured horizontally, and  $a$  and  $k$  are appropriate constants, if the water be sufficiently deep, then, as is shown in text-books on hydrodynamics, the velocity of propagation is given by the following equation:

$$V^2 = \frac{g\lambda}{2\pi} \dots (2).$$

The expression for the horizontal velocity of the wind at any point found by Goldie is as follows:

$$U^1 - ka(U^1 - V)e^{-kz} \sin k(x - Vt) \dots (3).$$

According to this equation a periodic variation of amplitude  $ka(U^1 - V)e^{-kx}$  is superposed upon the mean velocity  $U^1$ . The range of gusts diminishes upwards according to the exponential  $e^{-kx}$ . The range of gusts at the surface is clearly  $2ka(U^1 - V) \dots \dots \dots (4)$ ;

$$i.e. \frac{2\pi a}{\lambda} \left( U^1 - \sqrt{\frac{g\lambda}{2\pi}} \right) \dots \dots \dots (5).$$

Vaughan Cornish has observed that in ocean waves  $\frac{a}{\lambda}$  is generally between  $\frac{1}{10}$  and  $\frac{1}{20}$ . It is possible, therefore, to obtain an idea of the range and period of gusts for various values of  $\frac{a}{\lambda}$ ,  $U^1$  and  $\lambda$ , and a table with this information is accordingly given. The next step is to see whether any evidence for the soundness of equation (3) can be obtained. Anemograms made at Lerwick were examined for the case of "equatorial" air-currents, in which the air had presumably travelled a great distance over the sea, to find the range of gusts for various mean velocities. Considering groups of speeds varying from 10 to 100 ft./sec., the mean range of gusts was found to be roughly a third of the mean velocity in each case. Taking  $R$ , therefore, to be  $\frac{1}{3} U^1$ , theoretical values of the "period,"  $T$  for the sea and air waves were calculated from equations (2) and (4).  $T$  varied from  $1\frac{1}{2}$  to  $2\frac{1}{2}$  seconds for a velocity of 10 ft./sec. to 7 to 10 seconds for a velocity of 100 ft./sec. These periods were not verified on the anemograms for Lerwick, as the time scale on the ordinary anemogram is not sufficiently open to allow of this being done; but it had been noted at Falmouth in 1913 and 1914 that during south-westerly gales (usually, at Falmouth, winds that have travelled great distances over the sea) the period of the gusts, as well as the period of the ocean waves, was always about seven seconds, an interval of time of an order of magnitude in accordance with the figures just quoted.

An interesting succession of winds was observed at Lerwick when, after many hours of south-west gale of mean hourly velocity of 60 ft./sec., with 20 ft./sec. range of gusts, the mean velocity fell gradually to 45 ft./sec. and the range of gusts to 10 ft./sec. Later, the mean velocity fell to 36 ft./sec., and the range of gusts to  $6\frac{1}{2}$  ft./sec., the direction remaining constant throughout. A possible explanation is as follows: As the wind dropped its speed approached that of the waves; the waves, owing to their great mass, changed their speed relatively slowly. Equation (4) shows that the gustiness is, other things being equal, proportional to the difference in speed between the wind and the waves, and should therefore have diminished.

The concluding part of this paper deals with certain types

of squall, namely, those arising at a surface of discontinuity in the atmosphere and giving rise to variations in wind velocity near the ground. The period in these cases has any value from five minutes up to one or two hours.

A summary of a paper by D. Brunt, which appears in the *Phil. Mag.* (vol. 1, No. 2, 1926, pp. 523-32) will not be out of place when dealing with the subject of turbulence, although the substance of this paper was communicated to the British Association nearly three years ago. The object of his investigation was to inquire into the rate of dissipation of kinetic energy by the friction due to turbulence in the air-streams comprising what is termed the "general circulation" of the atmosphere. It should be noted that only rough approximations were sought, and, indeed, only such are possible in the present state of knowledge of atmospheric movements over the earth as a whole, although soundings by pilot balloons have furnished detailed information for some regions, particularly the temperate and sub-tropical zones of Europe and North America. The general circulation of the atmosphere in the first few kilometres of height may be divided into two portions; one of these extends roughly from  $30^{\circ}$  S. to  $30^{\circ}$  N., and in this tropical portion the winds are mainly easterly; nearly half of the whole mass of the atmosphere is involved. Outside these latitudes are the westerly winds of temperate latitudes and the relatively small zone of variable to easterly winds round the polar regions. If we take the mean velocity in the first zone to be 10 metres/sec., since the mass is about  $2.7 \times 10^{21}$  grams the kinetic energy will be about  $1.35 \times 10^{27}$  ergs. Equality of the moments of momentum of the two zones requires that the mean velocity of the westerly winds in the second zone be rather greater than 10 metres/sec., which is quite in accordance with observation. The combined kinetic energy of the two zones may be taken as roughly  $3 \times 10^{27}$  ergs.

Some years ago G. I. Taylor studied the variation of wind with height from a theoretical point of view, making use of a coefficient of eddy diffusion ( $K$ ). The same methods of analysis are adopted here in the calculation of the dissipation of kinetic energy by turbulence. It is found that the dissipation above the level at which the wind direction becomes tangential to the isobars is negligible compared with that which takes place lower down. Further calculation shows that the rate of dissipation in this layer is such that, if maintained without any potential energy being converted into kinetic energy to replace that which is lost, all the available kinetic energy will be destroyed in about 14 hours. For a layer extending up to 10 km. of height the mean dissipation per second for each square metre is shown to be about  $5 \times 10^{-3}$  kilowatts, and of this total the region from

the ground up to 1 km. contributes  $3 \times 10^{-3}$  kilowatts, i.e. six-tenths of the total.

Now the total energy above one square metre of the earth's surface, on the assumption of a uniform velocity of 10 metres/sec., is  $5 \times 10^{12}$  ergs, so that an uncompensated rate of dissipation of  $5 \times 10^7$  ergs per square metre per second would dispose of the total kinetic energy in  $10^5$  seconds, or  $1\frac{1}{2}$  days. If, on the other hand, the rate of dissipation be assumed to be proportional to the total kinetic energy at any moment, the total would be reduced to one-tenth of its original amount in three days.

It is interesting to compare the rate of dissipation of energy just calculated with the rate at which radiant energy is received from the sun. Taking the solar "constant" as 2 gm. cal. per cm.<sup>2</sup> per min., and allowing for a loss of about 37 per cent. on account of reflexion from the earth's surface and from the clouds, the effective incoming solar radiation is 0.22 kilowatt per square metre, when averaged over the whole of the earth's surface. The rate of dissipation of energy by turbulence is, therefore, only about 2 per cent. of the incoming radiant heat. In other words, the conversion of slightly over 2 per cent. of the incoming solar radiation into kinetic energy suffices to maintain the general circulation of the atmosphere.

An Electrical Resistance Hygrometer is described by F. Albrecht in *Beiträge zur Physik der freien Atmosphäre*, XI, 4, pp. 164-80. The ordinary hair hygrometer is unreliable at low temperatures and low pressures such as are met with in the higher levels of the atmosphere, and has a large lag. This paper describes an instrument which is based upon the varying electrical resistance of a thread soaked with some hygroscopic substance. Threads of silk were found most satisfactory. Electrolysis was prevented by using an alternating current. It was found that the lag was comparatively small, so that finer "aqueous structure" of the atmosphere could be recorded than with the ordinary hair hygrometer. The instrument has, however, some serious drawbacks. For each hygrometric substance a limited range of humidity only could be recorded; sometimes, with a high humidity, the instrument appears to record much too high, due apparently to the threads collecting cloud particles on hygroscopic nuclei. It is considered possible, however, to make allowances for this last effect. For the very high regions of the atmosphere, sulphuric acid appears to be the most suitable substance of those tried: at a temperature of  $-55^\circ\text{C}$ . it can register humidities between 50 per cent. and 70 per cent., and also between 5 per cent. and 10 per cent.

A useful contribution to the study of the wet-bulb thermometer appears in the *Journal of the American Society of Mechanical Engineers* (Dec. 1924). The paper is by W. H.

Carrier and Daniel C. Lindsay, and the work described is a continuation of W. H. Carrier's earlier researches.

The authors find that in practice the theoretical temperature of evaporation derived by equating, either the heat of evaporation to the decrease of sensible heat in the surroundings (Apjohn's method), or the rate of inward diffusion of heat from the atmosphere due to difference of temperature to the outward heat flow due to the evaporation of the water and its diffusion into the atmosphere (Regnault's method), is normally approximated to in practical hygrometry. The causes of discrepancy are usually :

(1) Incomplete saturation in the film immediately in contact with the wet surface.

(2) Direct radiation from surrounding objects.

(3) Possibly transmission of heat through the stem of the thermometer.

These discrepancies were investigated experimentally, elaborate apparatus being designed and used to ensure adiabatic conditions for the wet-bulb. The results do not admit of being summarised adequately in a short space, but it may be mentioned that in examining the coefficients to be applied in working up observations with sling psychrometers, the authors found that the coefficients used in the U.S.A. Weather Bureau give distinctly low values for the dew-point and moisture content.

*Notes on the Behaviour of Certain Plants in Relation to the Weather*, is the title of a paper in *Q. J. Met. Soc.*, vol. 52, No. 217, pp. 15-24, by N. L. Silvester.

Certain plants are known to undergo changes which appear to be correlated with changes in the weather, and these are held by some people to precede changes in the weather. It is impossible for anyone having any knowledge of physics or plant physiology, still less for anyone also familiar with modern synoptic weather-charts, to hold such a view ; the author of this paper has, however, done a useful piece of work in subjecting the question to the impartial test furnished by a long series of observations of the behaviour of the more common plants believed to have these supernatural powers, followed by a statistical study of the weather at the time and immediately afterwards. The results supported the conclusions of common sense ; a clear connexion was found between the behaviour of the following plants :

Scarlet Pimpernel (*Anagallis arvensis*),  
Common Daisy (*Bellis perennis*),  
Common Chickweed (*Stellaria media*),  
White Clover (*Trifolium repens*),  
Dandelion (*Taraxacum officinale*),

Gentian (*Gentiana pneumonanthe*),  
Marigold (*Calendula pluvialis*),  
Blue Pimpernel (*Anagallis arvensis*),

principally as regards the opening and shutting of the flowers, and the physical state of the air or surface soil at the time of observation ; but there was no evidence that the behaviour of the plants was connected with subsequent weather.

In the case of the daisy, and of chickweed, there is a close response to the temperature of the surface soil ; in the case of these two plants, and of the pimpernel, there was found to be also, above a certain critical temperature, a response to the relative humidity, the flowers closing their petals when the relative humidity exceeded a certain value which was different for each kind of flower. The closing is, therefore, a prognostic of rain to the extent that rain is a little more probable when humidity is high and increasing ; but this is merely weather forecasting from the existing physical state of the air at the place in question, and a hygrometer might equally well be used instead of the flower.

*On the Dynamics of Geostrophic Winds*, by H. Jeffreys (*Q.J. Met. Soc.*, vol. 52, No. 217, pp. 85-104). Dr. Jeffreys has far more mathematical analysis at his command than most meteorologists, and it is fortunate for the science of meteorology when he devotes this gift to the elucidation of problems as difficult as those relating to the general circulation of the atmosphere. In this paper he attempts to calculate the winds that would arise from the differences of pressure arising from certain distributions of temperature, without any reference as to how such distributions might arise. He finds that if friction be ignored, the solution of such problems can be obtained by mathematical methods strictly analogous to those employed in studying tides formed in a homogeneous incompressible ocean of uniform depth. The disturbing potential of the tidal theory is replaced by a function of the mean conditions over the level surfaces and of the departures of the temperatures from mean values over level surfaces. Fair quantitative agreement is found between observed and calculated winds in the case of the monsoons, when this method is employed ; but, in calculating the general circulation, although the mean velocity of the wind agrees fairly well with the mean found from observational data, the direction of the wind should in theory be everywhere easterly. The author naturally attributes the divergence between theory (when friction is neglected) and observation to the neglect of this important factor of friction and proceeds to consider the modifications in the ideal motion which may be deduced when friction is taken into account, the principle of

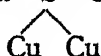
the conservation of angular momentum being made use of in this part of the discussion. He claims to find dynamical necessity for a permanent region of low pressure around each pole, with a continual interchange of air between high and low latitudes by means of cyclones, and that these cyclones must have structures resembling those ascribed to developing cyclones by Bjerknes. From what has been said it will be seen that friction is regarded as a cause of cyclone formation, whereas almost, if not quite, all other writers have regarded friction as the force which tends to destroy existing cyclones and prevent the formation of new ones.

**PHYSICS.** By L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

*The Magnetic Behaviour of Phosphorescent Substances.*—In the December number of the *Annalen der Physik* E. Rupp contributes an important paper on the magnetic behaviour of phosphorescent substances. The latter may be pictured on account of their mode of preparation as made up of particles each consisting of atoms of an alkaline earth, *e.g.* Ca, with their coupling atoms, *e.g.* S, and an atom of a heavy metal. Lenard and Klatt (*Ann. d. Phys.*, vol. 15, p. 425, 1904) have shown that during fusion the alkaline earth and the binding atoms form large molecular groups, in which the atom of the heavy metal necessary for the occurrence of phosphorescence is embedded. The phenomena of phosphorescence should therefore be explicable in a simple manner in terms of the structure of such a molecular arrangement. A model of such a phosphorescent centre was given by Lenard (*Ann. d. Phys.*, vol. 31, p. 641, 1910), and according to this model a phosphorescent centre consists of a combination of a large number of molecules of an alkaline earth sulphide in a ring-like formation, within which the atom of the heavy metal is attached to one of the S atoms, so that a  $\text{CaCu}\alpha$  phosphorescent centre, *i.e.* one which emits red bands, termed  $\alpha$  bands by Lenard, may be represented by a formula  $\text{—Ca—S—Ca—S—Ca—S—}$ . The fact that a



particular metallic atom may give rise to different phosphorescent bands is explained by considering that different values of the valency of the metallic atom are possible within the combination, so that a  $\text{CaCu}\beta$  centre, *i.e.* one which emits blue bands, may be represented by  $\text{—Ca—S—Ca—S—Ca—S—}$ . This mode



of representation was modified by Tomaschek (*Ann. d. Phys.* vol. 65, p. 561, 1924), as a result of his examination of phos-

phorescent substances containing rare earths. Tomaschek assumes that the heavy metal atom is not immediately connected to a S atom in a molecular group, but that some sulphide of the metallic atom is attached to an atom of the alkaline earth, for the different phosphorescent bands may be explained in terms of the various sulphide combinations of the heavy metallic atom present.

Now, so far, we only have indirect information of the nature of the structure of the phosphorescent centre, and direct chemical analysis is out of the question. Therefore, an investigation of the physical properties of these substances and their constituents is of considerable importance. The phosphorescent substances under consideration are formed of a diamagnetic basic material like CaS (plus some diamagnetic substance like NaCl), in which paramagnetic atoms such as Mn, or diamagnetic atoms such as Cu, are embedded. In this paper Rupp considers the magnetic behaviour of these substances and attempts are made to find empirical relations between the magnetic susceptibility and other factors connected with phosphorescent phenomena, *e.g.* the formation of complex molecular groups and their distortion by pressure, the various bands emitted by the same atomic constituent, the temperature variations of the centres, the amount of metal present, etc. Naturally, this is not the place for a detailed discussion of the experiments on these points, but we will consider some of the more important results. In the first place, it is found that groups of molecules of the basic material ( $\text{—Ca—S—}$  and  $\text{—Ca—O—}$ ) are more paramagnetic than the simple molecules (CaS and CaO). Phosphorescent substances with ZnS as basic material are also more paramagnetic in the luminous state than when non-excited. The investigation of the atomic susceptibilities of the various heavy metals as they occur in these compounds shows, in the cases of  $\text{CaMn}\alpha$  and  $\text{ZnMn}\alpha$ , substances which contain very small amounts of Mn, that the Mn possesses a susceptibility of the same order of magnitude as it possesses when in the form MnS, and this is in agreement with the fact that these substances possess absorption bands whose edges correspond to divalent S combinations of Mn. A most interesting result, however, is obtained in the cases of the atomic susceptibilities of the diamagnetic metals Bi, Cu, and Ag. When these metals occur in very small quantities, Rupp assumes that we are able to examine the magnetic properties of individual isolated metallic atoms, which is of course impossible with specimens of the pure metals, for the coupling of the atoms in the crystal lattice strongly disturbs the magnetism of the individual atoms. Measurements made with three phosphorescent substances  $\text{CaBi}\alpha$ ,  $\text{CaCu}\alpha$  and  $\text{SrAg}\beta$ , which contained  $1.2 \times 10^{-5}$ ,

$1.7 \times 10^{-5}$  and  $5.2 \times 10^{-5}$  gm. of Bi, Cu, and Ag respectively, per gm. of substance, show that the atomic susceptibility (diamagnetic) of the metal present in this form is in each case about 1,000 times as great as that in the pure metal or its simple combinations. If these values are confirmed they represent the greatest diamagnetic susceptibilities known to us at present; the diamagnetic susceptibility of all known substances lies between  $10^{-4}$  and  $10^{-7}$ , and a substance with a susceptibility of  $10^{-2}$  has never before been recorded. This enormous diamagnetism is regarded as a property of the isolated atom, which is brought to light in this case because of the particular way in which the metallic atom is bound in the phosphorescent centre, and perhaps is due to an orientation of the individual metallic atoms with respect to the external magnetic field. It will be remembered that Glaser recently described a similar phenomenon in the case of diamagnetic gases, and he considered that it provided evidence of molecular orientation (SCIENCE PROGRESS, No. 78, p. 203). Just as in the case of gases at high pressure the phenomenon is absent, so in the case of phosphorescent substances with appreciable heavy metal content there presumably results a complete disorientation of the atomic axes in a magnetic field. Some evidence that an orientation of the heavy metallic atoms takes place in a magnetic field to a greater extent when the heavy metal content is very small is afforded by the phenomena of luminiscence in electric and magnetic fields (Rupp, *Ann.*, vol. 75, p. 325, 1924). This increase in diamagnetism is greatest in the case of Bi, and then follow Ag and Cu in order. Although the great susceptibility of the individual atoms of Bi, Cu, and Ag will not in these cases permit one to decide the nature of their chemical combination as in the case of Mn, yet other experimental material is sufficient to warrant the assumption that these metals occur in the form of sulphides, in fact a CaCu substance with a very high copper content gave an atomic susceptibility for copper between that of CuS and Cu<sub>2</sub>S.

The magnetic behaviour of phosphorescent substances containing metals of the rare earths is of particular importance because these metals possess such enormously high atomic susceptibilities; moreover, their oxides also possess huge susceptibilities, and these phosphorescent substances emit sharp lines (Tomaschek, *Ann. der. Phys.*, vol. 75, pp. 106 and 561, 1923). In the examination of substances containing CaS, SrS, BaS, CaO and SrO as basic materials with Sm as the heavy metal constituent, it is found that the values of the susceptibility in all these cases are considerably lower than the values for phosphorescent substances containing an equal content of Mn. Also, there seems to be a definite connection between the width

of the doublets and the sharpness of the lines emitted and the decrease in paramagnetic susceptibility corresponding to a decrease in the amount of Sm in the phosphorescent material. It is, therefore, considered that in these centres there must exist electron orbits which circumscribe the whole phosphorescent centre, and that these orbits play a part in the emission of light, perhaps as the outer orbits of an electron liberated from the metal under excitation. We may, perhaps, consider this conception of orbits which circumscribe a molecular complex to be a logical extension of the views of Mulliken (*Phys. Rev.*, vol. 26, p. 259 and p. 451, 1925). The latter has advanced evidence in favour of the view that in molecules, such as CO, N<sub>2</sub>, etc., each atom retains its own K electrons whilst the remaining electrons are held in common by the two nuclei, and may have the same arrangement as in atoms. In the molecules CO and N<sub>2</sub> there are thus eight electrons with a rare gas configuration, together with two "valence" electrons. In this connection, it may be mentioned that Birge (*Nature*, Feb. 1, 1926) has evaluated the energy levels for the CO molecule, and further, has discussed his results in the light of the above conception (*Nature*, Feb. 27, 1926). In fact, his results lead him to make the following generalisation, viz.: "The energy levels associated with the valence electrons of molecules agree in all essential aspects with those associated with the valence electrons of atoms." As a matter of interest, the writer calculated the mean effective areas of the outer electron orbits which would account for the molecular susceptibilities of CO and N<sub>2</sub>, using the method and some of the data given by Cabrera (*Journ. de Phys.*, Aug. 1925). It was found in both cases that the calculated areas were of very reasonable dimensions.

Incidentally, Glaser (*Ann. d. Phys.*, vol. 78, p. 641, 1925) has published a further account of his work on gases in which he shows that, as the pressure of the gas is reduced, a deviation in the proportionality of the susceptibility to pressure also occurs in CO, commencing at a pressure of 425 mm. Hg with a magnetic field of about 5,100 gauss. Further, he finds that O<sub>2</sub> shows no such deviation throughout an investigation with pressures ranging from 0 to 156 atmospheres. Glaser shows that his results for H<sub>2</sub>, N<sub>2</sub>, CO and CO<sub>2</sub> may be represented by a formula

$$p = C \cdot \frac{Z \sqrt{H}}{\sqrt{\theta}}, \text{ where } p \text{ is the pressure at which the de-}$$

viation commences in a magnetic field of strength H, Z is the number of electrons in the gas molecule,  $\theta$  its moment of inertia (taken from *Landholt u. Bornstein*, V. Aufl., Table 38, p. 123), and C is a constant. The meaning of this formula

is not clear, and we must wait for further experimental results before an explanation can be attempted.

Whilst we are dealing with magnetic phenomena, attention may be directed to a brief note by P. Weiss (*Compt. Rend.* vol. 182, p. 105, 1926) in which he points out that paramagnetic solutions of potassium bichromate show no temperature variation in susceptibility over the range  $14^{\circ}$  to  $50^{\circ}$  C. (P. Weiss and Mlle. Collet, *Compt. Rend.*, vol. 178, p. 2146, 1924), and that Mlle. Collet (*C.R.*, vol. 181, p. 1057, 1925) has shown that solutions of luteocobaltic chloride (a yellow cobaltamine) behave in a similar manner. Now, the conception that paramagnetism is due to the possession of a definite magnetic moment by the atom or molecule concerned leads us to expect a definite variation of the susceptibility with temperature. Weiss, therefore, suggests that a single electron orbit by itself possesses a magnetic moment, but that in the case of a substance with constant paramagnetism the resultant magnetic moments of all the electron orbits in the molecule is zero. In the case of potassium bichromate the molecule possesses two atoms of chromium, and we may suppose that, although each atom possesses a magnetic moment, the moments of the two atoms are oppositely directed so that they cancel one another. However, in luteocobaltic chloride the molecule only contains one atom of cobalt, and here, therefore, this kind of magnetic compensation cannot occur. Consequently Weiss comes to the conclusion that whilst paramagnetism arises from an orientation of some kind, only a portion of the atom is involved, and we must regard constant paramagnetism as an intra-atomic phenomenon. He considers that this conclusion is also valid for the solid state, as Mlle Collet has shown that the paramagnetism of potassium bichromate in solution and in the solid state is constant. It is difficult to understand what Weiss means by intra-atomic phenomenon, but the phenomenon of constant paramagnetism is worthy of careful attention.

*The Absorption of Slow Electrons in Gases.*—If we consider a beam of electrons passing through a gas, then electrons may be considered to be absorbed when they disappear from the original beam either owing to loss of energy or owing to changes in their direction of motion. If  $I_0$  is the initial number of electrons present in a beam passing through a known gas, and  $I$  is the number present in the beam after it has traversed a distance,  $x$ , in this gas at a pressure,  $p$ , then  $I = I_0 e^{-axp}$ , where  $a$  is the absorption coefficient for the electrons when the pressure of the gas is unity. In other words,  $a$  is the total effective absorbing area, expressed in sq. cm., of all the molecules contained in one cubic cm. of gas under a pressure of 1 mm. of Hg, when  $x$  is given in cm. and  $p$  in mm. of Hg.

Hence, the effective absorbing area of a single molecule may be found by dividing  $\alpha$  by  $3.65 \times 10^{15}$ , the number of molecules in 1 cubic cm. of gas under a pressure of 1 mm. of Hg. Consequently, by making determinations at two pressures,  $p_1$  and  $p_2$  respectively, we obtain two equations, viz. :

$$I_1 = I_0 e^{-\alpha x p_1} \quad \text{and} \quad I_2 = I_0 e^{-\alpha x p_2},$$

whence, 
$$\alpha = \frac{1}{x(p_1 - p_2)} \log \left( \frac{I_1}{I_2} \cdot \frac{I_0}{I_0} \right).$$

The variation of the above absorption coefficient with the velocity of the electron was first investigated by Lenard (*Ann. der Phys.*, vol. 12, p. 714, 1903), but Akesson appears to have been the first to observe that in methane the absorption coefficient was a maximum at an electron velocity corresponding to a potential drop of 8 volts, and that the slower electrons were much less absorbed. Akesson (*Lund's Acta*, N.S. 2, vol. 12, No. 11, 1916) also observed two maxima in the absorption curves for CO, CO<sub>2</sub>, C<sub>2</sub>H<sub>2</sub>, N<sub>2</sub>O, H<sub>2</sub> and O<sub>2</sub>. The absorption coefficients for slow electrons in gases were also extensively investigated by Ramsauer and by Maye (*cf.* Ramsauer's article in the *Jahr. der Radioakt. u. Elekt.*, vol. 19, p. 345, 1922). Ramsauer used a beam of photo-electrons emitted from a zinc plate, which was raised to suitable potentials and illuminated with ultra-violet light. Electrons of uniform velocity were obtained by arranging a number of slits on the circumference of a circle whose plane was perpendicular to a uniform magnetic field. By suitably adjusting the magnetic field, electrons of a known velocity were made to describe a circular path passing through the slits; the uniformity of the velocity, of course, depended on the size of the slits. The numbers of the electrons which passed along certain portions of the circle were determined, and thus all the quantities in the above equation were known. Ramsauer found that with electron velocities ranging from those corresponding to potential drops of 0.5 to 50 volts, the absorption coefficients for H<sub>2</sub>, N<sub>2</sub>, He, and Ne remained nearly constant, although they usually decreased a little as the velocity of the electrons was increased. Argon, however, was found to possess a maximum absorption coefficient at an electron velocity corresponding to 13 volts approximately, the absorption coefficient decreasing steadily to one-thirtieth of its maximum value for a velocity corresponding to 0.7 volt. Later experiments, made with Kr and Xe, verified the prediction, which Minkowski and Sponer (*Zeit. für Phys.*, vol. 15, 399, 1923) put forward on the strength of their experiments on the effects of the presence of the space charge about a hot filament, that these gases should exhibit the same phenomenon as argon;

in fact, very pronounced maxima of the absorption coefficients were found at 11.3 and 6.4 volts respectively. G. Hertz (*Proc. Amst. Acad.*, vol. 25, p. 80, 1922) verified the results in the case of argon, and further, the experiments of Townsend and Bailey (*Phil. Mag.*, vol. 44, p. 1033, 1922) showed that low velocity electrons possessed very small absorption coefficients in argon. Ramsauer's results with gases which exhibit this phenomenon showed that the effective absorbing area rose to a maximum of about four to five times the value calculated from kinetic theory data, and fell, with the slowest electrons investigated, to a value of the order of one-seventh that calculated from kinetic theory data. It is striking that with the gases Ne, A, Kr, and Xe the maximum became the more pronounced the higher the atomic number, whilst simultaneously the position of this maximum moved with increasing atomic number to a region of lower electron velocity.

More recently R. Brode (*Phys. Rev.*, vol. 25, p. 636, 1925) used a heated filament instead of a zinc plate as a source of electrons. He confirmed the results of Mayer and Ramsauer for A and He, maxima being found at electron velocities corresponding to 12 and 4 volts respectively. Methane gave a maximum at a velocity corresponding to about 7.5 volts. Carbon dioxide and nitrogen gave almost identical curves with maxima about 18 volts and minima about 9 volts. No maximum was observed with hydrogen, the absorption coefficient increasing steadily as the electron velocity decreased to that corresponding to 2 volts, whereas Ramsauer's results indicated that a maximum might exist in a region of velocity corresponding to 1 to 2 volts.

Brode (*Proc. Roy. Soc.*, vol. 109, p. 397, 1925) extended his measurements to the vapours of mercury, cadmium, and zinc, and he found no evidence of long free paths with low velocity electrons in these cases. This paper is worthy of attention because it describes a valve suitable for closing off apparatus *in vacuo* at high temperatures.

It is rather strange, in the study of the motion of electrons in gases, that the extensive work of Townsend and his collaborators seems to have been somewhat overlooked. Townsend and Bailey (*Phil. Mag.*, vol. 42, p. 873, 1921) described a method of finding electron velocities and the application of the results to the determination of some properties of gaseous molecules, and they carried out experiments in hydrogen, nitrogen, and oxygen with various ranges of pressure and electric force. They showed, in fact, that it was possible to calculate the loss of energy of an electron in colliding with a molecule and the mean free path of an electron, from an experimental determination of the velocity of the electron in the direction of the electric

force and its velocity of agitation. Later (*Phil. Mag.*, vol. 43, p. 593, 1922), they made similar experiments on argon. The results showed that the mean free path of an electron of velocity  $10^8$  cm. per sec. was about ten times as long in argon as in hydrogen or nitrogen at the same pressure. This result is remarkable, when we remember that the radii of the molecules of these gases, deduced from viscosity data, are not very different. Moreover, they found a large increase in the mean free path in argon as the velocity of the electron was reduced, and a similar effect was observed in nitrogen, oxygen, and hydrogen with low velocity electrons. Thus, in nitrogen at 1 mm. pressure, the mean free path increased from 0.266 mm. to 0.455 mm. (*i.e.* the absorption coefficient correspondingly decreased) when the electron velocity was reduced from  $8.85 \times 10^7$  to  $3.15 \times 10^7$  cm. per sec. (*i.e.* from about 2.2 to 0.3 volts). Ramsauer and Mayer found that the mean free path in argon was about fifteen times that in nitrogen or hydrogen, and they found, as we have seen, that the free paths in argon were much increased when the velocities of the electrons fell from  $6.3 \times 10^7$  to  $5.2 \times 10^7$  cm. per sec. However, Ramsauer concluded that there was no noticeable change in the free paths of electrons in other gases for similar changes of velocity. Townsend and Bailey also recorded the interesting fact that when an electron of velocity  $10^8$  cm. per sec. collided with an atom of argon it lost about one ten-thousandth part of its energy, but on colliding with a molecule of the other gases it lost more than 1 per cent. of its energy. A description of Townsend's experimental arrangements will be found in the *Phil. Mag.*, vol. 44, p. 1033, 1922. H. L. Brose (*Phil. Mag.*, vol. 50, p. 543, 1925) has also investigated the motion of low velocity electrons in oxygen, using the apparatus of Townsend and Bailey modified in certain particulars. He found that, within a comparatively short range of electron velocities, there existed a maximum and minimum value of the mean free path of the electrons. Actually, the minimum free path or maximum absorption coefficient was obtained with electron velocities corresponding to 2.7 volts ( $10^8$  cm. per sec.), and the minimum absorption coefficient was found at 0.48 volt ( $4.1 \times 10^7$  per sec.). Brose also confirmed Skinker's results (*Phil. Mag.*, vol. 44, p. 924, 1922) with carbon dioxide, in which a minimum absorption coefficient was found in a velocity region corresponding to about 1.8 volts. The curves given in Brose's paper include the results of Townsend and Bailey for hydrogen and nitrogen, which show absorption maxima at approximately 1.4 and 2.2 volts for these gases respectively.

M. Rusch (*Phys. Zeit.*, November 1, 1925) has also determined absorption coefficients down to electron velocities

corresponding to 0.2 to 2.0 volts. The pressures of the gases investigated were so low that the mean free path of the electrons was greater than the distance,  $x$ , to be traversed, so that there was no marked electron diffusion, as only every second or third electron suffered a collision with a gas atom in its path. A zinc sphere of radius 1 mm., when illuminated with monochromatic light, served as a source of electrons. Concentric with it was mounted a spherical shell of brass, 10 mm. thick and of mean radius 33 mm. The latter was perforated radially with about 400 holes, 2 mm. in diameter; 2 mm. from its outer surface was mounted a hemispherical shell of brass which served as a collecting electrode. The electrons, leaving the zinc sphere, proceeded radially from it owing to its smallness. Their velocity distribution curves could be obtained with the gas in the apparatus under two different pressures, *i.e.* the numbers of electrons of a given velocity reaching the receiving electrode under the two pressures could be found directly, and the absorption coefficient corresponding to this velocity could be calculated as before. These experiments showed that argon and krypton possess minimum absorption coefficients at velocities corresponding to 0.7 and 1.1 volts respectively. Neon showed a steady decrease in the value of its coefficient down to a velocity corresponding to some tenths of one volt. Hydrogen (*cf.* Townsend's results) showed a maximum corresponding to about 1.6 volt, below which the coefficient appeared steadily to decrease.

It cannot be said that we have any really satisfactory explanation of the weird manner in which slow electrons seem to wander through atoms. A theoretical treatment of the question by Hund (*Zeit. für. Phys.*, vol. 13, p. 241, 1923) is, however, of interest in that it accounts for the phenomena in the case of rare gases, although it breaks down in the case of gases like nitrogen and carbon dioxide. He assumes that in a collision of an electron with an atom there is a probability that the electron may leave its rectilinear path and pass to another orbit of lesser energy. The new orbit is also a straight line, and the difference in energy in the two orbits appears as radiation of frequency,  $\nu$ , in accordance with the usual quantum conception. The maximum value which  $\nu$  may attain is given by the equation,  $h \nu_{\max.} = \frac{1}{2} m v^2$ , where  $\frac{1}{2} m v^2$  is the kinetic energy of the electron. The probability of the transition of the electron to another orbit is limited in such a way that for no frequency can there arise a greater intensity of radiation than that required by the classical theory. For slow electrons, with these assumptions, an approximate calculation shows that the probability of transition with decreasing electron velocity rapidly becomes small compared with unity. A large fraction of the slow

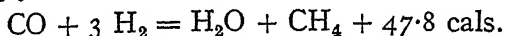
electrons will, therefore, not radiate during collision. Then, in order to explain the large values of the free path with slow electrons, it is necessary to postulate that all electrons which suffer no such transitions are not influenced by collision, and, therefore, continue their paths undisturbed. It is interesting that this treatment predicts the existence of the absorption minima observed experimentally.

Zwicky (*Phys. Zeit.*, vol. 24, p. 183, 1923) showed, however, that it was possible to give an explanation of the phenomena in the case of the rare gases with the aid of the classical theory alone, if certain atomic models were considered. In fact, starting with the motion of an electron round an atomic nucleus, he showed, on elementary considerations, that an electron might describe a spiral path around a highly symmetrical atom without passing through it. It is clear, however, that much more detailed experimental evidence with many gases is needed before an adequate theory can be devised.

Closely allied to the above phenomena must be the effects recorded by Dempster and G. P. Thomson. Dempster (*Nature*, Dec. 19, 1925, and *Phys. Rev.*, Jan. 1926) found that positively charged hydrogen atoms, or protons, with velocities acquired by falling through 300 to 900 volts, possessed an unexpected range in helium and other gases. In fact, in some instances the protons must have passed unchanged through more than a hundred helium atoms. On the other hand, singly charged helium atoms did not behave in this way, neither did charged hydrogen molecules. Thomson (*Nature*, Feb. 13) found that in the course of experiments on the scattering of hydrogen positive rays by helium and other gases, the protons were scattered to a maximum extent in helium when their energy corresponded to a fall through 10,000 volts. The slowest rays investigated possessed an energy corresponding to 3,500 volts, and were scattered to the extent of approximately 75 per cent. of the maximum. The velocities of the protons used by Thomson were, therefore, of the same order as the velocities of the electrons used by Ramsauer. Thomson found that protons passing through hydrogen did not exhibit any abnormality; the scattering increased with decreasing energy of the rays, although less rapidly than forces obeying an inverse square law would require. However, Rusch's and Townsend's results with very slow electrons in hydrogen would indicate that such an effect might be observed with protons with velocities a little less than those used by Thomson, and it is to be hoped that his measurements will be extended to protons with velocities corresponding to about 2,500 volts.

**ORGANIC CHEMISTRY.** By J. N. E. DAY, B.Sc., A.I.C., University College, London.

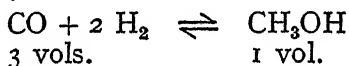
*Methanol.*—An interesting article on the "Industrial Production of Synthetic Methanol" has been published by C. Lormand (*Industrial and Engineering Chemistry*, 1925, **17**, 430). A summary of work on this problem is given, together with a bibliography and list of patents. Sabatier and Senderens, who tried to prepare methanol by passing carbon monoxide and hydrogen over reduced nickel, actually obtained water and methane:



Patart, in his research on the preparation of methanol in accordance with the equation



based his work on the fact that in the production of methanol from carbon monoxide and hydrogen, there is a decrease in volume. Therefore increase of pressure should favour the production of methanol, *i.e.* the reaction which produces a decrease in volume:



The conditions disclosed by Patart's patent are a temperature range of 300°–600° C., and a pressure range of 150–200 atmospheres. The catalysts included are metals, their oxides and salts. Actually a mixture of catalysts has been found best. A gaseous mixture of two volumes of hydrogen to one of carbon monoxide is used.

The Badische have developed this synthesis, and it is stated in the article that the Badische plant at Merseburg was, at the end of 1923, producing from 10 to 20 tons of methanol per day.

It is stated in the *Journal of Chemical Education*, 1925, **2**, 429, that synthetic German methanol is now being imported into America, and that the cost, including import duty, is below that of the methanol which is obtained as one of the products of the distillation of wood. Thus the position of the American wood chemical industry is very seriously affected.

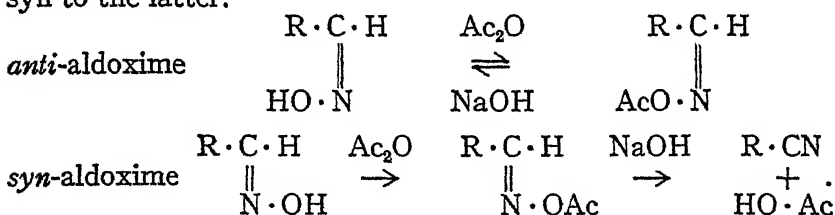
This would appear to be another example of the production, as the result of a long series of researches, of a synthetic article, more uniform in quality, and cheaper in price, than the article prepared from natural sources.

*Ether.*—In view of the important use of ether as an anæsthetic, reference may be made to two papers by Rowe and Phelps in the *Journal of the American Chemical Society* (1924, **46**, 2087, and 1926, **48**, 1049.) These papers deal with the analysis of

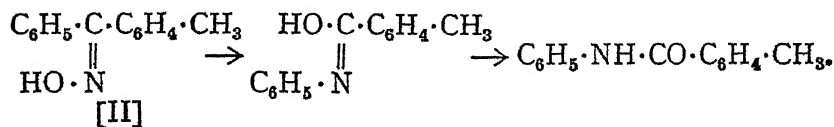
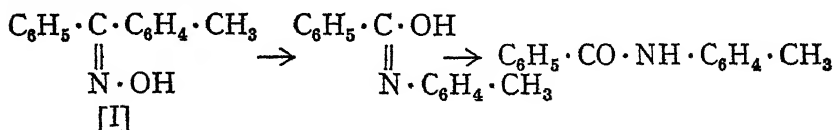
anæsthetic ether. The chief impurities are peroxide and aldehyde, which are probably produced by oxidation of the pure material during storage. In the first paper a detailed method is given for the estimation of peroxide, by reacting with cadmium and potassium iodides in dilute sulphuric acid in the presence of pure alcohol. The question of the time factor is also discussed. The second paper gives the conditions for the estimation of aldehyde. Schiff's reagent, prepared by the method of François, is used. As the colour produced is not proportional to the aldehyde concentration, details are given for the estimation by comparison with standard solutions of aldehyde in ether. (See also Rowe, *Industrial and Engineering Chemistry*, 1924, **16**, 896.)

*Oximes*.—Recent work on the configuration of the oximes suggests that the usual structures given to the *syn* and *anti* forms should be reversed.

The structure of the aldoximes depends on the method of determination suggested by Hantzsch. Acetic anhydride, below 30°, converts both isomerides into acetyl derivatives. When these derivatives are treated with dilute sodium hydroxide, one is hydrolysed, giving back the oxime, while the other, by elimination of acetic acid, gives the nitrile. On the assumption that *cis* elimination takes place, Hantzsch gave the name *syn* to the latter.

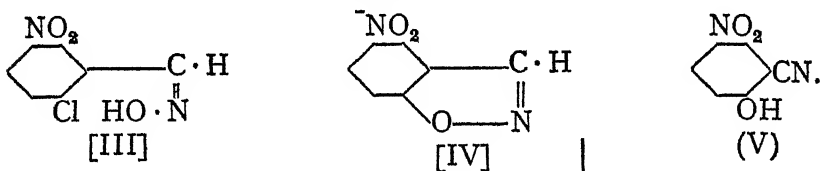


In the case of the ketoximes, the configurations assigned to the isomerides depend on the products obtained after subjecting them to the Beckmann rearrangement. Thus in the case of phenyl-*p*-tolyl-ketoxime, the configuration [I] is assigned to the oxime giving benzoyl-*p*-toluide, and [II] to that giving *p*-toluoyl-anilide.

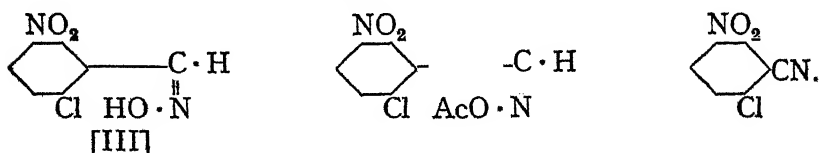


Thus the configuration of both aldoximes and ketoximes depends on the assumption of *cis* elimination and *cis* interchange of radicals.

That this assumption is not correct in the case of the aldoximes, is suggested by the work of Brady and Bishop (*J.C.S.*, 1925, **127**, 1357). These authors have now prepared the two forms of 2-chloro-5-nitro-benzaldoxime. They found that the *anti* isomeride (that is, the one which gives an acetyl derivative from which the oxime can be regenerated) did not give any ionisable chlorine when heated at 50° for four hours with 0.5 N. sodium hydroxide. On the other hand, the *syn* isomeride (that is, the one of which the acetyl derivative yields the nitrile) gave with 0.25 N. sodium hydroxide 27 per cent. of its chlorine in an ionisable form after one hour, 45 per cent. after two hours, and 53 per cent. after four hours. While the *anti* isomeride was almost unchanged by boiling for 30 minutes with N. sodium hydroxide, the *syn* isomeride was, by this treatment, converted into 5-nitro-salicylo-nitrile (together with some 5-nitro salicylic acid). The formation of the 5-nitro-salicylo-nitrile [V] from the oxime [III] takes place through the intermediate formation of the benzo-*iso*-oxazole [IV], which undergoes tautomeric change to the hydroxy nitrile. (Compare Bone, *J.C.S.*, 1893, **63**, 1346.)



In this case it is assumed that ring formation will take place more readily (leading to the formation of the *iso*-oxazole) in the case where the hydroxyl group is attached to the nitrogen on the side next the halogen. Since it is this isomeride [III] which gives the nitrile with acetic anhydride, it is evident that, in the reaction which Hantzsch used to orient the oximes, *trans* elimination and not *cis* elimination takes place. That is to say, the ordinary structures given to the *syn* and *anti* aldoximes should be reversed.

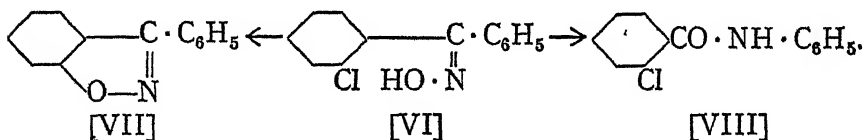


Reference may also be made to a paper by Beckmann,

Liesche, and Correns (*Ber.*, 1923, **56**, (B), 341) which supports this view on theoretical grounds, and to another paper by Auwers and Ottens (*Ber.*, 1924, **57** (B), 446) in which a comparison is made of the two forms. In the case of benzaldoxime, on the assumption that the *cis* form will have a smaller molecular refractivity than the *trans*, it is shown that the  $\alpha$ -oxime has the *syn* configuration. This is contrary to the generally held view, but is in agreement with the above work of Brady and Bishop.

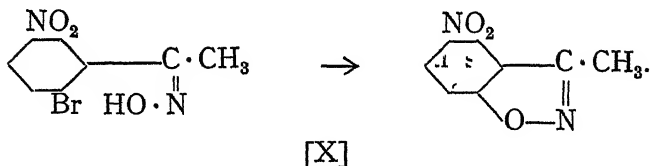
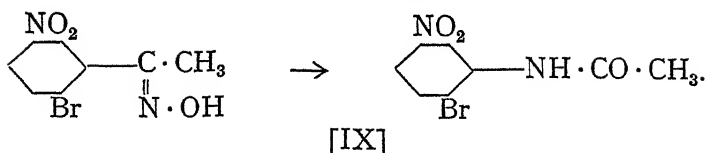
The structure of the ketoximes has already been discussed (this JOURNAL, 1924, **19**, 189) in connection with the work of Meisenheimer on the Beckmann rearrangement in the case of the oximes of benzil.

Further evidence in support of *trans* interchange is given by Meisenheimer and Meis (*Ber.*, 1924, **57** (B), 289) who found that *o*-chloro-(or *o*-bromo-) benzophenone oxime [VI] when treated with sodium hydroxide, lost hydrochloric (or hydrobromic) acid and gave the *iso*-oxazole [VII]. When made to undergo the Beckmann rearrangement, these oximes gave *o*-chloro-(or *o*-bromo-) benzanilide [VIII], no halogen substituted anilide being obtained.



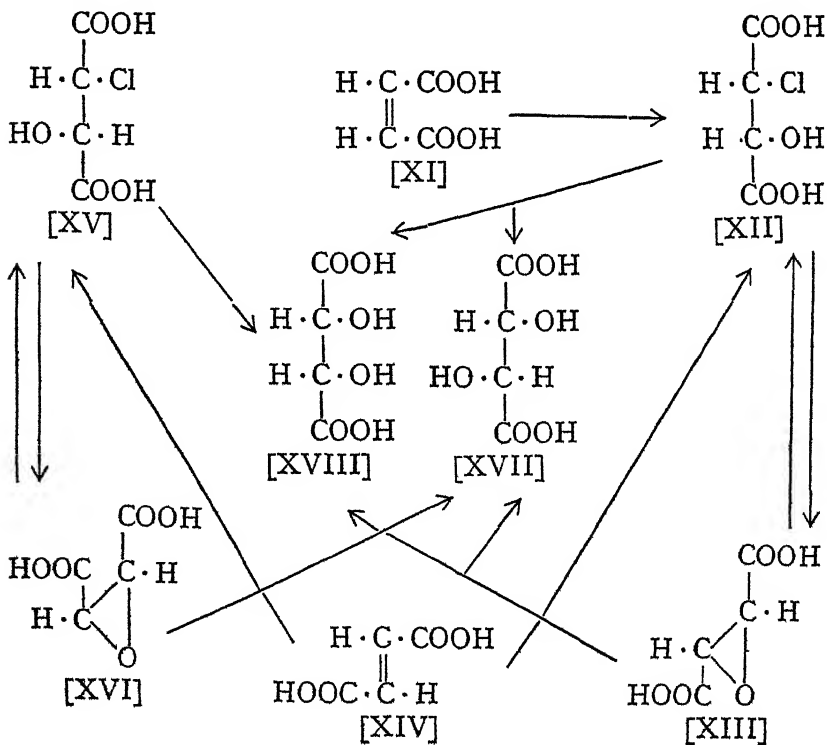
In continuation of this work, Meisenheimer, Zimmermann and Kummer (*Ann.*, 1926, **446**, 205) describe experiments with 5-nitro-2-chloro-(and 2-bromo-) benzophenone oximes. With dilute sodium hydroxide these oximes give 2-phenyl-4-nitrobenz-*iso*-oxazole; the presence of the nitro group para to the halogen greatly facilitates this reaction. The product of the Beckmann rearrangement is 2-chloro-(or 2-bromo-) 5-nitro-benzanilide. As these oximes undergo ring closure with great ease, it is most probable that the halogen and hydroxyl groups are vicinal, and therefore *trans* interchange must take place in the Beckmann change.

In the case of the benzophenone oximes only one form was obtained. When, however, the phenyl group was replaced by methyl, by using 5-nitro-2-bromo-acetophenone, two forms were obtained. The more stable  $\alpha$ -oxime [IX] gave with the Beckmann change acet-2-bromo-5-nitro-anilide, and was but little affected by sodium hydroxide. The less stable  $\beta$ -oxime [X] gave, with cold sodium hydroxide, 2-methyl-4-nitrobenz-*iso*-oxazole.



As, under these conditions, ring closure must be accepted as a better criterion of structure than interchange of radicals, it is now necessary, in the case of the Beckmann rearrangement, to consider the probability of *trans* and not *cis* interchange of radicals, and, in the case of the oximes, to reconsider the structure of the *syn* and *anti* forms, with a view to the reversal of the generally accepted formulæ.

In connection with the question of *trans* cleavage of rings, a paper has been published by Kuhn and Ebel (*Ber.*, 1925,



58 (B), 919) giving the results of the reaction of hypochlorous acid on maleic acid and fumaric acid. Maleic acid [XI] with hypochlorous acid gives chloro-malic acid [XII]; this in alkaline solution loses hydrochloric acid and gives *trans*-oxidoethylene- $\alpha\beta$ -dicarboxylic acid [XIII] which is resolvable. Fumaric acid [XIV] with hypochlorous acid gives [XII] and in addition a second chloro-malic acid [XV]. With alkali [XV] loses hydrochloric acid and gives *cis*-oxidoethylene- $\alpha\beta$ -dicarboxylic acid [XVI] which is not resolvable. With hydrochloric acid [XIII] gives [XII], and [XVI] gives [XV]. The interesting part of this work is the action of water on these compounds. It has been found that while [XII] gives a mixture of *dl* tartaric acid [XVII] and *meso* tartaric acid [XVIII]—

[See formula on p. 29.]

on the other hand [XV] gives [XVIII] only. The *cis* acid [XVI] gives [XVII]. The *trans* acid [XIII] gives a mixture of 37 per cent. *dl* tartaric acid and 63 per cent. *meso* tartaric acid. *Cis*-oxidoethylene- $\alpha\beta$ -dicarboxylic acid should, on stereochemical grounds, give *meso* tartaric acid. The production of only the *dl* tartaric acid is, therefore, advanced by these authors as proof that *trans* cleavage of the ring has taken place.

**BIOCHEMISTRY.** By R. KEITH CANNAN, M.Sc., University College, London.

*The Rôle of Iron in Tissue Respiration.*—In the previous review were outlined some present tendencies of thought in the field of biological oxidation-reduction. We would now return to one special phase of this problem which has captured a new interest within the last few years.

The constant association of iron with the oxidases has long been recognised and has recently been emphasised by Willstatter (*Annalen*, 1923, **430**, 269). Moreover, the significance of iron as well as the peroxide theory of biological oxidation received powerful support in the elaborate demonstration by Dakin that Fenton's reagent, hydrogen peroxide and a trace of ferrous salt, was unique amongst oxidising agents in its close imitation of the more obvious biological oxidations. More recently, Warburg has crystallised with some definiteness our conception of this biological function of iron. It is unnecessary to discuss in detail the conclusions of Warburg, as these have been the frequent subject of review elsewhere (*Chemical Dynamics of Life Phenomena*, by O. Meyerhof, 1923); but his main contentions are so pertinent to our argument as to bear repetition. Emphasis is laid on two predominant phenomena in tissue respiration. One of these is the significance of the structural integrity of the living cell, and the other the association of iron

with these specialised respiring structures. The activation of oxygen is to be regarded as essentially a surface catalysis wherein iron-containing areas are the particular seats of activity. These views find cogent demonstration in the charcoal model which Warburg has studied to such good purpose, and one of the most intriguing suggestions of these studies is that it is not the occurrence of iron *per se* in the charcoal surface which determines catalytic activity. The chemical state of the iron is important. The substance from which the most active preparations of charcoal were obtained was hæmatin—the iron-pyrrole compound which forms the non-protein portion of the hæmoglobin molecule. It would seem, therefore, that the atomic skeleton of hæmatin which persists in the charcoal must contain the iron in a form particularly favourable to its surface activity. Before, however, turning logically to the chemistry of hæmoglobin and its derivatives for elucidation of this peculiarity, we shall consider briefly some studies of Baudisch which have, as yet, not received the attention which they merit in discussions of the biological rôle of iron. To the writer the work of Baudisch offers a “model” which is, in many ways, more striking than is the charcoal model of Warburg.

It is well known that ferrous hydroxide and bicarbonate suffer rapid autoxidation in air. Baudisch (*Journ. Biol. Chem.*, 1924, **61**, 261) has shown that if these substances be precipitated in the presence of certain organic compounds (lactic acid, pyrimidines, etc.) as well as oxygen, then it is the organic compound and not the iron which becomes oxidised. The case is patently one of activation of oxygen. But more startling is the observation that ferrous hydroxide will *reduce* nitrates in the presence but *not* in the absence of oxygen. We must either accept the bizarre conclusion that active oxygen exhibits reducing properties towards nitrate or be content, merely, to consider the oxygen-iron complex as a system intrinsically different from oxygen. Whatever the conclusion, a further significant fact derives from this work, that the catalytic activity of the iron compound falls off rapidly after precipitation (Baudisch, *Journ. Biol. Chem.*, 1925, **64**, 753). This ageing process can be reasonably attributed to the change from the amorphous to the crystalline state, a change which Baudisch goes some way towards analysing in a study of the catalytic activity of the oxides of iron (*Chem. Zeit.*, 1925, **49**, 661). He finds that synthetic magnetite,  $\text{Fe}_3\text{O}_4$ , is catalytically active, whereas hæmatite,  $\text{Fe}_2\text{O}_3$ , is not. But the activity does not depend merely upon the state of oxidation of the iron, for magnetite may be oxidised by controlled heating in air until it is converted into something analytically indistinguishable from hæmatite. Nevertheless, it retains the activity of the

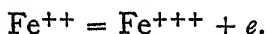
lower oxide. It retains, likewise, the magnetic properties, crystal structure and "peroxidase reactions" characteristic of magnetite. Further heat in the absence of air effects no further chemical change, but all those characteristics of the lower oxide which it retained after oxidation have now disappeared and the product is identical in all respects with hæmatite. To what conclusion can we come other than that in magnetite, oxidised magnetite and freshly precipitated ferrous hydroxide or bicarbonate there occurs a peculiar atomic configuration which is the seat of magnetic and catalytic properties, and which cannot be defined merely in stoichiometric terms? For the appreciation of the intimate nature of this we must await a better understanding of the special electronic systems which determine magnetic properties. Baudisch summarises his conclusions in a co-ordination formula for the iron compound wherein molecular oxygen may become co-ordinated with the iron atom. If an oxidisable substance can become attached to this molecule of oxygen, then it is held that oxidation of it may result and the oxygen again be removed from the iron. The significance of this lies in the fact, as we shall see, that others, for other reasons, have been led to similar hypotheses in the case of hæmoglobin and other catalytically active compounds of iron.

*The Chemistry of Hæmoglobin.*—With the specialised function of the respiratory pigments we shall not concern ourselves. Hæmoglobin would find no place in a discussion of the catalytic properties of iron were it not that there is increasing evidence that the special function of "oxygen-carrier"—i.e. the transporting of molecular oxygen about the labyrinth of the highly differentiated organism—is one which has been superimposed upon a more fundamental catalytic function of iron in the form in which we find it in hæmoglobin and its derivatives. The reader is reminded that hæmoglobin and hæmatin give the usual reactions of a peroxidase. Miss Robinson (*Biochem. Journ.*, 1925, **8**, 255) has shown recently that they will catalyse the autoxidation of unsaturated fatty acids, and the numerous papers of Neill and his associates provide further data of interest (*J. M. Neill et al.*, *Journ. Exp. Med.*, 1924, **39**, 757; *Journ. Biol. Chem.*, 1925, **63**, 479; *Journ. Exp. Med.*, 1925, **41**, 551). Moreover, the many similarities between hæmoglobin and the amino-ferrocyanides of Manchot and, in particular, the sodium pentacyanoaquoferroate (see Baudisch, *Journ. Amer. Chem. Soc.*, 1923, **45**, 2972) which forms a dissociable compound with oxygen, are of great significance.

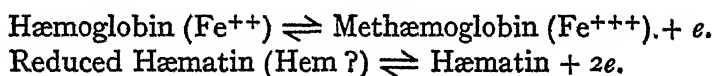
Turning, then, to the chemistry of hæmoglobin we find in the work of Anson and Mirsky (*Journ. Physiol.*, 1925, **60**, 50, 161) some radical modifications of conventional views. They

consider that whereas hæmoglobins of different origin are chemically distinct they arise by combination of globin with a common iron-containing substance to which they give the name Hem. This substance is, itself, capable of existing in two reversible states of oxidation. Hem will give with a wide variety of nitrogenous substances products which have spectra allied to that usually attributed to "hæmochromogen." Such substances are ammonia, glycine pyridine, nicotine, albumin and globin. But the globin compound is unique amongst these in possessing high solubility and the fact that, by adjustment of the hydrogen-ion concentration, the hæmochromogen spectrum passes into that of hæmoglobin. At the same time the solution assumes the power of forming a dissociable compound with oxygen. The function of oxygen-carrier would, therefore, appear to be the province of this particular association of hem and globin and the catalytic activity the property of the atomic structure found in hem. Additional significance is brought to this analysis of the blood pigments by the studies of Keilin (*Proc. Roy. Soc.*, 1925, 98B, 312), who has found very widely distributed in nature a pigment—to which he gives the name Cytochrome—which appears to be a mixture of two or three hæmochromogens. It is found, for example, in yeast, bacteria, some higher plants and the tissues of a variety of lower and higher animals. It appears to be identical with the myohæmatin described by MacMunn in 1887. Cytochrome is easily oxidised by air and reduced by the cell, where it exists chiefly in the oxidised state. Cyanide inhibits the oxidation but not the reduction of the pigment, whereas narcotics (*cf.* Warburg) have the reverse effect. Cytochrome gives a compound with carbon monoxide as well as oxygen. Since its concentration in tissues runs parallel with the intensity of the peroxidase reaction of the tissue and since its distribution is amongst those cells and tissues suspected of the greater activity, it is reasonable to conclude with Keilin that cytochrome has a definite catalytic function. The work on cytochrome has been purely spectroscopic, so that it must be appreciated that the chemistry of the substance has been inferred from changes in the spectra, and some points of difficulty remain open. If the oxygen compound is of the type of hæmoglobin it would not be expected to have catalytic properties, as the latter yields up only molecular oxygen. The compound may, however, be of the type of Baudisch's iron-oxygen systems, or it may be essentially an electrochemical system not involving oxygen specifically. In favour of the latter suggestion is the work of Conant (this JOURNAL, 1925, June), which has already been reviewed in these columns. It is the conclusion of this work that the reversible oxidation of hæmoglobin to methæmoglobin

is an electrochemical system which is expressed essentially by the reversible change :

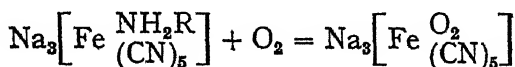


The reversible oxidations of the derivatives of hæmoglobin might be expected to be reactions of the same type, so that a facile explanation would be to hand of the catalytic properties of the iron of hæmoglobin and its derivatives. The system could then be regarded as an oxygen or hydrogen acceptor in precisely the sense in which methylene blue finds frequent experimental employment. This possibility is prejudiced not only, as we have seen, by the conclusion that the peculiar activities of iron cannot be explained merely by the intervention of the reversible equilibrium of the ferrous and ferric states, but also by the unsatisfactory character of the attempt by Conant to extend his electrochemical studies to hæmatin. In so far as these were open to interpretation they suggested that the reduction of this compound or the oxidation of the reductant to hæmatin was a reversible reaction involving *two* equivalents of hydrogen. This would make the system radically different from that of methæmoglobin and would suggest that in hæmatin the seat of oxidation-reduction was not the iron atom at all. The contrasting reactions may be represented thus :

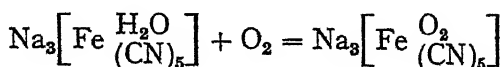


Such a contrast is difficult to accept, and a repetition of the work is earnestly to be desired.

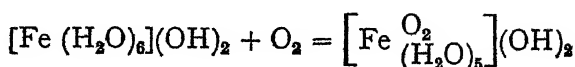
We see, then, that a number of workers approaching the problem by different routes have been led to propose co-ordination formulæ for a variety of catalytically active compounds of iron. A comparison of these structures is of interest.



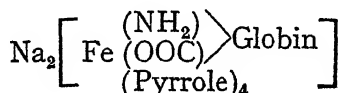
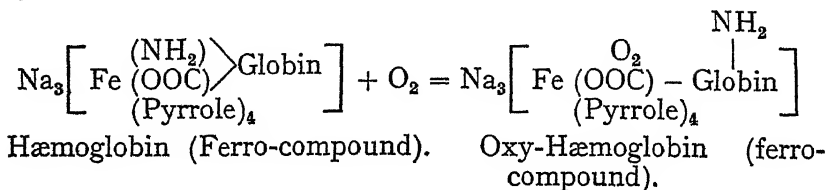
Manchot's pentacyanoaminoferroate and its dissociable compound with molecular oxygen :



pentacyanoaquaferroate (Baudisch).



Baudisch's scheme for the activation of oxygen by ferrous hydroxide.



Methæmo globin (ferri-compound).

It is to be observed that much yet remains to be done before these structures can be accepted as a satisfactory explanation of the peculiar activities of iron. Particularly is this so in respect of the biological functions. At present we have to attribute indiscriminately to these co-ordination systems the power to form with oxygen dissociable compounds which in some cases provide us with molecular oxygen and in others with what we can, at best, describe as "active" oxygen. Nevertheless, the problem has now been narrowly defined by these very defects and a better understanding should, therefore, not be long delayed.

**PHYSICAL CHEMISTRY.** By R. K. SCHOFIELD, M.A., Trinity College, Cambridge.

*Homogeneous Unimolecular Reactions.*—A year ago it was reported in SCIENCE PROGRESS (1925, 75, 382) that the work of Hinshelwood indicated that the decomposition of phosphine is a surface reaction, and thus cast doubt on the existence of "true" homogeneous unimolecular reactions. During the last year four independent investigations on the thermal decomposition of nitrogen pentoxide have appeared (Hunt and Daniel, *J.A.C.S.*, 1925, 47, 1602, White and Tolman, *ibid.*, 1925, 47, 1240, Hirst, *J.C.S.*, 1925, 127, 657, Hirst and Rideal, *Roy. Soc. Proc.*, A, 1925, 109, 526). They all agree in finding no wall-effect or autocatalysis, and no variation in the rate either with pressure over a large range down to less than 1 mm. or in the presence of excess argon, air, or nitrogen peroxide. Hirst and Rideal found, however, that the rate increases if the pressure falls below 0.25 mm. and approaches a limiting value five times the normal at very low pressures (0.01 mm.), when

it is in good agreement with the value calculated from the radiation theory by the Dushman-Rideal equation

$$-dC/dT = \nu e^{-N h \nu / RT} \cdot C.$$

Although the reason for this variation is not yet clear, there seems no reason to doubt that this reaction is truly homogeneous.

The thermal decomposition of sulphuryl chloride (Smith, *J.A.C.S.*, 1925, **47**, 1862) is also practically independent of wall effect. Thus, although many gaseous decompositions are complicated by wall and other effects, we now have two definitely established cases in which decomposition takes place in the bulk and follows the unimolecular equation

$$-dC/dT = K e^{-B/RT} \cdot C.$$

The difficulty of drawing from this fact any definite conclusion about the *mechanism* of these reactions is emphasised in a theoretical paper by Tolman (*J.A.C.S.*, 1925, **47**, 2652), in which it is shown that the above equation can be deduced from Arrhenius's theory—that activation of the reactant molecules precedes their decomposition—without any specific assumption being made either as to the rate or the mechanism of activation. The only assumptions made are :

(1) That the reactant molecules can exist in an activated state or states.

(2) That a certain fraction of the molecules which arrive in any activated state may decompose and the remainder fall back to the unactivated state.

(3) That activated and unactivated molecules are in statistical equilibrium, which, interpreted in the light of the *principle of microscopic reversibility*, means that as many molecules attain a given state by a particular path from another state as pass in the same time in the reverse direction (except in so far as this equilibrium is upset by the progress of the reaction).

Thus the establishment of the existence of homogeneous unimolecular reactions, taken in conjunction with Tolman's work, lends support to Arrhenius's theory that activation precedes reaction. It cannot be said, however, that we are appreciably nearer to discovering the exact mechanism by which activation is brought about.

*The Combustion of Well-dried Carbon Monoxide-oxygen Mixtures.*—Considerable interest attaches to the recent re-searches of Prof. W. A. Bone and his collaborators, F. R. Weston, R. P. Fraser, and D. M. Newitt (*Roy. Soc. Proc.*, A, 1926, **110**, 615 and 634), which show beyond doubt that steam is not *essential* to the combustion of carbon monoxide. It is nearly fifty years since Prof. Dixon announced the failure of his

attempts to ignite mixtures of carbon monoxide and oxygen which had been dried by long contact with phosphorous pentoxide, and the researches of Baker have led to the opinion that the presence of a trace of moisture is necessary if combustion is to proceed. The presence of moisture undoubtedly makes it *easier* to ignite these mixtures, but these new researches would suggest that previous failures have been due to the use of too small an ignition spark. Thus, whereas the "minimum spark energy" necessary to ignite a  $2\text{CO} + \text{O}_2$  mixture at atmospheric pressure saturated with moisture at  $15^\circ \text{C}$ . is only 0.005 joule, 0.126 joule is required if the gases have been dried with calcium chloride, and when "dry" in the Bakerian sense a still more powerful spark is needed. The authors find that an increase of initial pressure tends to make combustion more complete. They consider that the direct CO-oxygen reaction is the cause of the characteristic colour of the CO flame, and are inclined to think that ionisation of one or both the reactants is a necessary precedent to their combination.

*The Kinetic Theory of Surface Films.*—Langmuir (*J.A.C.S.*, 1917, **39**, 1858) attributed the lowering which a unimolecular film produces in the tension of a water surface to an outward force exerted by the film. On this view the observed surface tension,  $\sigma$ , is the resultant of two forces: (1) the *unaltered* tension,  $\sigma_0$ , of the water (or other solvent) surface and (2) an outward force,  $F$ , exerted by the unimolecular film. Thus we write for the observed surface tension  $\sigma = \sigma_0 - F$ . By analogy with the behaviour of their insoluble homologous, he argued that the molecules of soluble organic substances adsorbed at the surface of aqueous solution are arranged in a single orientated layer, and also lower the surface tension by exerting an outward force equal to the difference between the observed surface tension and that of pure water. The area,  $A$ , occupied by a gr. mol. of surface excess (reciprocal of surface concentration) is equal (by Gibbs' adsorption equation) to  $-\frac{RT}{C} \frac{dc}{d\sigma}$  for solutions weak enough to be regarded as ideal, and Langmuir showed that the relation  $FA = RT$  analogous to the Boyle-Charles law for gases is approximated to when  $F$  is less than 5 dyne/cm.

If the molecules of a soluble fatty acid are adsorbed at the surface of a solution in a single layer, the data indicate that, for all but the smallest values of  $F$ ,  $A$  does not greatly exceed the least area a gr. mol. can occupy when close-packed. Hence a variation of  $FA/RT$  from unity as  $F$  increases is not necessarily inconsistent with the theory that  $F$  originates from the thermal agitation of the adsorbed molecules. R. K. Schofield and E. K. Rideal (*Roy. Soc. Proc.*, A, 1925, **109**, 57), on closely

examining the available data, find that the  $FA/RT - F$  curves for fatty acids at a water-benzene as well as a water-air interface strongly resemble the  $p\bar{v}/RT - p$  curves for highly compressed gases. For values of  $F$  above 10 dynes/cm. the equation  $F(A - B) = x RT$  is obeyed. This is exactly analogous to the relation found by Amagat to be obeyed by gases when under pressures exceeding 500 atmospheres. If, as the authors suggest, a surface pressure of 1 dyne per cm. is to be compared with a gaseous pressure of roughly 50 atmospheres, it is reasonable to expect an equation of this form rather than one analogous to Van der Waals's equation to be applicable over the range of  $F$  values most readily accessible to experiment (10 - 30 dynes/cm.).  $B$  is the same for all the fatty acids examined from  $C_4$  to  $C_{12}$  which supports Langmuir's orientation hypothesis and leads to a constant limiting molecular area 24 - 25 sq. ÅU.  $1/x$  increases with the length of the hydrocarbon chain, and is taken to indicate an increasing lateral cohesion in the film. At a water-benzene interface  $x = 1$  - no cohesion. The same is true when cane sugar is adsorbed at a water-mercury interface, giving the exact analogue of Sackur's osmotic pressure equation  $P(\bar{v} - b) = RT$ .  $B$ , which is in striking agreement with the dimensions of the molecule found in other ways, indicates that here its long axis is in the plane of the interface.

N. K. Adam and G. Jessop (*Roy. Soc. Proc., A*, 1926, **110**, 423), in some new and very beautiful experiments, show that films of insoluble substances, when examined at sufficiently low surface pressures, show phenomena completely analogous to the critical phenomena of three-dimensional fluids. Thus the idea of Devaux that surface films may exist in three states corresponding to the three states of three-dimensional matter is demonstrated. It should be noted, however, that, as contended by R. K. Schofield and E. K. Rideal (*Roy. Soc. Proc., A*, 1926, **110**, 167), the "expanded" film is the *liquid* state. Soluble films are "gaseous," and Adam and Jessop have now for the first time laid bare the true "vapour" film which never exists at surface pressures greater than 0.4 dynes/cm. This last fact supports the view of Schofield and Rideal that 1 dyne per cm. corresponds roughly to 50 atmospheres, and the form of Adam and Jessop's curves conforms closely to that to be expected on this view (Schofield and Rideal, *loc. cit.*). The limiting value of  $FA$  for "vapours" at very small  $F$  - values differs little, if at all, from  $RT$ , where  $R$  is the ordinary gas constant and not one many times smaller, as Marcelin contended (*Nature*, 1926, **117**, 484).

M. Volmer and P. Mahnert (*Z. Phys. Chem.*, 1925, **115**, 239) and S. C. Kar (*Physikal. Z.*, 1925, **26**, 615) have put forward

the relation  $F(A - B) = RT$ , but since  $x$  of Schofield and Rideal's equation is only unity in special cases this equation cannot be generally applicable. Langmuir, however (*Colloid Symposium Monograph*, vol. iii, 1925, p. 70 seq.), prefers to retain the equation in the above form, but argues that the true kinetic pressure is not obtained by subtracting the observed surface tension from that of *pure water*. In the case of an expanded film of a long-chain fatty acid on water he suggests that the combined tensions of the surface between the ends of the hydrocarbon chains and the air and that between the hydrocarbon chains and the water oppose the outward kinetic pressure of the carboxyl "heads" operative at the second interface. Thus he writes the equation of state in the form :

$$(F - F_0)(A - B) = RT$$

where

$$F_0 = \underbrace{\sigma}_{\text{water-air}} - \underbrace{\sigma}_{\text{hydrocarbon-air}} - \underbrace{\sigma}_{\text{hydrocarbon-water.}}$$

The idea is undoubtedly an interesting one, but at present no critical examination of the above equation has been published. The matter is certain to give rise to discussion in the near future. Whatever may be the final conclusion about what may be called the "background tension," there is very strong evidence for the existence of two-dimensional kinetic pressures.

**PLANT PHYSIOLOGY.** By R. C. KNIGHT, D.Sc., Imperial College of Science and Technology and East Malling Research Station (Plant Physiology Committee).

*Vegetative Propagation.*—The problems of vegetative propagation are of extreme practical importance from the point of view of the horticulturist, but they have also their academic aspect, and the well-known work of Loeb on *Bryophyllum* showed that physiology has much to gain from the investigation of those particular aspects of growth which are involved in regeneration processes. Recent work on the subject has included both points of view, and has aimed at the elucidation of fundamental principles as well as at the purely utilitarian object of obtaining methods of propagating desired varieties by vegetative means.

Priestley ("Problems of Vegetative Propagation," *Journ. Roy. Hort. Soc.*, 1926, **51**, 1-16) has summarised the problems involved in vegetative propagation and has contributed evidence towards the solution of these. Propagation from a root or a stem-cutting involves a process of restoration of the balance between root and shoot, and in this process appropriate new organs must be formed. The conditions necessary for the regeneration of new organs include the presence of

meristematic tissue (or capacity to develop it), and a suitable nutrient supply. In some plants, *e.g.* willows, the stems contain meristematic centres which remain dormant under normal conditions, but which will produce roots in a suitable environment. In other plants the vascular cambium may give rise to meristems under favourable conditions. Shoot-forming meristems arise superficially from a cork phellogen, and it is suggested that the supply of nutrients to the meristems is regulated by anatomical considerations. When a functional endodermis is present it prevents outward diffusion of sap and so conserves the materials for growth in the regions of the meristems, *i.e.* the root-initials. With regard to polarity, stress is laid on the importance of the distribution of the meristems, or potential meristems, and on the gradients of food supply, although the opinion is offered that the counter-currents of ascending shoot-forming materials and descending root-forming materials play but a small rôle in determining polarity.

Neilson Jones has investigated the polarity in regeneration from seakale roots (*Ann. Bot.* 1925, **29**, 359-72) and found that the well-marked polarity is only slightly influenced by gravity, and even so, only quantitatively. A piece of root, split longitudinally and inverted, still forms most shoot-buds at the end which is morphologically apical, but the bud-forming region extends along the cambium to a greater distance than in the case of a root orientated normally. Centrifugal force applied to pieces of root has an effect similar to that of gravity, but to a markedly higher degree. Very short lengths of root produce shoots from both ends, but roots from the distal end only. If shoot-production is associated with high metabolic activity this result indicates that the gradient of activity is not steep, and in a short length of root the difference in activity between the two ends is insufficient to produce the normal dominance of one end over the other. Some difficulty is obviously experienced here in the rather different phenomena exhibited by root-development from short parent pieces. Local increase of temperature, even to such a slight extent as 2° C., induced shoot-formation at the end which was at the higher temperature, but some attempts to reverse polarity by electrical and chemical methods were unsuccessful. Tissues bordering on the cambium appear to be the important factors in the exhibition of polarity, since severing the cambium region only at any point produces the same result as severing the whole organ. Severance of other tissues has not this effect. The opinion is expressed that stimulatory factors rather than transmission of formative or inhibitory substances determine the locality of regeneration, and some support is found for Child's idea of "metabolic gradients."

Chandler ("Polarity in the Formation of Scion Roots," *Proc. Amer. Soc. Hort. Sci.*, 1925, **22**, 218-22), in some grafting experiments, has found evidence of polarity in the regeneration of apple roots. Scions (stems) are commonly grafted upon pieces of root, approximately end to end, and the formation of new roots on the original root-piece appears to inhibit root-formation on the scion above. Chandler made grafts by attaching the root-piece at the side of the scion instead of at the end, thus leaving a stub of scion extending alongside and beyond the root-piece. In such cases root-formation appeared to be much more abundant on the part of the scion below the union than on the part above, and Chandler was satisfied that the difference was not due to external conditions.

The influence of internal nutritional conditions upon regeneration has received some consideration, particularly with reference to the carbohydrate-nitrogen relations of stem-cuttings. Reid ("Relation of Kind of Food Reserves to Regeneration in Tomato Plants," *Bot. Gaz.*, 1924, **77**, 103-10) found that tomato cuttings taken from plants which had been supplied with a soil rich in nitrogen, and which, therefore, had a high nitrogen content, produced new shoots in abundance if placed with the basal end in a nutrient solution, whether this solution contained nitrogen or not. Root-production from the high-nitrogen cuttings was relatively deficient, and was further inhibited by the presence of nitrogen in the nutrient solution. On the other hand, cuttings from plants rich in carbohydrates, and containing little nitrogen, produced many roots. It appears that in a plant which regenerates easily from stem-cuttings, the quantitative relations of the new organs are closely controlled by the quantitative relations of the internal nutrient supply. Reid extended her observations (*Bot. Gaz.*, 1924, **77**, 404-18), and indicates that cuttings in light make use of carbohydrate reserves and also synthesise higher nitrogen compounds more rapidly than in darkness.

In general, cuttings tend to distribute their new growth activity in the same manner as this was being distributed in the parent plant when the cuttings were taken. A plant producing abundant roots and few shoots gives cuttings which will also tend to have a low shoot-root ratio, and vice versa.

Starring ("Influence of the Carbohydrate-nitrogen Content of Cuttings upon the Production of Roots," *Proc. Amer. Soc. Hort. Sci.*, 1923, **20**, 288-92), working with tomato and *Tradescantia*, found that root-production was profoundly influenced by the internal nutritional conditions. The carbohydrate content of his plants was varied by varying the daily period of illumination, and the nitrogen content was regulated by the

adjustment of the nutrient solutions supplied. Thus he was able to obtain cuttings from a wide range of plants in which the carbohydrate-nitrogen ratio varied. Starring consistently found more root-production from the cuttings with a high carbohydrate-nitrogen ratio than from cuttings with a low ratio. In tomato, the length of root per cutting in the low carbohydrate-high nitrogen series averaged 0.05 cm., whilst in the high carbohydrate-low nitrogen series the average root length per cutting was 19.7 cm. The differences in *Tradescantia* were somewhat less, but still very considerable. As might be expected, the starved cuttings (low carbohydrate-low nitrogen) produced least roots of any series. Some indications were obtained with regard to the possibility of nutrient conditions affecting root initiation as distinct from root growth. In *Tradescantia* plants low in carbohydrates there was no starch at the nodes, whilst in the plants high in carbohydrates starch was abundant at the nodes and internodes alike. In spite of this, roots were developed only in the nodal regions, indicating the influence of some factor other than nutrition in the development of the meristematic "root initials."

Schrader ("Relation of Chemical Composition to the Regeneration of Roots and Tops on Tomato Cuttings," *Proc. Amer. Soc. Hort. Sci.*, 1924, **21**, 187-94) also found that high carbohydrate content favoured root-production, but the nitrogen content below a certain value decreased root-production in spite of the high carbohydrate. If both carbohydrates and nitrogen were high, abundant growth of both shoots and roots occurred. On the other hand, new shoot-production was most vigorous with a low carbohydrate-nitrogen ratio, although here again carbohydrates became limiting in extreme cases.

Smith (*Trans. and Proc. Bot. Soc. Edin.*, 1924, **29**, 17-26), working on the propagation of *Clematis* species, was able to deduce some conclusions concerning the relation of internal nutrition to root-production from stem-cuttings. Normally, *Clematis* cuttings will produce roots in internodal regions only, but if the plant is darkened for from 10 to 21 days before taking the cutting, roots are readily formed at the nodes. Etiolation in this manner results in the depletion of the starch stores in the stem. Further, it is stated that polysaccharide deposition on a cell wall is reversible whilst the protoplast is living. Such reversal is affected by etiolation. In this way rooting is aided by a softening of the tissues and consequent reduction of mechanical resistance to growth. Meristematic activity is dependent upon a definite carbohydrate-nitrogen balance, and under normal illumination conditions excess carbohydrate accumulates and tends to inhibit new growth.

Re-establishment of the balance, as for example by etiolation, may stimulate mature tissue to regeneration. Mere shading of cuttings detached from the plant does not bring about this result, because, whilst carbohydrate is being used up, no nitrogen is being supplied. There is no record of experiments in which the C/N balance is adjusted in detached cuttings by supplying nitrogen. No evidence was found to indicate that the starch sheath, which in *Clematis* is not considered to be equivalent to an endodermis, in any way influences adventitious root-production. Smith (*Nature*, 1926, **117**, 339-40) has also investigated the influence of the reaction of the external medium upon root-formation by *Coleus* cuttings. The cuttings were grown in tap water, and rooting was found to occur only between pH 4.0 and pH 9.2. The range pH 7.0 to 7.2 was found to be the most favourable. In water no callus formation took place, but rooting was not retarded on that account. A few days after taking the cuttings there was a concentration of the carbohydrate reserves at the base, and the cuttings with the greater amount of carbohydrates appeared to root best.

Van der Lek ("Over de Wortelvorming van Houtige Stekken," *Meded. v. d. Landbouwhoogeschool te Wageningen*, 1925, **28**, No. 1, 1-230) has recorded a series of observations on the influence of internal and external conditions upon the rooting of stem-cuttings. In general, in *Ribes nigrum*, the polarity of the cutting, as indicated by the distribution of the new roots, is weak, and is readily influenced by external conditions. The "root-germs" are concentrated near the nodes in this species, whilst in *Salix* and *Populus* species both nodal and internodal root meristems are found—in all cases in association with medullary rays. No root-germs were found in the stem of *Vitis vinifera*, but stem-cuttings of this plant readily form roots. Roots may arise in the neighbourhood of a wound ("wound roots") or from normally occurring root-germs ("morphological roots"), and in the majority of cases roots do not originate in the callus, but behind it. Roots never appear at an apical callus, although shoots may arise at a basal callus. A close correlation was found between the development of new shoots and root-formation. Both callus formation and root-growth are inhibited if the shoot buds are removed from the cuttings, and the same thing occurs to a less extent if the buds are not removed but fail to grow. This influence of the buds is restricted to cuttings which have passed through the dormant period, and is not observed in cuttings taken at the beginning of dormancy. The influence of the developing shoot on root-production is considered to be due to some action of hormone nature and not to purely nutritive factors.

Swingle ("Burr-knot of Apple Trees," *Journ. Hered.*, 1925, 16, 313-20), in an investigation of a large number of varieties, emphasises the distinction between burr-knots and the aerial form of Crown Gall. The burr-knots are actually clusters of young roots which do not grow owing to the unsuitability of external conditions. If surrounded by a moist medium, such as soil, these young roots will develop into a normal root system. Swingle also distinguishes very clearly between the two types of roots recognised by van der Lek. The development of "morphological roots" from normally occurring root initials is considered to be merely an extension by growth of already existing organs, and is quite distinct from true regeneration, which takes place when "wound roots" are formed.

Wallace and Hutchinson ("Development of Root Systems of Willow Cuttings in Nutrient Solutions," *Ann. Rept. Long Ashton Res. Sta.*, 1924, 25-9) found that the composition of the nutrient solution influenced the nature of the root system developed from the cutting. Lack of potassium or of calcium results in a root system deficient in fibre. A similar result was obtained with apples.

Marked differences have been recorded in the behaviour of different species and even varieties under propagation conditions. Malloch ("Asexual Propagation as an Aid to the Breeding of Rootstocks," *Journ. Agr. Res.*, 1924, 29, 515-21) investigated the rooting capacity of cuttings from different varieties of apples, and found much variation in the production of roots. Similar varietal differences were found by Hatton, Amos, and Witt ("Some Problems of Propagation," *Ann. Rept. East Malling Res. Sta.*, 1923, 100-9) in applying stooling, layering, and cutting methods to apples and plums.

Stewart (*Trans. and Proc. Bot. Soc., Edin.*, 1924, 29, 41-2) records that when *Gardenia* is propagated from a perpendicular shoot the resulting plant is vigorously vegetative and does not flower, whilst a plant obtained from a branch shoot produces flowers early in life. This latter type of flowering growth is, however, replaced after a few years by a vigorously vegetative sucker. A branch shoot never gives the upright form directly.

**RECENT ADVANCES IN PREHISTORIC ARCHÆOLOGY.** By J. REID MOIR, F.G.S., F.R.A.I.

*The Journal of the Royal Anthropological Institute* (vol. lv, 1925, January to June) contains three interesting papers on prehistoric archæology. The first, by Mr. Thomas and Mr. Dudleyke, deals with "a Flint Chipping Floor at Aberystwith" (pp. 73-89). The discovery was made in the vicinity of the

new Isolation Hospital at Aberystwyth, at the junction of the rivers Rheidol and Ystwyth, and due south of the town. The prehistoric occupation-level was found to rest upon glacial deposits, and to be covered by a dark-red loam—which the authors describe as “rain-wash”—in places about 4 feet in depth. The ancient land surface itself is composed of a dark, honey-coloured, friable soil, and in this material humanly-flaked flints were very numerous. It is supposed that the ancient artificers of Aberystwyth obtained their raw material—flint, chert, and chalcedony—from the nearby shingle beach, and, with this somewhat unsatisfactory medium, produced cores, scrapers, graters, knives, notched tools, awls, broad, leaf-shaped arrowheads, and “pigmy” points. Associated with these relics were found remains of charcoal and some burnt flints, together with certain implements, such as “limpet-scoops,” hones, axes, and adzes in fine-grained mudstone, or grit. It is supposed that the people who made these various specimens arrived upon the site soon after the glacial drift—underlying the ancient floor—had been laid down. After a review of all the evidence afforded by the discoveries, the authors favour the view that the Aberystwyth floor is probably of “very early Neolithic, or a slightly pre-Neolithic date,” and their paper provides a further contribution to the rapidly accumulating knowledge of Late Stone Age times in Britain.

The second paper (pp. 115–22), by Professor Wood Jones and Mr. T. D. Campbell, is entitled, “A Contribution to the Study of Eoliths: Some Observations on the Natural Forces at work in the production of Flaked Stones on the Central Australian Table-lands.” The authors took advantage of a stay at the Stuart Range Opal-fields (Coober Pedy) in August 1923, to examine the wide table-lands of the ranges, and make some study of the stones which lie strewn in myriads upon them. The results of this examination were to induce them to reject the opinion of Prof. Walter Howchin that many of these stones were flaked into eolithic forms by some long-vanished race of primitive Australians. It appears that the boulders of silicated desert sandstone occurring on the Central Australian table-lands disintegrate by the fracturing effects of alternating intense heat of the sun and nocturnal cold. The flakes detached in this process of disintegration sometimes carry away with them a part of the thermal pitting present upon the original boulder, and when this pitting is associated with the edge of the flake it is somewhat difficult, as the authors allege, to distinguish it from human flaking. In addition to this, however, the table-lands where these flakes are found are swept by violent winds which are able to lift pebbles which, impinging upon the edges of the flakes, remove further flakes of a definitely

percussive type. Again, it is stated that "the original fall of the flake from its parent boulder may produce some chipping by contact with other stones met with in its fall, while, "as a consequence of the crowded nature of the surface stone community, the movements of a particular fragment, or of adjacent fragments, or of a boulder itself, may produce stresses which cause chipping of the edges which is akin to pressure flaking."

The authors of this paper consider that the flakes they have collected, and examined, have an important bearing upon European eoliths, but it is very doubtful if this is the case. No one familiar with fractured flints can have very much difficulty in distinguishing between those fractured by thermal effects and other broken by human blows, or by natural pressure. The question as to whether the Harrisonian eoliths, for example, have been fractured by thermal changes does not now arise, and the only problem left for solution is whether the edge-flaking of these specimens has been caused by human or natural blows, or by natural pressure. The majority of those who have studied the Kentian eoliths favour the former alternative.

The third and most important paper is by Mr. Leslie Armstrong (pp. 146-78), and is entitled, "Excavations at Mother Grundy's Parlour, Cresswell Crag, Derbyshire, 1924." These successful excavations are being conducted by Mr. Armstrong on behalf of a Research Committee, appointed for the purpose, by the Royal Anthropological Institute, and the British Association. The site with which the first part of Mr. Armstrong's contribution deals is a rock-shelter, situated in the forefront of the cave known as Mother Grundy's Parlour, at Cresswell Crag, Derbyshire. The recent diggings have exposed a section consisting of five separate layers. The uppermost of these have been found to contain only relics of recent date, with the exception of that thrown out in excavations carried out many years ago, in which were discovered teeth of bison and hyæna discarded, or overlooked, by those who then investigated the site. Beneath the surface accumulations occur red cave-earth, and stones, to 3 feet in thickness, containing at its base flint implements of Upper Palæolithic Aurignacian types, associated with bone tools, and some engraved bones. These Upper Aurignacian implements were found also in the middle zone of the cave-earth, but, towards its surface, the artifacts assume the well-known forms of those referable to Azilian and early Tardenoisian times—that is, to the close of the Palæolithic epoch. Under the red cave-earth was discovered a layer of yellow cave-earth in which were found examples of coarsely-flaked implements of Mousterian type. The engraved bones were

discovered at the base of the Aurignacian deposit and exhibit on their surfaces outlines of (?) Reindeer, (?) Bison, and (?) Rhinoceros.

The human relics of the base, and the upper middle zones at the site examined were associated with bones of the rhinoceros, mammoth, hyæna-lion, and reindeer, while the upper zone, containing implements of Azilian type, yielded remains of horse, ox, pig, red-deer, fox and bison. The excavations carried out in the Pin Hole Cave, adjacent to Mother Grundy's Parlour, revealed a more or less similar series of deposits and artifacts, but in the former place a most important discovery was made of an engraved lance-point, with double bevel, in mammoth ivory. This specimen is of classic Magdalenian type, and its discovery establishes the former presence of the Magdalenians at Cresswell. Mr. Armstrong is to be congratulated upon these interesting finds, which he has described with lucidity, and by means of well-executed and telling illustrations.

## ARTICLES

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# THE NEW QUANTUM THEORY

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SINCE the birth of the Quantum Theory, which we now venture to describe as the Old Quantum Theory, there has been preserved in the minds of all physicists a hope that sooner or later a way would be discovered of obliterating those sharp discrepancies between it and the Classical Theory which form its characteristic feature.

A most important step in this direction has been made by Heisenberg, in a paper in the *Zeitschrift für Physik* (vol. 33, p. 879). He makes this step by adopting a plan of campaign which has, in the past, led to far-reaching successes in the domain of Physics. It is a method in which assumptions regarding details of structure are avoided, but, instead, a stand is taken on broad general principles which rest on direct observation.

Thermodynamics affords one of the best known examples of such a theory in Classical Physics. Its far-reaching results rest on general laws, and not on any ideas of the detailed structure of matter. Whatever this structure may be in its detail, one supposes that the natural laws are in keeping with the principles of Thermodynamics.

The Theory of Relativity is a more recent example of the same mode of attack. The fundamental principles are of a broad and general character. Deductions from them may throw light on details of structure. Theories about this structure may vary from one generation of theorists to another, but apparently the theory will stand, in spite of changing ideas concerning detail. It sometimes happens that the theories about details lead to difficulties or show up existing difficulties which might have been undiscovered ; at such a point a general theory is of value.

Before the Restricted Principle of Relativity arrived we had got into difficulties concerning the ether. The new mode of attacking the problem would have nothing to do with the

ether—it did not require its existence and it did not deny it the right of existence. The position is that the Relativist can get on with his theory without the ether, but if such a medium exists, then it must obey the laws of the Theory of Relativity. Heisenberg appears to begin his venture inspired by considerations of this sort. He introduces his paper by mentioning some of the objections which may be raised against the formal rules of the Old Quantum Theory. The rules of this theory concern quantities, such as positions and periods of electrons, which cannot be observed.

If one clings to the hope that in the future such quantities will come to be more and more directly observable as methods of observation improve, one can regard them as fundamentally observable. Such a hope might be justified if the rules made up a self-contained and consistent theory applicable to a definite domain. But experience shows that it is only in the case of the hydrogen atom and the Stark effect of this atom that the rules strictly apply. Difficulties arise at once in problems concerning "crossed fields," *i.e.* when electric and magnetic fields are present which are inclined to one another, and the rules prove unsatisfactory in considering the reaction of atoms on periodically changing fields, while finally an extension to the consideration of atoms with several electrons has proved impossible.

It is usually assumed that these difficulties arise because of the invalidity of classical mechanics. This, however, is not a very reasonable objection, for the fundamental principles of the Quantum Theory are already a complete denial of the validity of the Classical Theory, so that the question of its validity should not be raised again at so late a stage.

This being the state of affairs, Heisenberg considers it better to give up the hope that the quantities mentioned above will ever be observable and to regard the partial agreement of the well-known quantum rules with experience as fortuitous. He proposes to build up a system of Quantum Mechanics analogous to the system of Classical Mechanics in which relations between observable quantities occupy a fundamental place.

In his approach to this new system of Mechanics, he is guided most particularly by the frequency condition and the dispersion theory of Kramers.

Frequencies in spectral lines are known to be expressible in terms of two variables in the following way :

$$\nu(n, n - \alpha) = \frac{1}{h} \{W(n) - W(n - \alpha)\}$$

and the combination relation—

$$\nu(n, n - \alpha) + \nu(n - \alpha, n - \beta) = \nu(n, n - \beta).$$

is a characteristic rule of combination associated with the frequencies. Guided by this, Heisenberg introduces into his Mechanics a new law of combination for his quantities.

A quantity,  $x$ , is regarded as being expansible in the following way :

$$x = \sum_{\alpha} A_{(n, n-\alpha)} e^{i\omega(n, n-\alpha)t},$$

where  $\alpha$  extends over all integers from  $-\infty$  to  $+\infty$ , and in which the  $A$ 's depend on  $n$  and  $\alpha$ , while  $\omega$  also depends on these two quantities.

If we wish to find the value of  $x^2$  we proceed as follows :

$$x^2 = \sum_{\beta} B_{(n, n-\beta)} e^{i\omega(n, n-\beta)t},$$

where

$$B_{(n, n-\beta)} = \sum_{\alpha} A_{(n, n-\alpha)} A_{(n-\alpha, n-\beta)}.$$

It is in this operation that he departs from the rules of analysis hitherto applied in Mechanics, and one observes how the suggestion from the rule of combination of frequencies influences the law adopted for combination in the analysis.

If we consider the product,  $xy$ , of two quantities represented thus :

$$x = \sum_{\alpha} A_{(n, n-\alpha)} e^{i\omega(n, n-\alpha)t}$$

and

$$y = \sum_{\beta} B_{(n, n-\beta)} e^{i\omega(n, n-\beta)t}$$

we have for the product :

$$xy = \sum_{\gamma} C_{(n, n-\gamma)} e^{i\omega(n, n-\gamma)t},$$

where

$$C_{(n, n-\gamma)} = \sum_{\alpha} A_{(n, n-\alpha)} B_{(n-\alpha, n-\gamma)},$$

and one may see very readily that in this rule of multiplication the law of commutation does not hold, *i.e.*,

$$xy \neq yx.$$

This is of great importance in the theory.

The solution of any problem consists in determining the quantities  $A$ ,  $\nu$  and  $W$ . This was carried out in the old theory in two steps consisting in the integration of an equation of motion of the form—

$$x'' + f(x) = 0,$$

and in the determination of the constants in the case of periodic motion by means of :

$$\int p \, dq = nh.$$

In this case  $x$  is subject to the well-known rules of operation as applied in Classical Mechanics.

Heisenberg then passes on to a generalisation of the integral relation and adopts the result :

$$\hbar = 4\pi m \sum_0^{\infty} \{A_{(n,n+a)}^2 \omega_{(n,n+a)} - A_{(n,n-a)}^2 \omega_{(n,n-a)}\}$$

previously given by Kuhn and Thomas and derived by them from considerations of dispersion.

It is supposed that, corresponding to a normal state in which radiation does not occur, and for which the value of  $n$  is some number  $n_0$ .  $A(n_0, n_0 - a) = 0$  for all  $a's > 0$ . Thus in solving problems in the new theory we take

$$x = \sum_a A_{(n,n-a)} e^{i\omega_{(n,n-a)}t},$$

substitute in the equation of motion and by the aid of the above value of  $\hbar$  determine the unknown quantities, paying attention, of course, to the new laws of operation.

In this way questions of electronic paths, steady states, and of such difficult points as the consideration of half quantum numbers do not arise.

In this connection we may note one of the problems considered in the paper. This is the solution of the problem of an electron rotating at constant distance about a nucleus. Heisenberg finds the value :

$$W = \frac{\hbar^2}{8\pi^2 m a^2} (n^2 + n + \frac{1}{2})$$

for this case.

This is, of course, a different result from that obtained in the Old Quantum Theory, but it is to be remembered that many band spectra require a formula of this type and in connection with it the difficulty of half quantum numbers has arisen.

It will thus be seen that the equations of Classical Mechanics are not regarded as faulty even in their application to quantum phenomena, but the operations of the analysis by which results are deduced must be modified if these results are to agree with experiment.

It may appear at first sight that the introduction of such an analysis is very artificial. It is, however, merely unfamiliar. We have discovered by long experience that from Newton's Second Law of Motion,  $mx'' = f$ , we can derive results in accordance with experiment by adopting the well-known methods of a branch of Pure Mathematics. Success has encouraged the adoption of this notation and its long use has made it familiar—its artificiality has been forgotten.

Should such a notation as Heisenberg's meet with success, and remain in use long enough to be familiar it will cease to appear artificial.

Shortly after the publication of Heisenberg's paper Born and Jordan (*Zeitschrift für Physik*, vol. 34, p. 858) pointed out that the operations introduced in the New Quantum Theory were none other than those of matrix analysis. This has become familiar of late years to the physicist because of its application in the Theory of Relativity. In this theory it is applied in a very general form, but in the Quantum Theory, at any rate so far as it has been applied up to the present, only very simple and restricted parts of this analysis are necessary.

The matrix analysis has been applied in the Theory of Elasticity and in the reduction of Maxwell's Equations in Electrodynamics.

In this case we consider a quantity represented by the matrix—

$$\begin{vmatrix} p_{xx} & p_{xy} & p_{xz} \\ p_{yx} & p_{yy} & p_{yz} \\ p_{zx} & p_{zy} & p_{zz} \end{vmatrix}$$

and we describe it as the stress-matrix, the components denoting the stress components.

The New Quantum Theory states that physical quantities are to be represented by such expressions as these and the analysis used in this theory is characterised by the fact that the matrix analysis takes the place of the familiar analysis of Classical Physics. It is well known that this analysis has a wide application also in Classical Physics and in the Theory of Relativity, although, perhaps, there is no need to regard Relativity as non-classical. This fact raises the hope that by this means it may be possible to find a way out of those striking clashes between the Quantum and older theories in the use of a language capable of expressing both.

The whole question is, of course, in an early stage of its progress and can only be settled by repeated applications of the new ideas to problems so that the results obtained may be verified by experiment.

Born and Jordan devote a few pages to the explanation of the operations of matrix analysis, and one of the simple and striking features which deserves special mention by reason of its importance is that the rule of multiplication is not commutative. This corresponds to Heisenberg's rule mentioned above.

It is, perhaps, better to adopt a vector notation rather than the matrix notation. This notation is more familiar to physicists and has the special feature that vectors correspond very closely with the quantities measured. The vector rules of operation are familiar, and those of tensors (or dyadics, as Gibbs called them in their simplest form) follow very simply.

We denote vector or tensor quantities by the use of clarendon

type. A vector,  $\mathbf{a}$ , may be multiplied scalarly by  $\mathbf{b}$  and the scalar product is denoted by  $\mathbf{a} \cdot \mathbf{b}$ .

The vector product is denoted by  $\mathbf{a} \times \mathbf{b}$  and the quantity denoted by  $\mathbf{A} = \mathbf{ab}$  is a tensor of the second order, or dyadic. This is the kind of quantity which is used in the New Quantum Theory.

In this theory we consider  $n$  vectors :

$$\mathbf{i}_1, \mathbf{i}_2, \mathbf{i}_3, \dots \mathbf{i}_n.$$

We speak of these as the fundamental vectors, and they have the properties denoted by :

$$\mathbf{i}_p^2 = \mathbf{i}_p \cdot \mathbf{i}_p = 1.$$

$$\text{and } \mathbf{i}_p \cdot \mathbf{i}_q = 0, p \neq q.$$

A vector,  $\mathbf{a}$ , can be expanded in terms of its components thus :

$$\mathbf{a} = a_1 \mathbf{i}_1 + a_2 \mathbf{i}_2 + \dots + a_p \mathbf{i}_n$$

$$= \sum a_m \mathbf{i}_m, \text{ or, written more shortly, } = a_m \mathbf{i}_m,$$

where it is understood that the summation is to be made. The tensor  $\mathbf{A} = \mathbf{ab}$  may also be expanded and may be written in the form :

$$\mathbf{A} = A_{11} \mathbf{i}_1 \mathbf{i}_1 + A_{12} \mathbf{i}_1 \mathbf{i}_2 + \text{etc.}$$

$$= A_{pq} \mathbf{i}_p \mathbf{i}_q \text{ (for convenience).}$$

Regarded as a matrix, it is customary to write :

$$\mathbf{A} = \begin{vmatrix} A_{11} & A_{12} & A_{13} & \dots & \dots \\ A_{21} & A_{22} & A_{23} & \dots & \dots \\ A_{31} & A_{32} & A_{33} & \dots & \dots \\ \vdots & \vdots & \vdots & \ddots & \ddots \\ \vdots & \vdots & \vdots & \ddots & \ddots \end{vmatrix}$$

The single scalar product of two of these quantities is defined thus :

$\mathbf{A}$  and  $\mathbf{B}$  are expanded by means of their components :

$$\mathbf{A} = A_{pq} \mathbf{i}_p \mathbf{i}_q, \quad \mathbf{B} = B_{rs} \mathbf{i}_r \mathbf{i}_s$$

$$\mathbf{A} \cdot \mathbf{B} = A_{pq} B_{rs} \mathbf{i}_p \mathbf{i}_q \cdot \mathbf{i}_r \mathbf{i}_s.$$

$\mathbf{i}_q$  and  $\mathbf{i}_r$  are multiplied scalarly so that the product vanishes unless  $q = r$ , in which case it is unity. Thus, if we remember that in the summation  $q$  and  $r$  have the values 0 to  $n$  we find :

$$\mathbf{A} \cdot \mathbf{B} = (A_{p1} B_{1s} + A_{p2} B_{2s} + A_{p3} B_{3s} + \dots) \mathbf{i}_p \mathbf{i}_s$$

$$= A_{pa} B_{as} \mathbf{i}_p \mathbf{i}_s.$$

This leaves a tensor of the second order.

Note that

$$\mathbf{B} \cdot \mathbf{A} = B_{pa} A_{as} \mathbf{i}_p \mathbf{i}_s$$

and that  $B_{pa} A_{as} \neq A_{pa} B_{as}$  except in special cases when some special property is possessed by the  $A$  and  $B$  components.

Thus:  $\mathbf{A} \cdot \mathbf{B} \neq \mathbf{B} \cdot \mathbf{A}$  in general, this being the non-commutative property used by Heisenberg in his notation. A particular tensor is the following:

$$\mathbf{I} = \mathbf{i}_1 \mathbf{i}_1 + \mathbf{i}_2 \mathbf{i}_2 + \dots + \mathbf{i}_n \mathbf{i}_n = \mathbf{i}_p \mathbf{i}_p.$$

It will be noted that this is a very simple form, and it is known as the idem-factor.

Apply it scalarly to a vector,  $\mathbf{a}$ , remembering that  $\mathbf{a} = a_p \mathbf{i}_p$

$$\begin{aligned} \mathbf{I} \cdot \mathbf{a} &= \mathbf{i}_p \mathbf{i}_p \cdot \mathbf{a} \\ \text{Now } \mathbf{i}_p \cdot \mathbf{a} &= a_p \\ \therefore \mathbf{I} \cdot \mathbf{a} &= a_p \mathbf{i}_p = \mathbf{a}. \end{aligned}$$

Thus the operation leaves  $\mathbf{a}$  unchanged, hence the name idem-factor.

$$\text{Similarly } \mathbf{I} \cdot \mathbf{A} = \mathbf{A}.$$

It is, of course, impossible to give more than a superficial introduction to the analysis here. The whole calculus has been in use for a long time, though not applied extensively in Physics in this country. We are indebted for it to Hamilton, who built up the notation in his Quaternions, to A. McAulay, who has extended it and applied it in Mathematical Physics, and to Gibbs, Wilson, and Lewis, who have presented it in this simple form. In concluding this introduction to the notation we must add the law of differentiation:

$$\begin{aligned} \frac{d}{dt} \mathbf{A} &= \dot{\mathbf{A}}_{pq} \mathbf{i}_p \mathbf{i}_q \\ \frac{d}{dt} (\mathbf{A} \cdot \mathbf{B}) &= \dot{\mathbf{A}} \mathbf{B} + \mathbf{A} \dot{\mathbf{B}}. \end{aligned}$$

We pass now to the fundamental laws of Mechanics, which are to be described in this notation.

In laying down the laws it is stated that the dynamical system is to be described by two tensors,  $\mathbf{q}$  and  $\mathbf{p}$ , both functions of the time and defined as follows:

$$\begin{aligned} \mathbf{q} &= q_{nm} e^{2\pi i \gamma_{nm} t} \mathbf{i}_n \mathbf{i}_m \\ \mathbf{p} &= p_{nm} e^{2\pi i \gamma_{nm} t} \mathbf{i}_n \mathbf{i}_m. \end{aligned}$$

where  $i = \sqrt{-1}$ , and it need hardly be mentioned that  $\gamma_{nm}$  corresponds to what is described in the Old Quantum Theory as the frequency of radiation due to transition of an electron between two stationary states characterised by the integers  $n$  and  $m$ , though we are not concerned at present with electrons, stationary states, or transitions.

The quantities  $\gamma_{nm}$  are such that  $\gamma_{nm} = -\gamma_{mn}$  and the components  $q_{nm}$  and  $p_{nm}$  are such that

$$q_{nm} = q_{mn} \text{ and } p_{nm} = p_{mn}.$$

Another way of stating this is to say that  $\mathbf{q}$  and  $\mathbf{p}$  are self-conjugate, for the conjugate of a tensor,  $\mathbf{A}$ , is denoted by  $\mathbf{A}^1$ , where  $\mathbf{A} = A_{mn} \mathbf{i}_m \mathbf{i}_n$  and  $\mathbf{A}^1 = A_{nm} \mathbf{i}_m \mathbf{i}_n$ .

If  $A_{nm} = A_{mn}$  then  $\mathbf{A}^1 = \mathbf{A}$  and the term self-conjugate is justified.

It is further assumed that :

$$\gamma_{jk} + \gamma_{kl} + \gamma_{lj} = 0$$

Thus :

$$\gamma_{jk} = \gamma_{jl} - \gamma_{kl}$$

On the right we have two quantities depending on  $j$  and  $k$  respectively, and we can write this side in the form :

$$\frac{1}{h} (W_j - W_k),$$

so that :

$$\gamma_{jk} = \frac{1}{h} (W_j - W_k).$$

This is clearly the introduction of the frequency condition into the Mechanics.

In Classical Mechanics the equations of motion of any system are

$$\begin{aligned} \dot{q} &= \frac{\partial H}{\partial p} \\ \dot{p} &= -\frac{\partial H}{\partial q}, \end{aligned}$$

where  $H$  is the Hamiltonian function,  $q$  is one of the generalised co-ordinates of the system and  $p$  is the corresponding generalised momentum, and the number of pairs of equations like the above is equal to the number of the co-ordinates.

It is assumed that the equations of motion in the New Theory are :

$$\begin{aligned} \dot{\mathbf{q}} &= \frac{\partial \mathbf{H}}{\partial \mathbf{p}} \\ \dot{\mathbf{p}} &= -\frac{\partial \mathbf{H}}{\partial \mathbf{q}}, \end{aligned}$$

where the quantities are second order tensors.

In their paper, Born and Jordan consider the case of systems defined by one co-ordinate only, but a treatment of cases with greater degrees of freedom is possible.  $\mathbf{H}$  is a function of the co-ordinates and momenta.

It is possible to reduce the whole system to a principle of action, as in the generalised Mechanics of the Classical Theory, and also to generalise the condition of Wilson and Sommerfeld :

viz. 
$$\int p \, dq = nh,$$

into the form adopted by Heisenberg.

The writers then pass on to the consideration of a number of problems and to determine the values of  $\nu$  appropriate to these cases. The whole method, by its generality and freedom from difficulties, such as those of half quantum numbers, gives hope that many problems, whether periodic or aperiodic, will be satisfactorily solved and compared with experiment in the near future.

It is essential to examine the equations of Electrodynamics, to see how they can be expressed in terms of the new system. There is no fundamental difficulty in this, for by their resemblance to the equations of the theory of elasticity we know that it is possible to write them in the tensor notation. Born and Jordan close their investigation by touching upon this point.

It is a problem for the future to consider what relation the theory of light quanta has to the present theory. This and many other questions offer a wide field for investigation, and mark this effort to throw light on the discrepancies between two important branches of Physics as the most important and far-reaching that has yet been made.

A more recent paper by Dirac (*Proc. Royal Society*, Dec. 1st, 1925, p. 642) is also a research into the nature of Heisenberg's operations and an explanation of a more general notation of which they form a part. A noteworthy feature is the extension to multiply periodic systems, but although no consideration of other systems is given in the paper there is no reason why non-periodic motion such as occurs in a complex atom should not be considered. Any difficulties arising would be difficulties due to calculation, and would not require special assumptions outside the theory.

# NOTES ON THE DIELECTRIC CONSTANTS OF LIQUIDS.

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THE following notes are intended to serve as a brief résumé of our present state of knowledge regarding the dielectric constants<sup>1</sup> of liquids, of their complex and at present only partially understood variations under different conditions, and of phenomena to which they are apparently related.

*Connection between Dielectric Constants of Pure Liquids and their Other Properties, Physical and Chemical.*—Grimm and Patrick<sup>2</sup> go so far as to say that "It is our belief that no generalisation connecting the dielectric constant with other (physical) properties has been formulated that is worthy of serious consideration." However, certain empirical relations have been found which, although approximate, and even then subject to numerous exceptions, are at least of interest, and may possess a hidden significance to be discovered later. A few of these will be cited. The approximate proportionality between the latent heat of vaporisation and the dielectric constant to which Obach<sup>3</sup> first drew attention was investigated more carefully by Dobrosserdov, who also pointed out other regularities in the quantitative relations between the dielectric constants and other physical properties of substances, in Russian publications which are often inaccessible<sup>4</sup>; the proportionality constant is about 6.5 for alcohols, nitriles, ketones, and alkyl halides, 25.5 for amines, 31 for aromatic hydrocarbons, and 39.5 for aliphatic acids. A number of such relations has also been found by Walden; a convenient précis is contained in his book *Elektrochemie Nichtwässriger Lösungen* (Barth, Leipzig, 1924). He finds that the latent heat of vaporisation at the b.p. divided by the Clausius-Mossotti expression  $\frac{\epsilon - 1}{\epsilon + 2} \frac{1}{d}$ , where  $\epsilon$  and  $d$  are the dielectric constant and the density respectively (measured at room temperature),

<sup>1</sup> The terms *dielectric constant* and *specific inductive capacity* are used synonymously throughout this article.

<sup>2</sup> *Jour. Amer. Chem. Soc.*, 1923, 45, 2794.

<sup>3</sup> *Phil Mag.*, 1891, 33, 113.

<sup>4</sup> See *Chem. Abs.*, 1911, 5, 607, 3362; 1912, 6, 1564, 2350.

has approximately the value 115 for forty-two substances, the extreme deviation from this mean value being of the order of 10 per cent. Even this approximate constancy is not followed when many other compounds are concerned, the values for benzene and water being 290 and 553 respectively. A similar "constancy" is also found for the expressions  $\frac{MK_1}{T}$  and  $\frac{\epsilon}{\alpha_o^2}$ , where  $M$  = molecular weight,  $K_1$  = the Clausius-Mossotti expression,  $T$  = absolute b.p., and  $\alpha_o^2$  is the specific cohesion at the boiling-point. Walden points out that the greatest deviations are found usually with strongly associated liquids. It will be observed that the values of  $\epsilon$  employed were those at room temperature; Cauwood and Turner<sup>1</sup> and Grimm and Patrick<sup>2</sup> draw attention to the desirability of the dielectric constant being measured at the same temperature as are the other properties with which it is being compared.

According to the Maxwell electromagnetic theory of light, the square of the refractive index of a liquid is equal to its dielectric constant. There is a common misconception that in the mathematical derivation of the theory the properties of only infinitely long waves were considered. The theory applies to waves of any length, but, as Maxwell says himself, "... the value of the index of refraction is different for light of different kinds, being greater for light of more rapid vibrations. We must therefore select the index of refraction which corresponds to waves of the longest periods, because these are the only waves whose motion can be compared with the slow processes by which we determine the capacity of a dielectric." We cannot, even now, determine the dielectric constants of substances using waves of frequencies at all near those obtaining in the visible spectrum, and for many liquids the squares of the refractive indices for light are not approximately equal to the values of the dielectric constants as measured by the means available to us. It should be noted, however, that the squares of the refractive indices of sufficiently long (electrical) waves have been found to be approximately equal to the dielectric constants of substances where such equality could not be obtained by the use of the refractive indices of light.

From the scanty experimental data then available, Maxwell concluded that our theories of the structure of bodies must be much improved before we can deduce their optical from their electrical properties. In this connection it is interesting to observe that Sir J. J. Thomson<sup>3</sup> states that one would

<sup>1</sup> *Jour. Chem. Soc.*, 1915, 107, 276.

<sup>2</sup> *Loc. cit.*

<sup>3</sup> *Phil. Mag.*, 1914, 27, 757; *The Electron in Chemistry* (Chapman & Hall, 1923).

expect substances containing polar molecules to depart widely from Maxwell's Law. He assumes that polar molecules have finite electrostatic moments, and says: "The molecules with a finite moment will tend to set in a definite direction under the electric field . . . hence such molecules will tend to give an abnormally high value to the specific inductive capacity. Again, since this setting of the molecule involves the rotation of the molecule as a whole, these will move so sluggishly that they will not be affected by vibrations as rapid as those of light-waves in the visible spectrum."

It is well known that the dielectric constants of liquids are usually qualitatively proportional to the degree of association of the molecules, though here, also, are marked exceptions.<sup>1</sup> If we assume that molecules which tend to associate possess electric moments, and this is now well substantiated, such exceptions can easily be understood. The electric moment

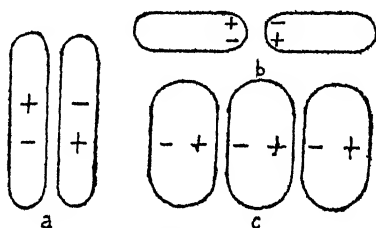


FIG. 1.

implies the presence of at least one pair of electric charges, of opposite sign, separated by a finite distance. These pairs of charges are known as dipoles. When several dipoles are in the same molecule, they will give a resultant moment. Ebert<sup>2</sup> points out very clearly that according to the position of the dipole in the molecule several types of association products are possible; examples are given in Fig. 1 (after Ebert). In cases a and b complexes are formed whose resultant moment is vanishingly small; in case c the resultant moment is large, thus tending to cause a high dielectric constant.

Even when dipoles are present, association may be slight, as Ebert remarks. Very small molecules containing dipoles, *i.e.* molecules where the distances between the charges approach the "diameters" of the molecules, will be strongly associated, whereas substances such as the higher aliphatic alcohols, where the dipole is at the end of a long chain, will only be slightly associated, if at all. With the latter substances the effect of the presence of dipoles may be shown only by the regular

<sup>1</sup> Cf. Turner, *Molecular Association* (Longmans, 1915).

<sup>2</sup> *Z. physikal. Chem.*, 1924, 113, 1.

orientation obtained when thin layers are on the surface of, say, water.

Walden finds a rough proportionality between the internal pressure of a liquid and its dielectric constant. W. C. McC. Lewis<sup>1</sup> shows that this is to be expected from the Obach relation, as a large latent heat accompanies large cohesion, and is in agreement with the hypothesis that molecular attraction is electromagnetic, not electrostatic, in which latter case one would expect the internal pressure to vary inversely as the dielectric constant.

When we turn to the connection between dielectric constant and the more purely chemical properties, we find again a number of general rules, subject to numerous exceptions, apparent at least, and certainly modified by other properties of the substances concerned. This connection can be, as with other physical properties, most easily followed by a study of organic compounds, as a vast number are liquid under ordinary conditions, or are capable of being liquefied without decomposition; also the constitution of many series of these substances is well established.

It was early recognised that the presence of certain groups, for example OH, CO, CN, and NO<sub>2</sub>, often accompanied high specific inductive capacity. Thwing<sup>2</sup> was apparently the first to make a careful investigation of the specific quantitative contributions of the various atoms and groups to the dielectric constants of compounds containing them. He put forward the equation  $\epsilon = d(a_1\epsilon_1 + a_2\epsilon_2 + \dots)/M$ , when  $\epsilon$ ,  $d$ , and  $M$  have the same significance as above, and  $a_1$ ,  $a_2$  . . . and  $\epsilon_1$ ,  $\epsilon_2$  . . . are the numbers of atoms or groups of the same kind and their "dielectric constants" respectively, these "dielectric constants" being deduced by Thwing from the examination of a large number of compounds. Some of Thwing's values for  $\epsilon_1$ , etc., at 15°, are as follows: H = 2·6, C = 2·6 × 12, CH<sub>3</sub> = 41·6, CH<sub>4</sub> = 46·8, O = 2·6 × 16, OH = 1356, CO = 1520, NO<sub>2</sub> = 3090. The danger of formulating a comprehensive rule without an exhaustive study of relevant data is here well illustrated. The above equation holds most remarkably well for many substances, but is quite inapplicable to others; if Thwing's formula and constants are used, the calculated value of the dielectric constant of tetranitromethane is 104·3, whereas it is actually about a fiftieth of this amount (2·13 at 23°). Rise of molecular weight in homologous series is usually accompanied by a decrease of dielectric constant, e.g. with the alkyl halides and the fatty acids; the reverse occurs, however, with the paraffins. From Thwing's point of view this behaviour is related to the variation in density of the substances concerned; the densities of the

<sup>1</sup> *Phil. Mag.*, 1914, 28, 104.

<sup>2</sup> *Z. physikal. Chem.*, 1894, 14, 297.

alkyl halides and the fatty acids decrease with rising molecular weight, whilst those of the paraffins increase. From other considerations, also, one would expect the dielectric constants of the paraffins and of their simple derivatives to approach a limiting value with rise of molecular weight in homologous series. When halogen derivatives of the same compound are examined, it is usually observed that the chloro- derivative has the highest and the iodo- the lowest dielectric constant. The evidence with regard to the effect of unsaturation is conflicting: a change from the unsaturated to the saturated state sometimes causes a rise and sometimes a fall of dielectric constant. As would be expected, the dielectric constants of isomers are not the same, this generalisation even embracing ortho-, meta-, and para- derivatives of benzene.

Examples of apparent connections or non-connections between dielectric constant and purely chemical constitution could, naturally, be multiplied. Enough has been said to give some idea as to how the matter stands. For further details and references to the original literature, Kauffmann's *Beziehungen zwischen physikalischen Eigenschaften und chemischer Konstitution* (Enke, Stuttgart, 1920) and an article by Meyer<sup>1</sup> may be consulted profitably.

In order that any marked advance in this connection may be made, an examination of both the chemical and the electric structure of the molecule is necessary, as is now being done. It has been noted above that the presence of some groups often leads to high specific inductive capacity. Such groups are polar. The consequent effect with regard to refractive index and degree of association has already been touched on. Where the electrical charges of the molecule are disposed symmetrically, *i.e.* where the molecule has not a definite electrostatic moment, the dielectric constant tends to be low, as with carbon bisulphide and carbon tetrachloride. Even when strongly polar groups are present but are symmetrically disposed, the same result will be obtained. The low value for tetranitromethane previously mentioned is thus explained. Sir J. J. Thomson points out<sup>2</sup> that the fact that the polar molecule must be unsymmetrical is an aid in determining its structure. Sulphur dioxide has a high dielectric constant. "It is therefore more likely to be represented by one of the unsymmetrical formulas  $S=O-O$ ,  $O=S-O$ , than by the more symmetrical one



." This type of method of determining the constitution of the molecule is being much employed in an elaborated form. It is clear that a critical inspection of the absolute values

<sup>1</sup> *Ann. Physik*, 1924, **75**, 801.

<sup>2</sup> *The Electron in Chemistry*.

of the moments of molecules, side by side with those of their constituents, should throw much light on chemical constitution. Such absolute values can now be calculated to a certain degree of approximation by means of Debye's theory (see below). These methods can be most successfully applied where the molecules are examined in the gaseous state, *i.e.* when they are not so subjected to mutual influences as in the liquid condition.

*Influence of Temperature.*—It is well known that the dielectric constants of liquids increase as the temperature falls. This effect is especially marked with liquids of high specific inductive capacity, for example, the dielectric constants of water at  $2\cdot3^{\circ}$  and  $99\cdot5^{\circ}$  are  $90\cdot7$  and  $67\cdot4$  respectively; on the other hand, those of benzene at  $18^{\circ}$  and  $80^{\circ}$  are  $2\cdot29$  and  $2\cdot17$ . Benzene is non-associated and water is associated at ordinary temperatures; rise of temperature diminishes the degree of association; here, therefore, is a type of association which apparently increases the dielectric constant beyond that due to single molecules.

The temperature-dielectric constant curves for liquids are generally nearly, if not quite, linear over considerable ranges of temperature; the curves are often slightly sagged towards the ordinates,<sup>1</sup> but there does not appear to be any evident relationship between extent of curvature and chemical character. Eversheim<sup>2</sup> and Tangl<sup>3</sup> have observed that when the temperature of the liquid under examination is near the critical point, a sudden rapid drop of dielectric constant occurs. A sudden rapid drop of dielectric constant also occurs when the temperature is lowered to the freezing-point. If dielectric constants are plotted as ordinates and temperatures as abscissæ, the curve commences to fall almost vertically a few degrees above the freezing-point. The extent of the drop may be large or small, depending on the substance concerned; a very slow change of the dielectric constant of the solidified substance follows, usually a decrease. These low-temperature phenomena were first studied by Dewar and Fleming<sup>4</sup> and by Abegg and co-workers,<sup>5</sup> contemporaneously. Dewar and Fleming noticed that the measured dielectric constant in this temperature range was very sensitive to changes of frequency, when this was low. From the small amount of experimental data available, it would seem that a certain minimum frequency is required for

<sup>1</sup> Cf. Cauwood and Turner, *loc. cit.*

<sup>2</sup> *Ann. Physik*, 1902, 8, 539.

<sup>3</sup> *Ibid*, 1903, 10, 748.

<sup>4</sup> Their results are published in a series of papers in the *Proceedings of the Royal Society*, 1897 and 1898.

<sup>5</sup> A number of papers in the *Annalen der Physik* and *Zeitschrift für physikalische Chemie*, 1897-99.

the decrease in dielectric constant to commence near the freezing-point.

Many examples now exist of this mode of change of dielectric constant on freezing, the phenomenon occurring with all types of substances, for example, various inorganic hydrides<sup>1</sup> and liquid hydrogen.<sup>2</sup> Isnardi has recently studied this phenomenon with a number of organic liquids.<sup>3</sup> His results for ethyl ether (*mp.*  $-117^{\circ}$ ) are shown in Fig. 2, as typical. The measurements were not carried to a high enough temperature to show the accelerated decrease near the critical point.

In accordance with the Clausius-Mossotti theory, the expression  $\frac{\epsilon - 1}{\epsilon + 2} \cdot \frac{1}{d}$  should be independent of temperature. This is not usually the case, however. Tangl, for example, observed

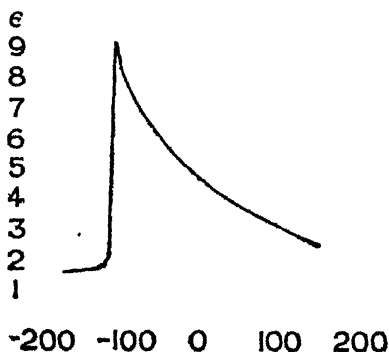


FIG. 2.

that the numerical value for this expression was constant for xylene between  $0^{\circ}$  and  $130^{\circ}$ , but with carbon bisulphide it increased by 5.4 per cent. between  $0^{\circ}$  and  $200^{\circ}$ , and with ether by 17 per cent. between  $20^{\circ}$  and the critical temperature,  $193.3^{\circ}$ . Walden<sup>4</sup> found that a decrease with rising temperature occurred sometimes, *e.g.* with benzyl alcohol the "constant" changes from 0.784 to 0.725 between  $1^{\circ}$  and  $49^{\circ}$ .

These variations are explained by the Debye theory, which is beginning to introduce order into the whole subject of specific inductive capacity. The conception of electric moments has already been brought forward. Thomson<sup>5</sup> reminds us that Sutherland<sup>6</sup> was the first to consider quantitatively the forces

<sup>1</sup> Palmer and Schlundt, *Jour. Phys. Chem.*, 1904, 8, 122.

<sup>2</sup> Wolfke and Onnes, *Proc. K. Akad. Wetensch., Amsterdam*, 1924, 27, 627.

<sup>3</sup> *Z. Physik*, 1922, 9, 153.

<sup>4</sup> *Z. physikal. Chem.*, 1910, 70, 509.

<sup>5</sup> *Phil. Mag.*, 1914, 27, 764.

<sup>6</sup> *Ibid.*, 1895, 39, 1.

exerted by a molecule containing electric doublets. Debye<sup>1</sup> assumes that each molecule (*a*) contains electrons bound by quasi-elastic forces to positions of rest, and (*b*) possesses a permanent electric moment.

According to the classical electron theory, the movement of such electricity as that under (*a*) by means of an applied electric field conditions the polarisation of the body and is in accordance with the theoretical constancy of the Clausius-Mossotti expression. Debye shows that where permanent doublets are present, this expression will change with temperature. The doublets tend to set in an electric field, thus contributing to the polarisation and dielectric constant of the medium. For clearness it should be stated that the conventional mathematical expression of polarisation is  $\frac{\epsilon - 1}{\epsilon + 2}$ , which is a measure of the total moment induced per unit volume by an applied electric field. The polarisation is a more characteristic function of a substance than is the dielectric constant *per se*. Polarisation may also be expressed in terms of the total moment per gram, or per molecule.

Debye deduced the following formula for gases at constant volume:

$$\frac{\epsilon - 1}{\epsilon + 2} = b + \frac{a}{T},$$

where *a* and *b* are constants characteristic of the substance. The term  $\frac{a}{T}$  contains the contribution of the permanent doublets to the total polarisation. *a* and *b* can be determined from measurements of the dielectric constant at different temperatures. Where the density changes with temperature, as in the case of liquids, the equation is modified to

$$\frac{\epsilon - 1}{\epsilon + 2} \cdot \frac{1}{d} = b + \frac{a}{T}.$$

If no dipole is present,  $\frac{\epsilon - 1}{\epsilon + 2} \cdot \frac{1}{d} = b = \text{constant}$ , in accordance with the classical theory. The moment,  $\mu$ , of the dipole is given by

$$\mu = \frac{3}{4\sqrt{\pi}} \sqrt{\frac{Ka}{N}},$$

where *K* is the Boltzmann-Planck constant and *N* is the number of molecules per cubic centimetre. Sir J. J. Thomson<sup>2</sup> has derived an equation for gases similar to that of Debye, namely,

$$a + \frac{.88\mu^2 \times 10^{18}}{T},$$

<sup>1</sup> *Physikal. Z.*, 1912, 13, 97.

<sup>2</sup> *Loc. cit.*

where  $a$  is a constant, independent of temperature. Debye's theory has been tested and developed by other investigators, of whom Gans and Isnardi, and Smyth may perhaps be specially mentioned.<sup>1</sup> Gans's equation takes account of the mutual influence of the molecules, but has been further modified by Smyth to a form which is not affected so much by association. These latter equations are considerably more complicated than that of Debye and will not be discussed here in further detail. The permanent moments of many molecules have now been calculated. In most cases it is found that the moments of substances usually classed as non-polar are vanishingly small, while those of polar substances are comparatively large.

In a further paper<sup>2</sup> Debye considers mathematically the effect of the frequency of the electrical oscillations used in measuring specific inductive capacity. The doublets take a finite time to set in an equilibrium position under the influence of an applied electrical field, the position of equilibrium being controlled by the opposing influences of the electrical field and of the temperature obtaining, the latter tending to make the molecules assume a haphazard orientation. If the field is rapidly and continually reversed, as is the case when measurements of dielectric constant are made, the equilibrium position will not be attained in either direction, if the reversals are more rapid than the time required to attain equilibrium. With increasing frequency the amplitude of swing of each doublet about a mean position will therefore become less, thus the polarisation of the medium and its dielectric constant will also become less. A correction for viscosity is introduced into the formulæ governing the above phenomena. This influence of increasing frequency is observed experimentally, though it may be small over a considerable range of wave-lengths. It should not be confused with the phenomenon of anomalous absorption.

The Debye theory cannot yet be said to be in its final form, but is even now proving itself of great use.<sup>3</sup>

<sup>1</sup> Gans, *Ann. Physik*, 1921, **64**, 481; Isnardi and Gans, *Physikal. Z.*, 1921, **22**, 230; Isnardi, *Z. Physik*, 1922, **9**, 152; Smyth, *Phil. Mag.*, 1923, **45**, 849; *ibid.*, 1924, **47**, 530; *Jour. Amer. Chem. Soc.*, 1924, **46**, 2151; 1925, **47**, 1894.

<sup>2</sup> *Verh. D. phys. Ges.*, 1913, **11**, 777.

<sup>3</sup> The following additional selection of references is given for the benefit of those interested in the Debye theory. Ratnowsky, *Verh. D. phys. Ges.*, 1913, **11**, 497; Boguslawski, *Physikal. Z.*, 1914, **15**, 283; Herweg, *Physikal. Z.*, 1920, **21**, 572; Zahn, *Phys. Rev.*, 1924, **24**, 400; Smyth and Zahn, *Jour. Amer. Chem. Soc.*, 1925, **47**, 2501; Ebert, *Z. physikal. Chem.*, 1925, **114**, 430; Debye, *Handbuch der Radiologie*, **6**, 1925 (Akademischen Verlagsgesellschaft M.B.H., Leipzig); Henri, *Structure des Molécules*, 1925 (Hermann, Paris).

*Influence of Pressure.*—Only a few investigations on the influence of pressure on dielectric constants of liquids appear to have been made.<sup>1</sup> The effect of increase of pressure is small, but all investigators agree in that it increases the dielectric constant. The results of Röntgen and of Ratz may be regarded as qualitative only. Ortway studied nine liquids, all of low specific inductive capacity, using pressures up to 500 atmospheres. He found that the dielectric constant appeared to approach a limiting value with increasing pressure, a formula of the type

$$D_p = D_1 (1 + \alpha p + \beta p^2),$$

where  $\alpha$  and  $\beta$  are positive and negative constants respectively, thus being applicable. Falckenberg studied water, ethyl alcohol, methyl alcohol, and acetone, under pressures up to 200 atmospheres. The mean percentage increases of dielectric constant per atmosphere over this range were 0.0046, 0.0097, 0.0102 and 0.0160 respectively. His dielectric constant-pressure curves are almost linear. He is of the opinion that Röntgen's view that the ratio polymerised molecules/non-polymerised molecules is influenced by pressure is correct. Grenacher, who used pressures up to 60 atmospheres, found that polar liquids were apparently more sensitive to the influence of pressure than non-polar liquids, but his data were insufficient for a definite generalisation to be made. Waibel (1–120 atmospheres) and Francke (1–800 atmospheres) studied a few liquids of low specific inductive capacity. The dielectric constant-pressure curves of both these investigators are nearly linear, but have a weak, though decided, curvature away from the pressure axis, thus being in accordance with Ortway's results. The following figures selected from Francke's results for hexane give a typical curve when plotted :

$p$ (atm.)	$\epsilon$
50	1.8905
150	1.9044
250	1.9181
350	1.9310
450	1.9417
550	1.9514
650	1.9612
750	1.9665

In the few cases where the necessary compressibility data have been available it has been found that  $\frac{\epsilon - 1}{\epsilon + 2} \frac{1}{d}$  diminishes

<sup>1</sup> Röntgen, *Ann. Physik*, 1894, 52, 591; Ratz, *Z. physikal. Chem.*, 1896, 19, 94; Ortway, *Ann. Physik*, 1911, 36, 1; Falckenberg, *Ann. Physik*, 1920, 61, 145; Waibel, *Ann. Physik*, 1923, 72, 161; Grenacher, *Ann. Physik*, 1925, 77, 138; Francke, *Ann. Physik*, 1925, 77, 159.

with increasing pressure: for example, with ether this expression diminishes by 1.3 per cent. between 1 and 500 atmospheres (Ortway). It may be remarked, in passing, that Francke finds that the dielectric constant of *solid* benzene is also increased by pressure; the dielectric constants of the solid and liquid phases of this substance are, however, influenced in opposite directions by corresponding temperature changes.

*Dielectric Constants of Non-conducting Solutions.*—Determinations of the dielectric constants of binary mixtures of organic liquids have been made from an early date. Formulæ, based on the law of mixtures, for the dielectric constants of the resulting liquids have been proposed, but do not hold generally. Phillip,<sup>1</sup> instead of applying a mixture law to the dielectric constants directly, applied it to the respective Clausius-Mossotti expressions of the constituents and of the mixtures. Although in some cases calculated values agreed approximately with those observed, especially when the dielectric constants of both constituents were low, on the whole there were marked discrepancies. Similar tests with the substitution for the Clausius-Mossotti expression of one analogous to that of Gladstone and Dale were not more successful. Phillip's equations have been applied quite recently by Grützmacher<sup>2</sup> to some simple binary mixtures with no better results. Phillip ascribed the apparent irregularities to the effect of association. Thomson<sup>3</sup> and Nernst<sup>4</sup> long ago pointed out, in connection with the ionisation of electrolytes, that the dielectric constant of a solvent will have an influence on the electrical force between the charged particles of a solute; from elementary considerations the greater the dielectric constant the less will be the attraction between oppositely charged particles, and vice versa. In the case now under consideration, where ionisation does not occur, but the molecules of one constituent, say, are polar and are normally associated, the degree of association is bound to be affected by the nature of the other constituent, thus causing a change in the dielectric constant. Thus the effective fraction of the dielectric constant of the mixture due to the associated constituent cannot be calculated from a simple mixture law. A further complication may occur. If the molecules of the other constituent are ever so slightly polar the disposition of their electric fields may be altered by the presence of the other, more polar, molecules or aggregates, with consequent change in polarity, possibly leading to association or to the formation of a loose compound between the two substances. This alteration in the polarity of substances by the presence of other substances is well

<sup>1</sup> *Z. physikal. Chem.*, 1897, **24**, 18.

<sup>2</sup> *Phil. Mag.*, 1893, **36**, 320.

<sup>3</sup> *Z. Physik*, 1924, **28**, 342.

<sup>4</sup> *Z. physikal. Chem.*, 1894, **13**, 531.

known.<sup>1</sup> All we need note here, is that such changes in polarity as mentioned may happen, and one can deduce therefore that the consequent changes in the electric moments may cause changes in the dielectric constants of the constituents, even if no alteration in the state of association is effected. We may remark in parenthesis that the reciprocal effect of polarity as applied above to mixtures may also be applied to the components of compounds. There are many instances of the effect on the basic or acidic properties of a group already present in a compound, caused by the introduction of another polar group; the introduction of this second group thus affects the dielectric constant of the molecule as a whole, and in a manner which one may fairly assume often cannot be even qualitatively predicted. The difficulty of formulating definite rules connecting dielectric constant and chemical constitution may now be better appreciated.

Phillip<sup>2</sup> drew attention to the importance of studying mixtures in which one constituent is non-associated in the pure state and the other is associated. He found that the calculated values of the dielectric constants of alcohols dissolved in benzene appeared to approach limits as the solutions were diluted. The limiting values for methyl, ethyl, and amyl alcohols were 16, 11, and 6, respectively, the dielectric constants of the pure substances being given as 37, 26.5, and 15.45. Frl. Lange, in an interesting paper,<sup>3</sup> gives the results obtained in a study of such mixtures from the point of view of the Debye theory. These results gave quantitative information regarding the polarisation of the molecules concerned, including information as to the association.

From the point of view of the Nernst-Thomson rule we would expect the same solute to be more associated in a solvent of low, than in one of high, dielectric constant. This is generally, but not always, the case. The matter has been studied by, among others, Turner and his co-workers, and Turner's book<sup>4</sup> should be consulted for details.

Before the subject of liquid mixtures is left a noteworthy observation made by Thwing<sup>5</sup> may be recorded. On examining the dielectric constant-composition curves of mixtures of alcohols and water, he found singular points corresponding with the composition of definite hydrates. Similar observations have been made since by Dobrosserdov<sup>6</sup> and by Salazar.<sup>7</sup> We have here then a supplementary method of studying

<sup>1</sup> See, for example, G. N. Lewis, *Jour. Amer. Chem. Soc.*, 1916, **38**, 762.

<sup>2</sup> *Loc. cit.*

<sup>3</sup> *Z. Physik*, 1925, **33**, 169.

<sup>4</sup> *Ibid.*

<sup>5</sup> *Loc. cit.*

<sup>6</sup> *Chem. Abs.*, 1912, **6**, 2350.

<sup>7</sup> *Ann. Soc. Espan. Fis. Quim*, 1924, **22**, 275.

chemical changes in solution and certain "phase-rule" problems.

Solutions of solids in liquids, where the solids are non-electrolytes, or are not ionised in the solvents concerned, must be subject to similar considerations. Fürth<sup>1</sup> has made a suggestive contribution to the theory of this subject; his conclusions can also be applied to mixed liquids. He lays special stress on the effect of the viscosity of solutions on the movement of the dipoles under the influence of an alternating electrical field. "Let us assume in the first place that the dipole moment of the solute, referred to unit weight, is greater than that of the solvent. An increase of concentration will cause at first an increase of the dielectric constant of the solution. However, the viscosity of the solution increases with increasing concentration. . . . This causes a diminution of dielectric constant. Both effects together cause, with increasing concentration, an initial rapid and then a slow rise of dielectric constant to a maximum, followed by an increasingly rapid fall. The position of the maximum depends on the strength of the dipole moments and on the viscosity." This maximum may lie in the hypothetical region of negative, or at nil concentration, in which cases the dielectric constant will decrease as the concentration rises (Type I). If the maximum lies at the saturation point, or in the hypothetical region beyond, the dielectric constant continuously rises with increasing concentration (Type II). In a third type of curve the maximum may lie in the region of positive, but unsaturated, concentration. When the dipole moment of the solute is less than that of the solvent the solution always belongs to Type I. Fürth gives examples, obtained from his own experimental data regarding aqueous solutions, of all three types. Solutions of dextrose, lævulose, and cane sugar belong to Type I, of urea and of glycoll to Type II, and of saccharine to Type III. In connection with the fact that substances with low dielectric constants (*e.g.* urea = 3.5) can considerably increase the already high dielectric constant of water, Fürth remarks that in the solid state the dipoles are arranged in regular lattices (where crystalline substances are concerned), and therefore exert forces on each other, quite different from those in solution. The dipoles in solids are, of course, unable to move, or only slightly, under the influence of an applied electrical field.

*Ionisation and the Dielectric Constant of the Solvent. Dielectric Constants of Conducting Solutions.*—When we turn to solutions containing well-ionised electrolytes the subject becomes rather more intricate. It has been noted already that Nernst and Thomson correlated the dielectric constants of

<sup>1</sup> *Ann. Physik*, 1923, **70**, 63.

liquids with their power of ionising dissolved electrolytes; the Nernst-Thomson rule is subject to exceptions which have been many times pointed out. The dielectric constants of liquid hydrobromic and hydriodic acids are low, and yet these solvents give solutions of high conductance with certain organic acids and alcohols<sup>1</sup>; a number of salts which yield excellent conducting solutions when dissolved in water show comparatively feeble conductance when dissolved in hydrocyanic acid, whose dielectric constant is higher even than that of water.<sup>2</sup> Walden suggests that such exceptions can be explained on purely chemical grounds.

Walden has observed an interesting approximate relationship between the concentrations of an electrolyte, at which the degree of ionisation is the same in different solvents, and the dielectric constants of the solvents, which is expressed by the equation

$$\epsilon \sqrt[3]{c} = \text{const.},$$

where  $c$  is the concentration for any given degree of ionisation. Krüger<sup>3</sup> has obtained the same equation from purely theoretical considerations. A number of reasons for the deviations, sometimes very large, from the constancy required by the equation have been suggested, for which Walden's book<sup>4</sup> and Krüger's paper may be consulted. It may be mentioned here that Walden has obtained, again empirically, a similar equation governing the solubility of salts in different solvents, *i.e.*

$$\sqrt[3]{s} = \text{const.},$$

where  $s$  = solubility, expressed as percentage of dissolved molecules to total molecules. This relation is also approximate, and its application is subject to reservations discussed in Walden's book. One cannot compare, for example, the solubilities of a salt in an associating and in an ionising medium respectively.

There is no doubt that the dielectric constant of the solvent must find a place in any equation dealing with inter-ionic attraction; it is an essential term in the Milner-Hückel-Debye theory. A difficulty occurs here, however: owing to the forces of electrostriction the medium is considerably compressed; hence, as we have seen, its dielectric constant will be influenced, and no dielectric constants have been measured at pressures greater than 800 atmospheres.

The measurement of the specific inductive capacity of

<sup>1</sup> Schaefer and Schlundt, *Jour. Phys. Chem.*, 1909, **13**, 669.

<sup>2</sup> Schlundt, *Jour. Phys. Chem.*, 1901, **5**, 503.

<sup>3</sup> *Z. Elektrochem.*, 1911, **17**, 453.

<sup>4</sup> *Ibid.*

solutions of electrolytes is difficult, owing to their conductance, and there is consequently a paucity of experimental data. Most observers agree that small quantities of ionised electrolytes lower the dielectric constant. Blüh<sup>1</sup> gives a reasonable explanation for this decrease; at the small concentrations employed the electrolytes are completely ionised, and the strong electrical fields due to the ions tend to orient the dipoles and thus to diminish the effect of the applied electrical field. Fürth<sup>2</sup> observed that with increasing concentrations of sodium chloride in aqueous solution the dielectric constant sank until the salt concentration was about 0.005 N; a slow rise of dielectric constant followed until at the highest concentration employed, about 0.017 N, it was approximately that of water. Fürth explains this rise on the assumption that with increase of concentration dipoles of sodium chloride are formed which do not affect the water dipoles as much as do the ions—also the salt dipoles themselves set in the field, thus adding to the total dielectric constant.

An important contribution to this subject has been made by Walden, Ulich, and Werner,<sup>3</sup> who, using improved experimental methods, have been able to obtain a large number of data. Solvents of widely differing dielectric constants were employed, and various salts as solutes. In most cases both the descending and ascending portions of the dielectric constant-concentration curves were obtained, in some cases the latter portion rising far beyond the dielectric constant of the pure solvent.

*Colloidal Solutions.*—Not many investigations have been made here. Errara<sup>4</sup> finds that the great majority of the widely different sols studied by him have the same specific inductive capacity as the pure media. He states that Vanadium pentoxide sols are exceptional, the dielectric constant of a hydrosol, containing 0.3 per cent.  $V_2O_5$ , being 1,280 as against 81 for water; this phenomenon is correlated with the double refraction exhibited by such sols, but Fürth and Blüh (*Kolloid Z.*, 1924, **34**, 259) working with much shorter wave-lengths showed that values of  $\epsilon$  quite near to that of water could be obtained. Keller and Fürth<sup>5</sup> state that certain gold hydrosols have considerably lower dielectric constants than water, but Errara<sup>6</sup> throws doubt on the data. Fürth<sup>7</sup> finds that the

<sup>1</sup> *Z. Physik*, 1924, **25**, 220.

<sup>2</sup> *Physikal. Z.*, 1924, **25**, 676.

<sup>3</sup> *Z. physikal. Chem.*, 1925, **116**, 261; see also Ulich, *Z. Elektrochem.*, 1925, **31**, 413.

<sup>4</sup> Several papers in *Kolloid Zeitschrift*, 1922 and 1923.

<sup>5</sup> *Kolloid Z.*, 1921, **20**, 193.

<sup>6</sup> *Bull. Soc. Chim. Belg.*, 1924, **33**, 422.

<sup>7</sup> *Ann. Physik*, 1923, **70**, 63.

dielectric constants of hydrosols containing 50 per cent. of gelatine and 15 per cent. albumin, respectively, are 44 and 32.8.

It is not surprising that attempts have been made to relate the sign of the charge on the disperse phase with its dielectric constant, and that of the medium, in accordance with Coehn's rule, that when two substances are in contact the one with the higher dielectric constant acquires a positive charge.<sup>1</sup> Hatschek<sup>2</sup> does not appear to believe in the applicability of the above rule to colloidal solutions; he says, for example: "But apart from not in any way explaining the origin of the charge the rule fails to account even for the sign in such cases as the hydroxides. Nothing appears to be known regarding their dielectric constants, but in the absence of such knowledge, it is difficult to believe that stannic acid, which is negative, should have a lower, and ferric or aluminium hydroxide, which are positive, a higher dielectric constant than water." Instances are also given of sols which are positively or negatively charged according to the method of preparation. Errara, however, certainly finds some regularities between the dielectric constants and the coagulating influences of various liquids of different specific inductive capacity from the media of the sols to which they are added.<sup>3</sup> The subject is complex, and should repay further study.

*Electrification caused by Bubbling Gases through Liquids.*—Coehn's rule, just referred to, is obeyed in the data obtained by Coehn and Mozer in their study of the electrical charges acquired by gases when bubbled through liquids. If the liquids were non-conducting, or only slightly conducting, the gases acquired negative charges. The amount of the charge acquired by hydrogen on bubbling through the following liquids was roughly proportional to their dielectric constants: water, nitrobenzene, benzaldehyde, aniline, chloroform, ethyl ether, and benzene. Acetone occupied an exceptional position in the series, but Graffunder<sup>4</sup> finds that the behaviour of acetone is apparently exceptional in other respects also. When the liquids contain small amounts of electrolytes more complicated relations ensue, for which the original paper should be consulted.<sup>5</sup>

Lenard, in one of his researches on "Waterfall Electricity,"<sup>6</sup> could discover no apparent connection between the dielectric constants of various gases and the effect obtained on bubbling

<sup>1</sup> *Ann. Physik*, 1898, **64**, 217.

<sup>2</sup> Introduction to the *Physics and Chemistry of Colloids*, J. and A. Churchill, 1922, 155.

<sup>3</sup> *Kolloid Z.*, 1923, **32**, 240.

<sup>4</sup> *Ann. Physik*, 1923, **70**, 225.

<sup>5</sup> *Ibid.*, 1914, **43**, 1048; see also Bloch, *Ann. Chim. et Phys.*, 1911, **23**, 28.

<sup>6</sup> *Ibid.*, 1915, **47**, 463.

through distilled water, but the effect runs parallel with the densities of the gases. The following illustrative table is from Lenard's paper, the figures being relative in each case :

		H <sub>2</sub>	CH <sub>4</sub>	N <sub>2</sub>	CO	Air	O <sub>2</sub>
Density	. .	0.07	0.56	0.97	0.97	1	1.11
$\epsilon - 1$	. .	0.47	1.63	1.00	1.21	1	0.95
Effect	. .	0.65	0.85	1.00	1.02	1	1.10

No monograph dealing exclusively with specific inductive capacity appears to have been published ; such a publication, with a good bibliography, would be of much service to workers in this field, in view of the large and constantly growing mass of relevant literature.

*Additional Reference.*—Since going to press a useful paper summarising our knowledge of the dielectric constants of solids, liquids, and gases has appeared (Blüh, *Physikal. Z.*, 1926, 27, 226).

# A SUMMARY OF THE RECENT WORK ON THE BIOCHEMISTRY OF THE PNEUMO- COCCUS

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DURING the past few years a group of workers at the Hospital of the Rockefeller Institute has been investigating the Physiology and Biochemistry of the pneumococcus.

This work has already yielded valuable results, and has given important leads which show that chemical methods can profitably be used for the study of certain phases of immunological processes. In view of the fact that many observers are continuing this work, and that the methods are being applied to organisms other than the pneumococcus, it seemed that a summary of the general results might be of value to workers in this country. Accordingly the papers listed in the Bibliography have been summarised under the following headings:

- (1) Studies on the specific substances of the Pneumococcus.
- (2) Studies on the enzymes of the Pneumococcus.
- (3) Studies on the oxidation-reduction system of the Pneumococcus.

## STUDIES ON THE SPECIFIC SUBSTANCES OF THE PNEUMOCOCCUS

Investigations by Dochez and Avery [1] have shown that there is present in cultures of pneumococci in fluid media a substance which precipitates specifically in antipneumococcus serum of the homologous type. Further work by Heidelberger and Avery has shown that two specific substances can be isolated from Types 1, 2, 3 pneumococci—(A) a species specific protein P, [2], (B) a type-specific Polysaccharide S [3, 4, 5]. The species-specific protein P was found to give positive precipitin reactions with immune sera of types 1, 2, 3, but not with Antityphoid or normal horse sera. (2) Antisera against proteins of types 1 and 2 were shown to cross-react in precipitin tests, with the isolated protein of both types [6].

Experiments were performed to prepare antiprotein sera by

inoculation of nucleoprotein P into rabbits [6]. The anti-protein sera did not contain type-specific agglutinins for pneumococci, and did not confer a passive immunity on mice [2]. Pneumococci which had been deprived of all S-producing functions, and simultaneously of the capsule were agglutinable by P antibodies. Intact pneumococci possessing specific antigenic properties, however, were not agglutinated by P antibodies, although they were capable of giving rise to agglutinins for organisms of the homologous type, and to precipitins for the specific carbohydrates derived from them [7]. The protein substance was prepared by precipitation from solutions of pneumococci by acetic acid, and consisted mainly of nucleoproteins and mucoid [2]. A substance later shown to be the type-specific polysaccharide isolated from Type 2 pneumococcus was shown by Cole [8] to be present in the blood and urine of man and experimental animals, and he also pointed out the property of neutralising pneumococcus antibodies possessed by infected exudates and sera containing these soluble reactive substances. Recently Sia has shown that the addition of carbohydrate substances in small amounts has the property of annulling the inhibitory action of sera of naturally resistant animals to pneumococcal infection [9]. The polysaccharide substances appear to act as receptors in Vitro, but not as antigens in Vivo. A carbohydrate antibody, however, was demonstrated by Avery and Morgan [7], and Zinsser and Tamiya [10], in bacterial immune sera, produced by inoculation of intact capsulated organisms. The carbohydrate probably exists in the cell in combination with some other substance which confers antigenic properties upon it [9].

One of the most remarkable features of the specific carbohydrates S is their activity at high dilutions, Types 1 and 3 react at dilutions of 1:6,000,000, whereas Type 2 reacts at dilution of 1:5,000,000 [5]. Type 2 specific substance was shown to be composed of Glucose units [3, 4, 5], and was dextrorotatory, as also was Type 1 specific polysaccharide [5]; it is weakly acidic and contains no nitrogen, whereas Type 1 polysaccharide was shown to have nitrogen content of 5.0 per cent. of the total substance, and 2.5 per cent. of this nitrogen as amino-nitrogen. It is a strong acid and a weak base, and is sparingly soluble in water at its isoelectric point. It yields i-Mucic Acid on oxidation with Nitric Acid. The specific polysaccharide of Type 3 Pneumococcus is nitrogen-free but turns the plane of polarisation to the left. It is a strong acid, and is composed of glucose and either glucuronic acid or some derivative of glucuronic acid. The formation of carbohydrate substances was shown to be most active in pneumococci exhibiting maximal capsular development, exalted virulence

and distinct type specificity. Pneumococci, which had lost the type specificity and had become avirulent, were incapable of producing carbohydrates.

The carbohydrate substances were present in very minute quantities in the organisms, as from 325 litres of 8-day broth cultures only 4.5 grams of specific substance were obtainable.

It is interesting to note that Lancefield [11] obtained positive precipitin reactions using pneumococcus protein solutions Types 1, 2, and 3 against four *Streptococcus viridans* antiprotein sera prepared by inoculation of animals with specific protein substances isolated from *S. viridans*.

Since the work on the specific carbohydrates of the pneumococcus, work has been carried out on other organisms, notably by Zinsser and his associates [12, 41, 42, 43], who showed that from a number of organisms such as Staphylococci, Meningococci, and Friedlander, Typhoid and Tubercle bacilli a non-protein reactive substance may be extracted which bears a distinct relation to the specific carbohydrate. As in the case of the pneumococcus none of these substances were shown to possess antigenic properties. They appear to be analogous to the lipoid substances of Taniguchi [14] which act as receptors in Vitro, but not as antigens in Vivo.

More recently Avery, Heidelberger, and Goebel have demonstrated the presence of a specific polysaccharide in Friedlander's *Bacillus* (Strain E) [15]. Although this polysaccharide precipitates with Type 2 pneumococcus antiserum, the evidence, which consists chiefly of serological findings, especially those which show that the absorption of agglutinins and precipitins in the two organisms is not reciprocal, tends to show that the substances are not absolutely identical [16].

It is of interest to note that Perlzweig and Keefe [17] have isolated a water-soluble antigen from Type 1 pneumococcus broth filtrates. They are at present investigating the chemical nature of this substance, and especially whether it corresponds with the protein obtained from bile solutions of pneumococci by Avery and Heidelberger [2].

#### STUDIES ON THE ENZYMES OF THE PNEUMOCOCCUS [4, 5]

During the course of their studies on the enzymes of the pneumococcus Avery and Cullen were able to show the presence of proteolytic [18], lipolytic [19], bacteriolytic [21], and carbohydrate-splitting [20] enzymes in the bacterial cell. To demonstrate the presence of these enzymes, they either dissolved pneumococci in bile, or allowed them to cytolize in phosphates solutions of pH 6.2, which were alternately cooled in a freezing mixture and thawed at room temperature.

The activity of the proteolytic, bacteriolytic, and lipolytic

enzymes was shown to be unaffected by solution in bile, whereas the action of the carbohydrate-splitting enzymes was completely inhibited by a concentration of bile necessary for solution of the pneumococci.

The proteolytic enzyme was shown to exhibit maximum activity on peptides, and was therefore called a peptonase. It was found to exhibit its maximum activity at a pH of 7.0 to 7.8, which gives a similar range to that of Trypsin and Erepsin. The enzyme was demonstrable in bacteria-free filtrates, only when the growth of the bacteria had proceeded to the stage of cell disintegration. Heating at 100° C. for 10 minutes destroyed the activity of the enzyme, but it was able to withstand a pH of 5.0 for two hours, whereas the living pneumococci were killed rapidly at this concentration. It was shown that pneumococci which were killed rapidly at this concentration were no longer soluble in bile. No measurable effect on the activity of the enzyme was noted, when the virulence of the organisms was attenuated to 1/1,000,000th the original virulence.

The lipolytic enzyme [19] was demonstrated by its power to hydrolyse Tributyrin. It was found that its activity was not increased by solution in bile, whereas most lipases show increased activity. The activity was found to diminish progressively with increasing acidity of the solution until at pH 5.0 the activity entirely ceased. After exposure to pH 5.0 the activity of the lipase could be again demonstrated by neutralising the extract. This showed that the phenomenon of insolubility in bile, of pneumococci which had received similar treatment, was not due to destruction of the enzyme. The activity of the enzyme was also shown to be completely destroyed at a temperature of 70° C. As in the case of the proteolytic enzyme, loss of virulence was found to be unassociated with loss of enzyme activity.

The bacteriolytic enzyme was shown to dissolve completely heat-killed pneumococci, and also to have a distinct but slow solvent effect on *Streptococcus viridans*. The lytic action of the enzyme was evident in the zone of pH 5.0 to pH 8.0, using heat-killed pneumococci Type 2 as substrate. The heat-resistance and activity of the enzyme were shown to be much less pronounced than that of Pyocyanase, for, whereas the bacteriolytic enzyme of the pneumococcus was destroyed by heating to 60° C. for 30 minutes, and only dissolved pneumococci and *Streptococci viridans*, Pyocyanase has been shown to withstand 100° C. for 2 hours and to be capable of causing lysis of such organisms as *B. Diphtheriæ*, *V. Cholera*, *B. Typhosus*, and others [22, 23].

The pneumococcus was also found to contain enzymes

which hydrolysed starch, inulin, and saccharose [20]. The optimum hydrogen ion concentration for the action of the carbohydrate-splitting enzymes was shown to be about 7.0, which agrees with the optimum reaction for the action of the pancreatic amylase.

None of the above enzymes were found to exist as secretory products of the cell. They were found to be present in cultures only when the organisms had reached the phase of disintegration.

#### STUDIES ON THE OXIDATION-REDUCTION SYSTEM OF THE PNEUMOCOCCUS

Macleod and Gordan [24, 25, 26] showed that hydrogen peroxide was produced in cultures of pneumococci, and also that it had an inhibitory effect on the growth of the organisms. The subject was taken up by Avery and Morgan, and they proved that the most favourable conditions for production of peroxide were (1) free access of air; (2) absence of catalase, peroxidase, and other enzymes capable of decomposing this compound [27]. Avery and Morgan were able to show that, although the growth of *Staphylococcus aureus* could not be initiated in peroxide-containing cultures of pneumococci, the inhibition of growth of the pneumococcus itself was due only secondarily to the formation of peroxide, primarily to the exhaustion of the nutritive material in the medium and to the reaction changes [28].

A large amount of work was performed by Avery and Neill, to determine the exact mechanism of the production of peroxide in pneumococcus cultures.

Pneumococci were grown anaerobically and then exposed to air. It was found that the rate of formation and quantity of peroxide formed varied in different strains, and also varied in accordance with the time the cells were kept under anaerobic conditions before exposure to the air [29]. In other experiments [30] sterile extracts of pneumococci were prepared by similar methods to those used in the enzyme work. Sterile extracts of unwashed pneumococci were found to produce peroxide on exposure to air within a reaction range of pH 5.0 to 9.0. The peroxide-forming activity was destroyed by exposure to 65°C. for 5 minutes. This type of extract was also found to be capable of reducing Methylene Blue [31] in the absence of oxygen.

Extracts of washed pneumococci, extracted in phosphate solution, were unable to initiate either of these two reactions without the addition of substances contained in the cell washings, yeast extract, or meat extract. It was shown that the substances contained in the cell extracts were thermolabile,

but the complementary substances contained in meat or yeast extracts were thermostabile. Further work showed that the hæmolytic pneumotoxin described by Cole [32] was inactivated under similar conditions to those necessary for the formation of peroxide and also by hydrogen peroxide [33]. It appears not unlikely that the destruction of pneumotoxin in oxidised cell extracts is due to the formation of peroxide [33].

It was shown by Avery and Neill that the property of oxidation of hæmoglobin to methæmoglobin by broth extracts of unwashed pneumococci was only possessed by extracts prepared from anærobically grown pneumococci [34]. Sterile filtrates of aerobic cultures of pneumococci were found to be capable of converting catalase-free solutions of crystalline oxyhæmoglobin to methæmoglobin in presence of hydrogen peroxide [35]. It was found that the methæmoglobin-producing system depended on a labile constituent of the bacterial cell, if the solution contained catalase. This intracellular substance was found to be susceptible to oxidising agents, and could be rendered inactive by exposure to peroxides before introduction into oxyhæmoglobin solutions [35].

The carbohydrate-splitting enzymes were shown to be, like the pneumotoxin inactivated by the oxidising agents formed when sterile extracts of the cellular substances, were exposed to the air. No effect was produced on the lipase and peptonase [36].

From the above it will be seen that the oxidising and reducing activities of the sterile pneumococcus extracts are determined by the presence or absence of molecular oxygen [37]. Analysis of the processes shows that the active systems consist of two components, one of which is thermolabile and the other thermostabile. The marked oxidation processes which occur when an active extract is exposed to air result in the inactivation of the oxidising and reducing systems. The thermostabile systems are still active in oxidised extracts and the inactivation of the oxidation-reduction systems is due to the destruction of the thermolabile constituent. This cellular substance is also destroyed by molecular oxygen itself.

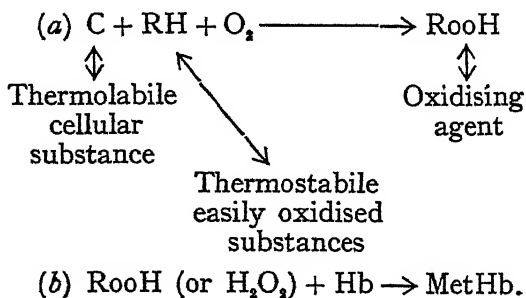
The formula  $C + RH$  has been used by Neill and Avery to designate the system by which peroxide is formed and hæmoglobin oxidised. It is assumed that the cellular substance C serves as a catalyst in accelerating the reactions involved. The fact that the cellular component is inactivated by oxidation is not held as a serious objection to its catalytic nature. It is further assumed that the Oxidation-reduction system may be destroyed by the same reactions which inactivate the hæmotoxin and enzymes, contained in the extracts, as the thermolabile constituent is very susceptible to peroxides.

The order of reactions put forward by Neill and Avery is as follows :

- (1)  $C + RH + O \rightarrow$  oxidising agent.
- (2) Oxidising agent +  $C \rightarrow$  inactive  $C$ .

Here it is inferred that  $C$  functions in equation [1] as a thermolabile cellular catalyst. The work so far performed has resolved the system into two components. (1) The component  $RH$  represents substances which are not found in the pneumococcus after washing. They are heatstable and are present in water or alcohol extracts of muscle, yeast, or vegetable tissue. By themselves these substances react slowly with oxygen to form oxidising agents, and in absence of oxygen they establish conditions by which methylene blue and methæmoglobin are reduced. (2) The thermolabile constituent  $C$  accelerates the reactions of oxidation and reduction to a remarkable extent. This component does not react with molecular oxygen and possesses no reducing powers. It appears to be catalytic in nature.

Further work by Neill [38, 39] on the oxidation-reduction of hæmoglobin and methæmoglobin has shown that the equilibrium between hæmoglobin and methæmoglobin in a mixture of blood and pneumococcus cellular substances, could be shifted in either direction at will, by the regulation of the oxygen tension. According to Neill, the oxidising agents involved in the "spontaneous" methæmoglobin formation are either "activated" oxygen of a peroxide nature or molecular oxygen itself. He suggests that the formation of methæmoglobin by pneumococci may be represented as follows, using the symbols of Neill and Avery.



#### EFFECT OF PLANT TISSUE ON GROWTH

Although it does not come under any of the above headings the work of Avery and Morgan [40] on the effect of plant tissues on growth of the pneumococcus is worthy of mention. They found that the addition of unheated sterile potato

promoted a steady constant growth. Acid production was small, the pH changing from 7.7 to 7.3, whereas in dextrose broth the pH changes from 7.7 to 5.3 at the end of 36 hours. It was shown that once the culture entered the logarithmic phase of growth, the generation time was approximately the same in dextrose broth culture and potato broth culture. Other plant tissues, including Parsnip, Banana, Sweet Potato, Carrot, and Turnip, were found to have a similar effect to that of Potato. It was found that growth could be initiated at pH 6.3 if a piece of unheated vegetable tissue were present in the broth, whereas if none were present a pH between 7.0 and 8.3 was required. A prolongation of the stationary phase of growth was also noted.

In conclusion the writer wishes to express his gratitude to Dr. Whitridge Davis and Professor T. J. Mackie for their very helpful advice and criticism.

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## THE MISLEADING AND PICTURESQUE IN ANATOMY

By PROF. FRASER HARRIS, M.D., D.Sc., F.R.S. (Edin.)

It is rather a curious fact that in a great many cases where the anatomists have departed from purely descriptive terms for structures, they have fallen into error. Where they have assumed the function of a part and given that part a definitely functional name, they have nearly always made a mistake.

A good example of this is the name "gustatory" given to a sensory branch of the inferior maxillary division of the fifth cranial nerve supplying the mucous membrane of the tongue. Now this lingual nerve is not a nerve of taste at all, but one for ordinary sensation or touch.

The true nerve of taste (gustatory) for the anterior two-thirds of the tongue is an afferent branch of the chorda tympani nerve; but the confusion has arisen from the fact that this chorda tympani travels amongst the fibres of the lingual nerve for some distance before it disappears into the Pons Varolii. It was by purely physiological research that this confusion was cleared up.

Physiological research once more cleared up a terminological inexactitude in connection with the ganglia related to the submaxillary and sublingual glands.

The relationship, as made out by the nicotine method introduced by the late Professor Langley of Cambridge, is that the ganglion which innervates the submaxillary gland is a small group of nerve-cells named "Langley's ganglion" situated in the hilum of the gland. The ganglion for the sublingual gland is that already called by anatomists "submaxillary."

Had the anatomists not given the ganglion functionally related to the sublingual gland the name "submaxillary," there would not necessarily have been any confusion.

The vascular system has several examples of terms which are certainly not descriptive of form. The very term "artery" is supposed to be derived from a Greek word *airo, tereo*, "I raise," which was originally applied to the air-containing trachea, and thereafter, in confusion to the larger, stiff-walled blood-vessels, thus perpetuating the pre-Galenical error that

the arteries contain not blood but air or gas, to use a term of later invention.

The term "carotid" is not any better as a descriptive one, since it is derived from a little-used Greek word *karos* meaning "deep sleep"; and is an allusion to the physiological fact that compression of these great cervical arteries will bring about a state of profound unconsciousness as a result of the severe cerebral anæmia.

The word "auricle" may be permitted, since it describes the appearance of "a little ear"; but it is somewhat peculiar to name an entire cavity by a term which refers only to the outward form of an appendix of that organ.

The "moderator band" of the right ventricle is another example of a functional misnomer. The name was given to the band of muscle which crosses the lumen of the right ventricle under the impression that it prevented over-distension of that cavity. We now know that this strand of muscle is part of the terminal distribution of the specialised fibres composing the auriculo-ventricular bundle (of His). The functional guess was once more incorrect.

The "external respiratory nerve" of Bell is another misnomer. The nerve thus named, the posterior or long thoracic nerve, supplies the axillary surface of the *serratus magnus* muscle. This is not a muscle of calm respiration, but is brought into play only occasionally, namely in forced inspiration, so that its nerve is by no means entitled to so specifically functional a designation as "respiratory."

The term "gland," so beloved of anatomists, has been for a long time a source of confusion owing to the variety of structures to which it has been applied. The only satisfactory definition of a gland is "a mass of epithelium differentiated for the purpose either of manufacturing from the blood some substance not found there or for separating from that fluid some waste substance already present in it."

Under this definition, the glands with ducts and an external secretion, and those with no ducts and an internal secretion are included, but nothing else.

The so-called "lymph-glands" are not glands at all under this definition, for they are composed not of epithelium but of lymphocytes, they have no ducts, and they manufacture or secrete nothing. Of late the term "lymph-node" had been substituted for the misleading functional name. To be sure as a term, the word "gland" merely means an acorn (*glans*, *glandis*), which in itself has no functional implication.

The group of glands, anatomically speaking, was large and heterogeneous: it included things so different from one another as the Thymus body, the Pineal body, the Pituitary, the patches of Peyer, the Pacchionian "glands," and the

lymph-nodes, besides all the true glands such as the liver, pancreas, the glands of Lieberkühn and such glands of internal secretion as the supra-renal and the thyroid.

No actual harm is done when the name, instead of having any functional significance, is merely picturesque, as, for instance, the Pomum Adami, or Adam's Apple. This term innocently perpetuates the popular mistake that it was an apple which in the Biblical story Eve offered to Adam. The word "apple" is not once mentioned in the account of the transaction in Genesis. A later "apple of discord" may have contributed to the error.

Other fanciful names such as the snuff-box of Botallus, the tendo Achillis, the crista galli, the mons Veneris, the nymphæ, the hymen, the verumontanum, the malleus, the incus, and the stapes were evidently given to these structures to introduce the element of the picturesque into an otherwise dull and uninteresting morphological recital.

Nor was the term "auditory" as applied to the whole of the eighth cranial nerve much more happy than any of the other functional names. For physiological research has made it clear that only a portion of that nerve is really auditory in function. The part that has to do with hearing arises in the cochlea; and, after establishing some complicated relationships in the medulla, makes for the temporo-sphenoidal lobe of the cerebrum. This is the acoustic nerve. The rest of the nerve called "auditory" consists of fibres which have arisen in the membranous labyrinth and in the semicircular canals of the internal ear, and, after passing through the medulla, end in the cerebellum.

Those fibres have nothing to do with hearing; they are related to the sense of orientation in space, and are concerned with the rather vague, sensory impressions which lead to the maintenance of the static and to the dynamic equilibrium.

In other words, all the fibres of the eighth nerve arise in the internal ear, but only a portion of them are acoustic in function. A merely descriptive name for the whole of the eighth nerve has yet to be found: the "otic" nerve would do, but "auditory" is a misnomer. Reminiscent of a similar misconception about their function is the naming of the maculæ and cristæ in the labyrinth of the internal ear as "acusticæ."

The "optic thalamus" is another misleading name for this great basal ganglion, because it is only in fishes and reptiles that its exclusive function is "optic." Indeed in mammals the optic as distinct from the visual ganglia are the anterior *corpora quadrigemina*.

In the descriptive anatomy of the alimentary canal a few inaccuracies are to be found: there is no pyloric "valve" at the gastro-duodenal junction; the "valvulæ conniventes"

are not valves, nor is the jejunum habitually empty: the lacteals convey a milky fluid only under certain conditions of absorption, yet by a name indicating only an occasional function are these lymphatic vessels known.

The term "sympathetic" for the system of nerves now known by the physiological name of "autonomic" is peculiarly badly adapted for the purposes of descriptive anatomy, seeing that, instead of indicating anything in morphology, it perpetuates an error in physiology.

The "sympathies" of the old writers are what we should nowadays call reflex actions and referred sensations. Now reflex actions are the very things which it is believed do not take place within the limits of the sympathetic system *per se*.

True reflexes involve centres in the central nervous system; there is but one absolutely certain peripheral reflex—the enteric reflex of Bayliss and Starling, the one that involves the plexus of Auerbach lying between the two muscular coats of the intestine.

Lastly, perhaps the best example of a misnomer is the "Os sacrum," a term not only not descriptive but one which perpetuates a very curious Rabbinical superstition. "Sacrum" is an adjective, the neutral form of the Latin *sacer*, sacred; so that merely to call the bone the sacrum is to apply to it an unrelated adjective. This bone was supposed to be the only one not to undergo decomposition but to remain as a nucleus for the reconstituted body at the resurrection. We shall not inquire how the cremated martyrs were to be provided with bodies; but perhaps, as it was the theologians who burned them, they purposely had made no provision for their re-incarnation.

Samuel Butler, in *Hudibras* (Canto II, Part III), gives the following account of how the bone came by its name:

"The learned Rabbins of the Jews  
Write there's a bone which they call Luez  
I' the rump of man, of such a virtue  
No force in Nature can do hurt to,  
And therefore at the last Great Day  
All other members shall, they say,  
Spring out of this as from a seed  
All sorts of vegetals proceed;  
From whence the learned sons of Art  
'Os sacrum' justly style that part."

## POPULAR SCIENCE

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### THE BUSHMEN OF THE KALAHARI

By PROF. E. H. L. SCHWARZ,

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My recent journey through the northern Kalahari has enabled me to supplement my studies of Bushmen, and I can now recognise ten separate types. There has been such a mixing up, both racially and linguistically, that it is doubtful whether a satisfactory scientific classification will ever be made, but it is worth while putting on record what remains of the distinctive characters of this people, before they become absorbed in the general welter of the coloured race.

The vast majority of the natives in South Africa are Bantu, a name used by Dr. Bleek to denote certain negroid tribes speaking a language characterised by a system of concord prefixes. The earliest records of these are obtained from the genealogical trees of the Bechuanas, which go back to A.D. 1270. They came from the north, finding the land already occupied by the Hottentots on the west and south coasts, and the Bushmen in the interior. The former speak a sex-denoting language allied to Masai and other Hamitic tongues, while the latter use an isolating one, the words consisting of clicks, snores and warbles in the throat, originating probably from a time before the human tongue acquired the nicer adjustments necessary for articulate speech. Now it appears that the people whom we have loosely called Bushmen are themselves late-comers in South Africa, and we find a number of tribes in the Kalahari, racially distinct, and evidently of more ancient origin. In the Bushmen texts given by Dr. Bleek there are constant references to these men of an earlier race, who are invariably referred to as giants. It is now proved that the tall aborigines still exist in the Makarikari. The following is an attempt to sort out these pre-Bantu races.

1. The term "Bushmen" is usually applied to the pygmy race that used to live in Cape Colony, and drew extensively on the walls of rock shelters. They were called San, or Sanquas, but they employed the word *Qhwaikhoë* themselves—that is, People of the *Qhuai*, or apron of leather thongs, similar to the

rahat of the Sudanese natives. They are under 5 feet in stature, typically 4 foot 6 for the men and 3 inches lower for the women. Their skulls have a peculiar quadrangular shape with a deep notch at the base of the nose. Their ears are square, with no lobes. The hair is twisted tightly into knots, each tuft growing separate from the others. Exceedingly few of these people remain, but occasionally a family of them may be met with, when they are at once recognised by the corpse-like, whity-brown colour; there are, however, plenty of half-breeds of this nation, scattered throughout Namaqualand, the Transvaal, and the southern Kalahari.

2. The Bakalahadi are the true aborigines of the Kalahadi, or desert, being regarded as inferior beings by the lowest Bushman. They vary very greatly in character, owing to admixture of both San and Bantu blood; but typically they are a slender, dwarf race, black, and with hair growing in a spiky mop, standing out some 4 inches from the scalp. Their affinity is with the long-haired Doko of Lake Baringo and the Shoa Highlands of Abyssinia, and thus with the long-haired pre-Dravidian races of Asia—the Kurumbas and Irulas of the Nilgiri Hills, the Koro-puk-guru of Japan, Aetas of Luzon, and so forth. They once possessed cattle and cultivated the ground, but they have let themselves be despoiled of everything, and now are the most hopelessly abject race in Africa.

3. The Masarwa of the Makarikari, to whom the name should be restricted, are a heavily built race of yellow men, of medium to tall stature. The word Masarwa is loosely applied to any Bushmen, but the true Sechuana term is Baroa for the people in general. The forehead is wrinkled into formidable furrows, even in the young men; there are enormous circular bony orbits, and a very long upper part to the mouth. The Gibraltar skull fits their physiognomy, except for the forehead, which has become pushed up either by evolution from within or admixture from without, so they are probably related to the Rhodesia (Broken Hill) man.

4. The Qung are a numerous race of red-brown men, slightly taller than the San, from whom they differ also in the hair, which grows in a continuous mat, and in the ear, which is elongated, with no lobes, the lower margin fusing with the cheek. They speak a language with only four clicks, against the five of the San, and men of the two races can hardly understand each other.

5. The Heikum are a lumpy, black race, considered by some to be crosses between the last and the blue-black Berg Damara, a primitive Negro race that came down from the West Coast in very early times. They live in orderly communities, and are on a slightly higher plane than the Qung.

The most outstanding feature about them is the appalling vacuousness of their expression.

6. The Tannekhoë, or River Bushmen, are black, slender folk, living by fishing in the Chobe and Okavango swamps and in Angola. They do a bit of gardening, raising especially tobacco, and possess goats. Their language, again, is quite separate from that of any of the foregoing.

7. The Batete, or Tletlekhoë, are slender black people, allied to the last, but very much more advanced. They have possessed cattle from the earliest times, and till the ground extensively, storing the grain ; there is no Bantu blood among them. They are confined to the shores of Lake Kumadow in the Makarikari.

8. The Madenassenas are true Bushmen, though many are as much as 6 feet 3 in height. They are black, with long hair, and use the spear as a weapon instead of the bow and arrow. They live in the Makarikari. There is a family of these at Litoane, characterised by enormously long front teeth in the upper jaw, who may belong to a separate type.

9. The Maxganikhoë are slaves of the River Bushmen, but they are not known sufficiently.

10. The Bongo Bushmen are small, stocky fellows, ashen grey in colour, living on the Okavango just above the marshes.

Hassanein Bey has published a photograph of a rock shelter in the oasis of Owenat, showing a San Bushman shooting an ostrich, so similar to thousands of similar pictures in Cape Colony that there can be very little doubt that men of the same race drew the Sudanese ones. Newbold has brought to light many others from farther south, while Father Bagshaw, S.J., has tracings of the same kind from Lake Tanganyika. The Rhodesian ones are well known, so there is a continuous chain of these from the latitude of Wady Halfa to Cape Point. Sir Harry Johnston has recorded the click language from Victoria Nyanza, while Ludovico Varthema, in the sixteenth century, tells us that it was also spoken at Sofala ; he says the sounds remind one of the language the muleteers in Sicily use to their animals when urging them on.

The Bushman shooting an ostrich at Owenat explains the classical story of the battle between the dwarfs and the cranes. The word "geranos" was the nearest the Greeks could get for an ostrich, while the terrible din this bird made, as mentioned by Homer, is the deep booming noise, likened by Dr. Livingstone to the roaring of lions ; many people, not knowing what it was, have been frightened out of their lives by the weird sound. We can, however, date the occupation of Owenat more closely.

Wallis Budge, in his memoir on the Sudan, tells the story

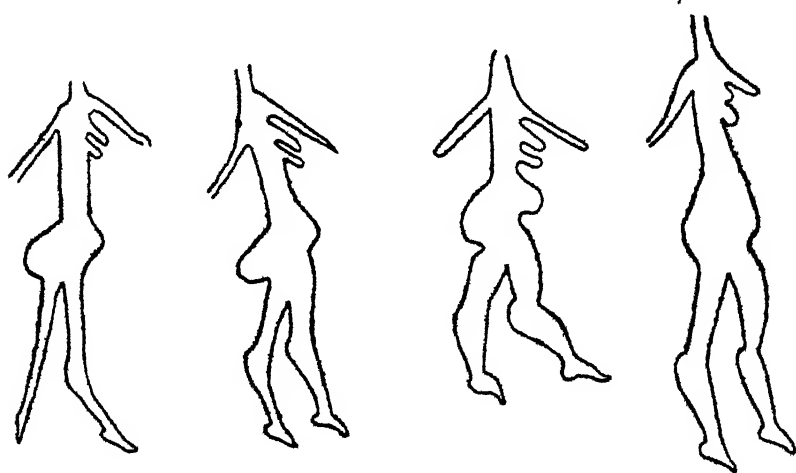
of the adventures of Her-khuf, governor of Elephantine, near Aswan, in the time of Pepi II, of the sixth dynasty, somewhere about 3,000 B.C. He went to an oasis corresponding in position to this very one, which he calls Uhat, and brought back with



SHOOTING AN ELAND.

Notice the end of the arrow sticking in, and the loose shaft falling away.

him a "tenk," or dwarf. Even from the first dynasty, these little people were in great demand; on the death of the king they were sacrificed, and their bodies buried with that of the king, in order that their shades should accompany his to the



THE GAO-O, OR GEBBI-GU, OR WOMEN'S DANCE.

(From a Bushman Rockshelter in the Cederbergen, Cape of Good Hope.)

Other World. When they appeared before the dread Osiris they were supposed to distract the attention of the deity by their dancing, and thus allow their royal master to pass on without too minute an inquiry into his sins. In later times,

the kings themselves endeavoured to learn the steps of the Bushman dance.

These same dances are still practised by the Bushmen ; they are not confined to any one race, but among the San there is the ceremonial one called the " khu," which is probably much the same now as when it so intrigued the Egyptians. It lasts all night, with the women sitting round clapping their hands and singing ; they have particularly soft, pleasant voices. The women's dance is the " gao-o," or " gebbi-gu," and is danced on any joyous occasion, such as the slaying of an eland (see figures 1 and 2). In every group of Bushmen there is one who is a born fool, or natural buffoon, and it is remarkable that this spirit of mirth and gaiety should have persisted when they have been hunted like baboons by every nation in Africa for five thousand years, and have lived constantly in a state of semi-starvation.

It is impossible to estimate the time they took to travel through Central Africa, but it does not appear probable that the San arrived in the Karroo much before the thirteenth century. Here they found an ideal country where they could live undisturbed, as, on account of the want of water, neither pastoralists nor agriculturists could exist there. They multiplied exceedingly, and as late as a hundred and fifty years ago there was a town of 1,133 huts between Aliwal North and Herschel, and others all over the country. Then came the Trek Boer with his art of making dams, so that water could be stored between the rains. Hundreds of the little people were killed annually, but the toll they took on the white man and his flocks and herds was enormous ; gradually, however, the Bushmen were exterminated, and would have disappeared entirely like the Tasmanian aborigines—people very like our Bushmen—had there not been the Kalahari, where even the Bantu did not want to live.

This area is the only one left in the whole land-mass of the three continents, Europe, Asia, Africa, where the wild hunter races of prehistoric times could find a means of livelihood, and where they could live without molestation. In the Madenassena and San we have a giant and a dwarf race, and in the Masarwa one resembling in some respects the Neanderthal. That these should actually be the descendants of the Magdalenians, and so on, would be less surprising than the persistence of the Pliocene giant sloth into our own times.

## NOTES

### On the writing-up of Scientific Papers (E. Pickworth Farrow, M.A., D.Sc.)

It may not be always well realised how very advisable it is, from a subsequent labour-saving point of view, that the notes in the notebook should be *written on one side of the paper only* during the progress of any research work.

The great point of this is that, when the results of the research come to be written up in the form of a scientific paper or book, the corresponding pages of the notebook can be cut up so that each separate fact, paragraph, or sentence appears on a separate "note-strip."

These latter can then be sorted out as much as one desires. At the end of a long research it may be found that several of these note-strips practically duplicate one another. All of these can then be destroyed except one—after entering up any separate facts from the others upon the one strip which is left. This, in the first place, thus appreciably reduces the mass of notes to be dealt with subsequently.

The note-strips can then be carefully sorted and arranged in little heaps upon one's desk, each heap upon a separate sheet of white paper, labelled, for example: "Introduction," "Chapter I. General Description of Breckland and its Vegetation," "Chapter II. Factors relating to the Relative Distributions of *Calluna* Heath and Grass Heath in Breckland," . . . "Chapter X. Concluding remarks upon Breckland and upon Ecological Research in General," etc.

At first one may not be able to decide what will be the best order for the material dealt with in the various heaps to appear in the final publication, but a little contemplation will usually enable one to decide upon the best order. When this has been decided the note-strips should be gone over again, for it may frequently happen that the material dealt with on a particular strip requires to be dealt with in several of the chapters or sections. In these instances transfer each of these particular strips to the pile corresponding with the first one of the chapters for which it is required. Then fold each one of the white sheets

upon which the individual piles lie into the form of a folder, label the outside of this, and shake all the note-strips down into the bottom of the groove.

We now come to the writing-up of the "Introduction." Instead of having to wade through all the note-books for the material for this amongst a lot of dead and irrelevant material in the complete notebooks we only have a very few note-strips in a folder. The best order of dealing with these is readily decided upon and the material in the "Introduction" is written up and elaborated as required.

Some of these "Introduction" note-strips may also be required for several of the later chapters, and each of these particular strips is transferred to the folder of the first one of these later chapters, after it has been finished with for the Introduction, being subsequently transferred to the folders for later and later chapters as the earlier chapters are written up.

Some of the longer and more complicated chapters may need to be subdivided for convenience in the same manner as the whole book is subdivided.

In these cases place smaller subsidiary folders, labelled appropriately, inside the larger chapter folders, treating these smaller subdivisions in exactly the same manner as the chapters of the book itself are being treated, the forward movement of subsequently required note-strips being of exactly the same nature. Such subdivision of long and complicated chapters renders them far more easy to write up.

After the note-strips have been finished with they are (less the ones which have been placed in later folders) transferred from a spare folder back into the original folders and placed under spring-clips again in case they may be required for reference later on.

It will probably be found that there are a number of note-strips which are of no use for the chapter to which they were temporarily allotted nor for any of the subsequent chapters, or that the points referred to in them cannot be conveniently worked in. These are transferred to a separate folder marked "Note-strips: Useless but saved for reference." In addition to this latter category, there will be some strips which contain suggestions suitable for future research, and these are transferred to another folder labelled "Suggestions for future Research." At the finish, after the book is written up, one has the latter valuable folder containing the suggestions for future work in a very handy and collected form.

It is not suggested that the method as described above is new. In fact it is so very useful that it must surely have been thought of and developed independently by many people in

the same way that the present writer thought of it.<sup>1</sup> However, it is such a great trouble-saving method that it seems worth while to describe it for the benefit of any people who have not thought of it. It will be noted that it represents a direct attack upon the task of writing up the contents of only one single note-strip, separated from the others, at a time, which is a comparatively easy matter. One can concentrate clearly and effectively upon writing up the material on the one strip, everything else being out of the way for the time being. This is an enormous help. Afterwards this particular strip is itself placed out of the way, and the sorted-out next one receives attention instead of one having to make a continued frontal attack upon the complete pages of the notebook.

One practical hint derived from experience—never leave two notes joined together just because one feels sure that they will both be required in the same chapter or section of a paper, but clip them up at the start. This facilitates the subsequent handling in many ways—particularly in the matters of discarding and transferring.

It may be gathered from the above notes that the writer's book *Plant Life on East Anglian Heaths* was written up by the method described. The writer has published a considerable number of original papers during the past year or so in various journals and considers that he would not have been able to get them written up in the time available without the advantage of the "flank attack" upon the writing-up which this method provides.

<sup>1</sup> For example, since completing the above article the writer has read the late Sir T. Clifford Allbutt's *Notes on the Composition of Scientific Papers* (Macmillan). This book (on p. 11) mentions that notes should be written on slips of paper about the size of cheques, and then deals with the truth that these slips can be sorted, arranged and discarded. It does not point out, however, or deal with, the great advantage of a "flank attack" which similar methods provide, and the method of handling the "cheques" is not described nearly so fully as in the above notes.

Allbutt recommended that the slips be secured by paper clips and be kept in drawers, whereas thick paper folders (which can, if necessary, be stood vertically in boxes or files) are probably in every way preferable. They take up far less room and the titles can be written on the tops of the folders so that any folder can be withdrawn without disturbing the others. It may well be considered that British scientists frequently do not employ vertical filing nearly so extensively as they might advantageously do. Apart from this the present writer does not see why such large slips of paper should be employed—they are unnecessary when the slips are placed in folders and must frequently entail waste of paper.

The above book gives many valuable hints concerning style and the meanings of words, such as the differences between "theory" and "hypothesis"; "fact" and "truth," etc. It nevertheless itself contains, on p. 15 of the 1904 edition, a bad unrelated participle which shocks the reader's sight.

**The Encouragement of Discovery (Sir R. Ross).**

Twenty-five pages of SCIENCE PROGRESS have already been expended on discussions of this subject by me (October 1924, January 1925, January 1926), and our readers are doubtless as wearied of it as I am ; but the theme is so important that I propose to pursue it as often as necessary. My thesis has always been that the best way to encourage discovery is to pay for it when achieved. This seems to be a surprising conclusion to some persons ; but I was glad to see that even so wild an idea has appealed to those leading journals, *The Manchester Guardian*, of January 2, and *The Pioneer* (Allahabad), of January 30 last. The former admits that "The common sense of the plea is unanswerable, and the British Government that follows the excellent lead given by Canada in this matter when insulin was discovered would have general support." The latter states that "In practice, this theory [the discovery-for-nothing theory] besides being unjust, naturally chains to routine work many whose ability might contribute notably to the welfare of the race." On the other hand, *The Lancet* of January 30 is more dubious. It begins by admitting that "In theory the proposal is so sound that it is difficult to find anything to say against it. If anyone would discover the cause of the common cold or of muscular rheumatism to be such that the knowledge gave us the means of preventing these appalling diseases, it is not extravagant to think or even to say that the State should give the discoverer anything he wanted, at any rate up to the level of dukedoms, palaces, and princely fortunes. And the man who found out how to prevent a disease such as pernicious anæmia, which by comparison produces a negligible amount of human suffering, might well have £1,000 a year for life or something of that order. The principle is sound enough ; the difficulties arise only in its application. In the first place, who is it that discovers anything ? Discovery is a process, not an event."

We have seen this old boggy of an argument before ! I engage to make a list of about a dozen men now living who have discovered things worth at least £1,000 a year to the public ; and, what is more, I will engage that their names would be accepted for such or for higher rewards by any competent committee appointed to make a selection. Their discoveries may be links in a chain, as *The Lancet* goes on to argue, but they are not the less important for that, and sometimes the whole chain consists of only one or two, or two or three, links. Who is it that discovers anything ? Why, when we come to think of it, every year the Royal Society admits about fifteen new fellows and allots many medals, precisely for discoveries—

though these may be only links in the numerous chains of science ; and every year dozens of other societies, and of academies and universities do the same. Every year most governments bestow public honours for various kinds of meritorious work, including scientific work. I presume that even *The Lancet* will admit that the selections are sometimes sound ; and if selections for honours are sound why cannot selections for rewards be sound ? Then, if we think hard, we will remember that every year the Nobel Committee actually does bestow pecuniary rewards precisely for discoveries ; and the whole of my case is that other nations should do the same, instead of leaving a single nation like Sweden to recompense their benefactors. Really, this alleged difficulty of selection is a preposterous argument which will not bear a moment's critical examination.

But *The Lancet* lodges another similar objection. It considers that I am "almost wholly wrong in thinking that such a system of rewards would 'encourage' medical discovery. The synthetic idea, which is nearer the common notion of original discovery than anything else, generally comes by way of revelation," and "it seems scarcely reasonable for anyone to embark on a search for a revelation because it may be worth £10,000." That the highly respected and respectable *Lancet* should actually take a leaf out of the great Book of Quackery seems to be hardly credible or creditable ! Scientific discoveries are not made by revelation, but by years of hard reasoning and laborious investigation ; and it is on this account that I advocate some form of pecuniary reward, or rather compensation, in connection with them. A leading horse-breeder tells me that this country is now spending about £350,000 a year on racing cups and "purses" for encouraging horse-breeding and sport, and that he himself sold a single colt for £12,000. Such sums may appear insignificant to *The Lancet* ; but when our people learn to pay as much for discoveries as they now do for race-horses they will receive still better value for their money. At present they usually treat their few medical "discoverers" so vilely that one wonders how any sensible man can be fool enough ever to touch such a subject as medical research, except on points which directly enhance private practice or lead to professorial or other salaries. As a matter of fact, very few do ; and the public continue to die by millions in consequence of diseases which will probably be found some day to have been easily curable or preventable. This is the price we pay for *not* rewarding discovery.

Yet another article, by J. B. S. Haldane, appeared in *Discovery* for last February ; but I find some difficulty, even after several trials, in understanding the author's exact views

on the question. He approves the Nobel Prize to Banting and Macleod for insulin, but evidently thinks, as others have done, that the preliminary researches of previous workers were equally deserving. Then why have they not also been rewarded?—and the argument merely adds to mine that the Nobel Prizes should be supplemented by other nations. At the same time one asks why did these earlier workers leave it to Banting and Macleod to complete their work? See also a letter by E. H. Starling in *Nature*, April 26, 1924.

Neither of the writers in *The Lancet* and in *Discovery* seems to have read my three papers with any attention. In *Nature* of April 17, page 554, Prof. H. E. Armstrong writes: "The bump of judgment appears to be lacking in the modern school of chemistry—the judicial sense is nowhere to be found. Seemingly, any wild-cat speculation may be indulged in upon paper." Not only in chemistry, I fear. Any statement of fact, any conclusion of opinion, is good enough—down they go on paper and out they come in print, as if science were politics, and as if the lives of thousands and the welfare of whole countries were not in the least concerned in the result!

#### Science Schools (R. R.)

Since the death of Sanderson of Oundle apparently few schools which make a speciality of teaching chiefly from the scientific standpoint exist in this country. One of these is St. Pirans', Maidenhead (Mr. V. Seymour-Bryant). Even this school is not supported by the public as much as it ought to be. The ideals of the British parent appear to be that their sons should always be young gentlemen who wear Eton jackets, top hats, and white gloves and spats. The outside of the head is more important than the inside. To play games is more important than to work hard. To belong to high life is more important than to live a high life. Surely the response to Sanderson's effort was so good that one would think a number of private schools in this country would have been formed with the same views. Mr. Bryant contributed an article on the subject to *SCIENCE PROGRESS* of July 1925, page 138, which see.

#### Oyster-breeding Experiments

Experiments to ascertain the conditions governing the production and settlement of oyster-spat have been carried on since 1918 at the Ministry of Agriculture and Fisheries' Shell-fish Research Station at Conway, and in 1925 the Ministry's mussel purification tanks at Lympstone were also used for this purpose during the off-season for mussel cleansing.

For the collection of the spat large numbers of limed tiles

are placed in the tanks together with ripe or spawning oysters. Under favourable conditions the tiny oysters settle on these tiles in large numbers and start to grow. When they have reached a suitable size they can be detached from the tiles and placed on the oyster-beds to complete their growth.

It is known that spawning depends to a considerable extent upon temperature, but the conditions under which oyster-spat settles are largely unknown. The effects of different temperatures, degrees of salinity, enrichment of the water, aeration, etc., have been carefully studied. A feeding experiment with ordinary yeast carried on this year met with some success. The young oysters were able to swallow the yeast-cells—which, of course, have a high nutritive value—and in one tank so treated at Conway a useful spatfall was secured. At Lympstone a very heavy spatfall was obtained, as many as 150 oysters settling on a tile. Two special features were noted in connection with the tank where the heaviest spatfall occurred: firstly, that there was a very large population of small fish (gobies) which were absent from other tanks; and, secondly, the presence of a minute vegetable organism. The intestines of the oysters were found to be full of this green material, almost to the exclusion of other organisms. Cultures have been made, and further experiments with this organism will also be carried on next season.

As regards the gobies, it is quite possible that they may have kept the water clear of certain organisms which compete with the young oysters for food. Such competitors are numerous; *e.g.* small crustaceans (copepods), the young stages of gnats and other flies, etc., which usually occur in the tanks in large numbers. Some hundreds of the gobies have been transferred to one of the tanks at Conway, and it is hoped that they may breed sufficiently to allow of experiments being carried out next year.

Another remarkable feature was the phenomenally rapid growth of the Lympstone oysters, which reached a diameter of two inches in less than two months—a rate of growth outside all previous experience.

The spat has been laid down in the Exe Estuary, and in the Menai Straits; the mortality has been negligible. Some of the spat has also been laid down by a large firm of oyster planters at Brightlingsea.

### Coal Survey

An important aspect of the work on fuel research undertaken by the Department of Scientific and Industrial Research is a survey and classification of the coal seams in the various mining

districts by means of physical and chemical tests in the laboratory, supplemented where desirable by large-scale tests at H.M. Research Station, East Greenwich, or elsewhere. The Fuel Research Board of the Department consider that the best way to carry out this work is by means of local committees representing the local colliery owners and managers, the local branch of the Institution of Mining Engineers, the Fuel Research Board, and the Geological Survey of Great Britain, as well as outside scientific interests. Each committee is charged with the duty of supervising the work of the physical and chemical survey in a coal-mining area ; and in this way the survey becomes of practical value from the commencement, since local knowledge and experience are made available, and the seams to be investigated and the general programme of work are decided by those who are able to estimate most correctly the relative importance of the problems to be solved. The seams selected undergo physical and chemical examination by local investigators appointed for the purpose, after which a final selection is made of those seams likely to justify experiments on a technical scale in order to test their suitability for particular uses or methods of treatment. Committees have for some time been actively at work in the Lancashire and Cheshire and in the South Yorkshire areas, while similar work has just been started in the North Staffordshire area and in Scotland.

### **The British Universities Year-book, 1926**

This year-book, which was first published in 1914, summarises the matters of general interest contained in the 67 separate Calendars of the Universities of the British Empire. It is divided into sections, each devoted to one of the constituent countries, namely, Great Britain and Ireland, Canada, Australia, New Zealand, South Africa, and India. Each section contains (a) a directory of the officers and members of the staffs of the several universities, (b) an account of their equipment, degrees, fees, scholarships, etc., and (c) an account of the events of outstanding interest in each college during the past academic year. There are also twenty-seven appendices dealing with such matters as matriculation examinations, qualifications for entrance to professions suitably preceded by a university career, centres of research in various special subjects, etc. The information supplied may be regarded as official and authoritative, and the book probably contains an answer to any pertinent question which might arise in connection with the work or facilities of any university or college of university rank in the Empire.

The book is edited by the Universities Bureau of the Empire

at its headquarters, 50 Russell Square, London, W.C., where members of the universities of the Overseas Empire will always receive a welcome and any assistance the Bureau can give. The publishers are G. Bell and Sons, Ltd., London, and the price 7s. 6d. net.

### Notes and News

The French Government has conferred the title of Commander of the Legion of Honour on Sir James G. Frazer, O.M.

The Council of the Royal Geographical Society has, with the approval of H.M. the King, awarded its Founder's Medal to Lt.-Col. E. F. Norton, for his work as leader of the Mt. Everest Expedition of 1924, and the Patron's Medal to Sir Edgeworth David for his work on the Funafuti atoll and with Sir Ernest Shackleton's Expedition, 1907-9.

Sir John Rose Bradford, formerly professor of medicine at University College Hospital, Medical School, London, has been elected President of the Royal College of Physicians.

The Bessemer Gold Medal of the Iron and Steel Institute has been awarded to Sir Hugh Bell.

Prof. E. G. Coker has been awarded the Louis Award Levy Medal of the Franklin Institute of America for his work on photoelasticity, and Mr. F. Twyman the John Price Wetherill Medal for his work on the design of interferometers.

The Prince of Wales will deliver his Presidential Address to the British Association at Oxford on August 4. The meeting of the Association extends over the period August 4-11. Among other addresses will be those by Prof. A. Fowler (on the production and analysis of spectra), Prof. Thorpe (on the scope of organic chemistry), Prof. J. Graham Kerr (on biology and the training of the citizen), and Sir Daniel Hall (on the area of cultivation needed to feed a population). Prof. Eddington will deliver one of the evening discourses.

Sir Arthur Keith has been nominated by the Council as President of the Association for the meeting in Leeds in 1927.

We regret to have noted the announcements of the death of the following well-known men of science during the past quarter: Prof. D. S. Capper, Professor of Engineering, King's College, London; Prof. A. R. Cushny, Professor of Materia Medica and Pharmacology, University of Edinburgh; Prof. F. Y. Edgeworth, Emeritus Professor of Political Economy in the University of Oxford; Dr. E. Ehlers, zoologist; Dr. S. Exner, physiologist; Prof. J. F. Gemmill, zoologist; Prof. L. G. Gouy, mathematical physicist of the University of Lyons; Dr. W. E. Haworth, entomologist; Prof. H. Kamerlingh-Onnes, For.Mem.R.S.; Prof. W. J. Lewis, F.R.S., Professor of Mineralogy in the University of Cambridge;

Mr. A. R. McCulloch, zoologist ; Admiral Sir John Franklin Parry, formerly Hydrographer of the Navy ; Sir Philip Watts, lately Director of Naval Construction ; Prof. A. Witz, of the Catholic University of Lille, known for his work on thermodynamics and internal combustion engines.

In a letter to *Nature* (April 24), Dr. J. J. Manley has given further details concerning the mercury helide whose formation he first announced last year. He states that it is formed most readily at a pressure of 8 mm. of mercury : that it is stable at ordinary temperatures, but decomposed by passage over a red hot platinum spiral and that it is neither liquefied nor absorbed by charcoal at the temperature of liquid air. Its formula is considered to be  $\text{HgHe}_{10}$ . A second helide has also been discovered having a density corresponding to the formula  $\text{HgHe}$ . It will, of course, be understood that the quantities of these substances available for experiment are extremely small, being of the order of 0.0003 gm. only.

In the Twenty-seventh Robert Boyle Lecture, delivered at Oxford, in June 1925, Prof. Joly gave an account of his latest conclusions regarding the geologic age of the earth. The estimate of this age generally accepted to-day is based on the lead content of uranium-bearing minerals, together with a knowledge of the rate at which the transformation uranium  $\rightarrow$  lead takes place. Its accuracy clearly depends on this transformation rate having remained constant during the period involved. Prof. Joly claims, however, that similar calculations based on the lead content of thorium-bearing minerals and on the rate of decay of thorium lead to an estimate of age only a fourth or fifth as great as that deduced from the uranium data, and he states that it has been apparent for some time that the rate of transformation of one (at least) of these elements has probably changed in the geological period. He considers that the radioactive haloes provide evidence on this question, indicating that the rate of decay of the radioactive material in uranium rocks in the Archean and Devonian epochs was certainly much greater than it is to-day, but giving no suggestion of any variation in the rate of decay of thorium. He therefore concludes that the estimates based on the lead content of thorium-bearing minerals are less unrelleable than those given by rocks containing uranium.

Prof. Joly suggests that the three most reliable methods of estimating the duration of the geological periods are (a) those based on the salinity of the ocean, which give from 100-175 millions of years, (b) those based on isostasy and the theory of "world revolutions," which require from 160-240 millions of years, and (c) those discussed above, which depend on the lead content of thorium rocks and which indicate a period of

the order of 250 million years. It should be added that certain minute and unidentifiable haloes found in ancient Arendal and Ytterby micas indicate long periods preceding the Archean epoch.

The lecture has been printed by the Oxford University Press, and may be obtained from Humphrey Milford, London, price 1s. net.

Prof. Joly's conclusions are disputed by Dr. A. Holmes in a letter to *Nature* (April 24). Dr. Holmes points out that the evidence provided by haloes cannot be interpreted correctly without taking into account the part played by the actinium series of elements, and this has not yet been done. He shows that the results given by the thorium minerals are very discordant, e.g. five minerals from Ceylon give periods varying from 112 to 429 millions of years. Finally, he gives reasons for supposing that the present rate of supply of sodium to the ocean by rivers may be from five to fifteen times greater than it was in previous geological times, in which case, of course, the estimates based on salinity would require to be prolonged in the same ratio. Finally, in a second letter (May 1), Dr. Holmes and Dr. Lawson point out that the heat generated by the disintegration of the potassium in the earth's crust may be as great as that produced by thorium: a fact which would presumably affect the age estimates based on isostasy, and apparently in the wrong direction!

The Rouse Ball Lecture delivered at Cambridge in May 1925, by Dr. J. H. Jeans, was entitled "Atomicity and Quanta." (Cambridge University Press. Price 2s. 6d. net.) It contains the most striking exposition of the subject yet written in our language. The greater part of the address will be found intelligible by every reader who has any interest in the subject; all those directly concerned with it have read the reprint several times already. It is a book to buy.

The Report of the Clifton College Scientific Society for the years 1924-5 which the President of the Society has kindly sent to us shows that the keen interest in scientific affairs for which the school has so long been notable is well sustained. The Society now embraces four sections, dealing respectively with Natural History, Meteorology, Archæology, and Engineering, this last including all the economic or industrial aspects of science. For many years the school has maintained an official meteorological station, and this Report contains curves showing the mean monthly sunshine and rainfall for the two years with which it is concerned. The Natural History section contributes a valuable record of the behaviour of living things in the surrounding country. We find ourselves in cordial agreement with the remarks of one of the contributors to this section, concerning

the harm done by the private collector of the rare birds and insects which may now so easily be made quite extinct in England. The members of this society limit themselves to observation and photography. The archæological section seems to be languishing at the moment; but the life of our own times is so full of things of vivid and living interest that a falling off in archæological enthusiasm is not to be wondered at. Such enthusiasm might, indeed, be considered unnatural and improper in the human boy of 1926 !

The attention of those interested in architectural acoustics is directed to Circular No. 300 of the Bureau of Standards, Washington (obtainable at the Government Printing Office, price 5 cents), which compresses into nine pages a simple and adequate account of the causes of acoustical defects and their remedy. The manner in which the reverberation time may be calculated is illustrated by several examples, data relating to the absorbing properties of all likely materials and objects being supplied. The treatment of the subject is entirely practical, and the circular should be consulted by anyone concerned with the design or improvement of any auditorium, particularly if they find the discussion of the subject in more elaborate treatises somewhat difficult.

Many other publications of considerable interest have been sent to us by the Bureau during the past quarter. They include *The National Bureau of Standards* (Circular No. 1) ; *Dielectric Constant, Power Factor, and Resistivity of Rubber and Gutta-percha* (Technologic Paper No. 299) ; *Radio-frequency Resistance and Inductance of Coils used in Broadcast Reception* (Technologic Paper No. 298), and *Relations between the Temperatures, Pressures, and Densities of Gases* (Circular No. 279). The first of these monographs is intended to show the intelligent layman the manifold benefits which accrue to the nation as a result of the work of the Bureau. The reader is first taken on a trip round the various laboratories, and is told something about the investigations now in progress. Later, he is given an account of some of the work in both pure and applied science, which has recently been completed, and, as a business man, he is told its cash value to the nation. The basis of these financial calculations is not very clear, but the final figures are really astounding. Thus, to quote only one of very many : " The total savings from hosiery standardisation research at the Bureau are estimated at twenty-eight million dollars by the officers of the national association of the industry." We wonder whether savings approaching this magnitude have resulted from the work of our own National Physical Laboratory or from that of the various isolated Research Associations. It is quite possible ; but if they have we are not told anything about it.

The paper on Rubber and Gutta-percha is interesting, not only on account of its description of the electrical properties of those substances, but for an excellent and succinct account of their preparation for industrial purposes. The dielectric constant of both substances is increased by the presence of impurities, the lowest values observed being 2.35 for rubber and 2.58 for gutta, the hydrocarbon of gutta-percha. Vulcanisation and the addition of fillers increase the dielectric constant; in particular, the presence of carbon produces a large increase. Rubber containing 20 per cent. carbon has a constant 6.0, and its power factor is more than twenty-five times greater than that of pure rubber. The resistivity ranges from  $10^{14}$  to  $10^{16}$  ohm-cm. for both rubber and gutta-percha, and is not greatly changed by vulcanisation, or by most compounding ingredients. Litharge in certain proportions increases the resistivity by a factor 10, while carbon reduces it very considerably, a value as low as  $10^8$  ohm-cm. being obtained for a material containing 35 per cent. carbon.

The next paper on our list contains a description of a method used to measure the resistance and inductance of the various types of coils commonly used for the reception of wireless broadcast, *e.g.* single-layer coils, various types of basket coil, honeycomb coils, etc. The final conclusions are (1) that *for frequencies in the broadcast range* the loose basket weave coil and the single layer coil have the lowest resistance and (2) that little advantage is gained by using solid wire larger than 24 AWG. Litz wire (No. 32/38) of a cross-section corresponding roughly to 23 AWG solid, was found to possess the lowest effective resistance, and this resistance is not seriously increased if a *few* strands of the braided wires are broken. Collodion was found to be the best binding material.

The last paper contains some useful curves and tables relating to the properties of gases. In particular, it provides a simple means of calculating the volume of a given mass of gas at high pressures, where the deviations from the perfect gas laws are sufficiently great to have a commercial importance. It also contains a valuable bibliography of papers dealing with the properties of gases.

The *Second Report of the Adhesives Research Committee* of the Department of Scientific and Industrial Research (H.M. Stationery Office, Adastral House, Kingsway, W.C.2. Price 3s. net.) contains information concerning the manufacture and properties of adhesives which should be of the greatest value in the glue trade and research laboratory, the data relating to the use of various shellac cements for smooth metal surfaces being of special interest. From the point of view of the general user it is unfortunate that the various proprietary

cements which were tested are not identified by their trade names. There are obvious objections from the point of view of some of the manufacturers ; but when tests of this kind are paid for by the public, the public are entitled to know the results. Moreover, the adhesive properties of the proprietary brands of cement depend on the surfaces on which they are spread. For example, the Report tells us that adhesive " B " is hopeless for walnut surfaces, but excellent for holding ebonite to metal, and so on—information which is utterly useless to the purchaser of the Report in the absence of a precise identification of B.

In a message from Adelaide, *The Times* of March 19 says that " Dr. W. A. Hargreaves, the Government Director of Chemistry, giving evidence before the Royal Commission which is investigating the development of secondary industries, emphasised the great possibilities of manufacturing power alcohol from straw. In a departmental test, one ton," he said, " had yielded 50 gallons, but a much higher return was likely if a company were formed to establish the industry on a permanent commercial basis. Enough straw was wasted in South Australia for the manufacture of sufficient alcohol to take the place of all the petrol it imported. It was estimated that considerably over half a million tons of straw were available every season within a 100-miles radius of Adelaide. South Australia was also more favourably situated than any other State for the establishment of a paper pulp industry using straw."

In the same city there was a recent fire at the Colonial Sugar Refining Company's works, with the result that burnt and raw sugar found its way to the sea. Apparently it destroyed animal and plant life in the water, and thousands of fish were poisoned. It is also stated that the " teredo worm," which extensively damages South Australian wharves, was destroyed, at least for the present. The paint and brass work of boats was also damaged. Further investigation of such effects due to sugar is being undertaken, but, if such destructive results actually occur, we may point out that sugar may prove to be an important mode of dealing with aquatic life, such as mosquito-larvæ, which is injurious to men, cattle, or crops.

At the annual dinner of the Imperial College of Science and Technology, held at the Hôtel Cecil on March 23, Lord Buckmaster, Chairman of the Governing Body of the College, said that scientific research was, after all, apart from the care of their fellow creatures, the greatest work to which the mind of man could be devoted. " It was a pity that the world at large stuffed its silly, empty mind with horse-racing and golf tournaments instead of turning its attention to some of the real things done by real men." Like the Bishop when someone

swore in his presence, we are glad to have these words taken so conveniently out of our mouth.

We understand that in the United States an "American Association of Vocational Counsellors" is being formed. We are told in a letter that the need for such an association is "evidenced by utter lack of understanding between members of the profession, by confusing designating titles, by absence of uniformity in methods of practice, and by almost universal failure of practitioners to attain dignified standing in the social order." There is much truth in these complaints, and we wish well to the new Association—of which the address is Suite 314, Hamm Buildings, Saint Paul, Minnesota, U.S.A.

In *The British Medical Journal* for April 24 last, Drs. J. O. Jones and C. L. Morris record a curious case of tobacco poisoning in a child whose mother rubbed her skin with a mixture of writing ink and scrapings from an old tobacco pipe (dottle), to cure a diffuse attack of ringworm—showing how poisonous tobacco residues may be, even if only applied externally. The authors wish to know the exact nature of the poisonous element contained in the applied mixture.

*Nature* of March 6, 1926, publishes a letter from R. D. Oldham, F.R.S., on the *Locus of Archimedes*. This was the name applied by the Romans to a puzzle consisting of a number of triangular pieces of ebony or ivory which, when put together, would produce figures of various kinds, such as those of elephants, boats, men, and so on. When the pieces are arranged in another way they form a couple of squares, evidently for the purpose of being put away in a case of some kind.

In the same number of *Nature* Mrs. Marie C. Stopes writes that "open fires in a house are immeasurably preferable to electric or gas fires." The reason for this is, she thinks, that "a glowing coal fire gives out something subtle," which is nourishing to the system in a way that vitamins and ultra-violet rays are nourishing. She seems to be very positive on the point, yet we know that nearly all the people of northern Europe and of America do not use open fires, but do use "heaters" of various kinds. If her hypothesis were sound, obviously these peoples should suffer in some way from which we, who use open fires, are exempt. But do they? Probably the Swedes, who use the most complicated heaters, are the finest people in Europe, and, apparently, amongst the healthiest; and so also the Canadians. Of course, where "heaters" give out fumes or other vapours, they may be injurious; but against coal fires we must remember that they also pour enormous volumes of fumes mixed with particles of carbon into the atmosphere of every town. Really, in scientific work it is necessary to look at both sides of a question.

On Friday, April 23, Mr. John F. Marshall, the director and founder of the British Mosquito-control Institute, at Hayling Island, delivered an interesting address on mosquito-control in England to the Bournemouth Natural Science Society, the chair being taken by Sir Daniel Morris. The lecturer said that fourteen hundred species of mosquitoes were now known, chiefly divisible into the two great divisions of Anophelines and Culicines. In England we have twenty-five species, three of which belong to the Anophelines. Though the Culicines do not carry malaria, they are not to be regarded as always harmless even in this country, because their bites are frequently followed by serious and sometimes fatal results, while they are often a great nuisance for a few weeks during the hot weather in the country. His Institute has dealt very successfully with the *Theobaldia annulata*, which used to be very prevalent in the south of Hampshire and along the south coast.

A correspondent in *The Times* of March 22 last sends a pitiful complaint regarding her rheumatoid arthritis. She was attacked at the age of eighteen, and was bedridden at the age of twenty-three. She cannot raise her hands to the top of her head and cannot reach her knees, she cannot put her feet flat on the ground, and her hands and fingers are contracted into the shape of reaping-hooks. For the last fifteen years every movement, except the very smallest, has been painful. And yet little is known regarding this malady, especially the cure of it. Researches on the subject are certainly being carried out in several institutions, but they have hardly been very successful as yet. Yet perhaps the day will come when we shall find the cure or prevention with certainty, just as we have done with many other diseases. As urged in SCIENCE PROGRESS over and over again, this country is not spending nearly enough money on medical research.

Some years ago, H. F. Taylor and A. W. Wells showed (Bureau of Fisheries, No. 947, Washington, 1923) that copper oleate preserves fishing nets from the destructive action of seawater; and Dr. W. R. G. Atkins, of the Plymouth Marine Biological Association (*Journal* of, No. 1, March 1926), confirms and extends this useful discovery, and has treated nets of silk, hemp, and flax with mixed copper soap prepared by Lever Brothers. There has appeared also a note by R. Fillon on the subject (*Office sci. et tech. des pêches maritimes*, No. 45, Oct. 1925).

Dr. F. W. Edridge-Green, C.B.E., has contributed an article to *The Medical World* of February 19 last on the necessity for an Appeal Board for Science. He complained that he has had much opposition from the Royal Society in connection with his work on wool-tests for colour blindness and other points,

and suggests such an Appeal Board of some kind in consequence. He is specially opposed to the system of anonymous reviews of papers adopted by the Society, and we must say that we cannot see why such reviewers should not give their names in full in connection with all such societies. If a man has formed a just opinion according to his lights regarding any paper he need not be ashamed of it. On the other hand, anonymity may occasionally lead to the indulgence of private opposition from persons who may not agree with the views expressed in a paper or who may even, though we hope rarely, possess some personal animosity or grudge against the writer. The argument in favour of anonymity is that views regarding a paper can be thus expressed more freely ; but a man must be a very timid creature not to be able to state frankly what he thinks about any article. He can always do so quite politely, and indeed even pleasantly, if he tries.

H.R.H. the Prince of Wales has consented to open the Ross Institute and Hospital for Tropical Diseases, Putney Heath, S.W.15, on July 15 (see SCIENCE PROGRESS, January 1924, page 461, and July 1925, page 131). The Duchess of Portland is President, in succession to the late Lord Leverhulme, and the Chairman is Sir Charles McLeod, Bart., and the Vice-Chairman, Mr. W. Shakspeare. The Directors are Sir Ronald Ross, Sir William Simpson, and Dr. Aldo Castellani. The Institute is supported by voluntary subscriptions, and has twenty beds for patients as well as laboratories for research—which has already been commenced in it.

The British Empire shows so little gratitude for the benefits which it receives from medical investigations that the following exception in the form of a contribution from Nigeria to the newly founded Ross Institute should be specially noticed. The letter runs : "Nigeria, Chief Secretary's Office, Lagos, 4 March, 1926. Sir,—I am directed by the Governor to acknowledge the receipt of your letter No. 12674/49 dated the 28th December, 1925, and to inform you that the following Resolution was unanimously carried at a meeting of the Legislative Council held on the 19th February, 1926 : 'That as a mark of its appreciation of the services rendered by Sir Ronald Ross, K.C.B., F.R.S., to residents in the Tropics, by his researches into the causation of malaria fever, this Council do make a grant of £1,000 to the fund which is being raised for the foundation of the Ross Institute and Hospital for Tropical Diseases.' I have the honour to be, Sir, Your obedient Servant, D. J. Jardine, Chief Secretary to the Government. To the Chairman, the Ross Institute and Hospital for Tropical Diseases, Putney Heath, London, S.W.15."

## ESSAYS

### **RECENT DISCOVERIES OF MISSING ELEMENTS. (N. M. Bligh, A.R.C.Sc., A.I.C., F.C.S.)**

WITH the discovery of four new elements within the last three years the list of elements which, in the light of modern knowledge, are presumed to exist is rapidly nearing completion. It will therefore be of interest to review, from the practical and theoretical side, developments following Moseley's epoch-making discovery in 1913. The importance of the work of this brilliant investigator cannot be overestimated; it is doubtful whether principles of such fundamental importance have been contributed to science by any other investigator at so young an age. By his untimely death at Gallipoli, in 1915, science is indeed the poorer. It is noteworthy to observe how, in three different spheres of thought, the work of past generations towards the establishment of exact laws had left us to recognise as lacking a definite but elusive principle, which, if it could be formulated, would raise laws of only particular application to the status of universal laws, or which would eliminate defects of laws known to be, in general, valid. Thus, in the search for a universal law of radiation, two laws of particular application had been established, suggesting the existence of a general law which would include both as particular cases. It remained for the genius of Planck to formulate a general law of radiation based on his quantum hypothesis, which by its far-reaching sphere of application has revolutionised theoretical physics. Again, Einstein, by his concept of discrete time, corrected an assumption which seemed so obvious that it was never regarded as an assumption, *i.e.* the universal invariance of time, and has gone far to eliminate long-standing difficulties in astronomical physics, as well as originating the most outstanding scientific theory since the time of Newton.

In the development of a table of the chemical elements half a century of work had evolved a classification which in general must approximate closely to an exact system. Its defect, however, lay in the assumption that atomic weight was fundamental, whereas Moseley, basing his work on the nuclear theory of the atom, advanced in 1911, pointed out that the atomic number, and not the atomic weight, of an element is fundamental,

the atomic number being the ordinal number, starting from hydrogen as unity, of the elements arranged in order of increasing number of electrons round the nucleus, or the number of free electrons in the neutral atom, or the number of free positive charges in the nucleus. He thus corrected those instances in the periodic table in which it was known that the properties of certain pairs of elements demanded that their positions should be interchanged, a course which could not be reconciled with their arrangement in order of increasing atomic weight. Laborious researches, *e.g.* in the case of iodine and tellurium, had in the past been conducted under the impression that the accepted atomic weights were inaccurate, and the application of Moseley's discovery cleared away the difficulty. In addition, as is well known, Moseley showed that the frequency of any line in the X-ray spectrum of an element, and its atomic number, are connected by the relation  $\nu = A(N - b)^2$ , where  $N$  is the atomic number, and  $A$  and  $b$  are constants. The properties of an element, its nuclear charge, and the number of its external electrons are thus defined by the atomic number, and the problem is brought into line with Bohr's quantum theory of spectra. The periodic law is thus seen in its wider and clearer comprehensiveness; moreover, the X-ray spectrum of an element, which is altogether simpler than its emission spectrum, can be predicted in the case of a missing element from that of its neighbours.

At this time (1913-14), gaps in the table indicated missing elements as follows: 43 and 75 analogues of manganese; 61 a rare earth element, 85 a halogen, 87 an alkali metal, and an element 91. Moseley assumed that 72 was filled by celtium, which as will subsequently be seen was not the case, so that in all there were seven gaps to be filled. According to the work of Soddy, Hahn, and Meitner (1918), number 91 is considered to be filled by a radioactive product protoactinium, in the actinium series. Attention will now be directed to number 72. This gap had been regarded as occupied by an element of the rare earth series named celtium. Urbain, in 1911, observed two faint lines in the optical spectrum which he attributed to celtium, and in 1922 the discovery of this element was definitely announced. Now according to Bohr's explanation of the rare earth elements on the basis of the quantum theory, there is a gradual development of four quantum electrons, while the five and six quantum electron groups remain unchanged. In lutecium (71) a four quantum group is complete, and it is therefore to be expected that in the following element 72 the number of electrons in the five and six quantum groups should exceed that in the rare earths by one; this points to number 72 not being a rare earth element at all, but an analogue of zirconium and titanium, which might reasonably be expected to be found in zirconium

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minerals. Moreover, discordant values in the atomic weight of titanium led Mendeléeff to suggest the possible presence of an analogue of higher atomic weight, and this should be capable of easy separation from the rare earths.

The weight of evidence was thus largely in favour of Urbain's celtium not being the missing element 72, when early in 1923 Coster and Hevesy, at Copenhagen,<sup>1</sup> announced that an extraction of zirconium minerals gave an X-ray spectrum containing six lines characteristic of element 72. To this was given the name hafnium, and it was subsequently shown to be present in very appreciable amounts and capable of easy separation from the rare earths, and generally showing itself to be a true analogue of zirconium. Thus Moseley's X-ray spectrum relationship was directly of service in filling one more gap in the table of elements.

The next gap which it appeared would probably be filled was number 43, for in 1924 two investigators<sup>2</sup> reported that they were examining the X-ray spectra of a large number of manganese minerals from different geographical sources, in the hope of discovering the missing analogue, which might be present in such small quantities as to have escaped detection by chemical means.

Although no success had been announced, the line of investigation was looked upon as so promising that an early result would certainly be forthcoming, and the name Moseleyum was proposed for the yet-undiscovered element, as a tribute to the brilliant scientist who had suggested a method of research having so general an application. The honour of making the discovery was, however, not reserved for the two above-mentioned investigators, for in June 1925 Noddack and Tacke,<sup>3</sup> of Berlin, announced the discovery of both 43, Mendeléeff's eka-manganese, and 75 dvi-manganese, the two missing analogues of manganese, and proposed the names masurium and rhenium for these two elements respectively. The line of research leading to these discoveries is especially notable on account of the careful plans and deductions employed, based on a particularly happy co-operation of the sciences of chemistry, physics, and mineralogy. It was considered that the two elements should be found in the platinum ores, or in the mineral columbite. The constitution of the earth's crust is known sufficiently to fix numerically, with a fair degree of accuracy, the frequencies of occurrence of various elements, and by this means an approximate forecast was possible of the proportion of the missing elements to be expected in the ores under investigation, which

<sup>1</sup> *Nature*, 1923, Jan. 20, Feb. 10, Feb. 17.

<sup>2</sup> *Phil. Mag.*, 1924, ii. 145.

<sup>3</sup> *Die Naturwissenschaften*, June 26, 1925, p. 56. *Nature*, July 11, 1925, p. 54.

in turn suggested the degree of accuracy in chemical separation to be attempted. From their position in the periodic table, the chemical properties of the two elements could be predicted to some extent, which again suggested the most suitable chemical treatment to adopt. Using Moseley's relation, already stated, the wave-lengths of lines in the X-ray spectra were calculated in advance. Taking account of all the above factors, a product was obtained which, on X-ray examination, gave lines agreeing almost exactly with those calculated for each of the two elements.

Confirmation of the existence of these two elements was soon afforded by two independent lines of researches announced towards the end of 1925. It will be noticed that Moseley's relation does not exclude the possibility of elements of higher atomic number than uranium (92), and indeed according to Bohr's system of atomic structure and quantum orbit considerations a series of quantum orbits is theoretically completed by an element of atomic number 118.

The first of the above-mentioned important series of investigations was carried out by Druce and Loring,<sup>1</sup> who decided, in view of the previous discoveries, to search for an element 93, or others of higher atomic number, by a study of the impurities in commercial manganese sulphate. A mixture of oxides was finally obtained containing more than 5 per cent. of div-manganese (75), and a series of photographs of the X-ray spectrum of the product was made, from a careful examination of which some indication of the presence of elements 43, 61, 75, and 93 might be obtained. The above investigators identified the  $L\alpha$  and  $L\beta$  lines of element 75, and also observed a line of the L series of the element 87 (eka-cæsium), sufficiently definite to lead them to conclude the presence of these two elements as confirmed. Indications were also obtained of two lines of the L series of element 85 (eka-iodine), and there was slight evidence of element 93; but in these two cases the presence of the elements could not be definitely claimed. The whole of the lines obtained have not as yet, however, been completely interpreted and explained. It had been pointed out many years previously<sup>2</sup> that the natural oxides of manganese usually contain traces of potassium and rubidium, analogues according to the periodic table of element 87, an observation which harmonises with the possible presence of that element in the material under investigation. In an attempt to confirm the line indicating this element,<sup>3</sup> the film was intensified and remeasured, giving a result which strongly indicated the  $L\alpha$  line of 87; further work

<sup>1</sup> *Chemical News*, Oct. 30, Nov. 6, Nov. 13, 1925. See also *ibid.*, Nov. 20, Nov. 27, Dec. 11, 1925.

<sup>2</sup> *Journal of the Chemical Society*, 1897, 533.

<sup>3</sup> *Nature*, March 27, 1926.

in this direction is in progress. It has long been known that potassium and rubidium exhibit feeble radioactivity, and Loring<sup>1</sup> has suggested that this may be due to the presence of traces of elements 85 and 87, which may be able to absorb and re-emit electrons ( $\beta$ -rays) and so account for the observed radioactivity without the apparent accumulation of a radiation product. Following up this line of reasoning, potassium and rubidium are now being examined in search of traces of element 87.

The results of Druce and Loring have been supported as regards divi-manganese by the second series of researches conducted at Prague by Heyrovský and Dolejšek.<sup>2</sup> This work has been carried out by means of an improved electrolysis apparatus known as a dropping mercury cathode,<sup>3</sup> measurements being made of the potential of a cathode immersed in saturated solutions of electrolytes. Polarisation curves are obtained indicating, at certain points, increases in current due to decomposition of the electrolyte. The polarisation curves are recorded photographically by an automatic instrument termed a polarograph.<sup>4</sup> When experiments were made with even a very pure solution of manganese salt as electrolyte a persistent impurity was indicated to the extent of  $0.5$  to  $4.0 \times 10^{-4}$  equivalents per litre. After suitable steps had been taken to remove any more common element from an enriched deposit of this impurity an X-ray spectrum giving three L series lines of element 75 was obtained. Element 43 was suspected, but not definitely confirmed, owing to the experimental difficulty of obtaining its K series spectrum. In a later communication<sup>5</sup> Heyrovský, after discussing the elimination of sources of possible error, regards the presence of element 75 as proved, and confirms some of its chemical characteristics as given by Druce, while Druce points out that as similar sources of manganese (the sulphate and not pyrolusite) were used, this view accords with his own researches. Pyrolusite gives lines difficult to identify, but with manganese sulphate all the proper lines were claimed to have been identified.<sup>6</sup>

Finally, in March of this year,<sup>7</sup> comes the announcement of the discovery of the missing element 61 by Dr. B. S. Hopkins, of the University of Illinois. Its discovery is of special interest as the group of rare earth elements lying between lanthanum (57) and lutecium (71) is now known to be complete. This group, always a difficulty to the periodic table, contains in itself no indication of the number of elements which it includes,

<sup>1</sup> *Nature*, March 27, 1926.

<sup>2</sup> *Nature*, Nov. 28, 1925.

<sup>3</sup> *Rec. trav. chim.*, 1925, **44**, 488-95.

<sup>4</sup> *Ibid.*, 1925, **44**, 496-498.

<sup>5</sup> *Nature*, 1926, **117**, 16, Jan. 2. See also Dec. 12, 1925, p. 866.

<sup>6</sup> *Ibid.*, Dec. 26, 1925, p. 943; Jan. 30, 1926, p. 153.

<sup>7</sup> *Ibid.*, March 13, 1926, p. 391.

so that Moseley's relation was here of primary importance in defining the number of elements to be expected. Moreover, the complete chemical separation of these elements is a matter of considerable difficulty. The group, however, finds a natural place in Bohr's generalised table of elements, and is beautifully explained in his quantum orbit development scheme. Apparently X-ray methods have been employed by Hopkins in his discovery, and details of his work are awaited with interest.

The general position at the present time may be summarised by stating that the halogen element 85 and the alkali metal 87 await definite discovery, as does any element of higher atomic number than uranium (92), which until recently was regarded as the highest of the series. An extensive field of work, of course, exists on the isolation, purification, and study of the chemical and physical properties of the three most recently discovered elements, while the corresponding work on hafnium is in active progress. It is hardly going too far to predict that, with improvements and refinements in experimental method on the general lines discussed, allied with the impetus lately given to the important problem of isolating the two remaining elements, this result will be an accomplished fact in the near future.

**THE LIFE AND WORK OF A MECHANISTIC PHILOSOPHER: JACQUES LOEB.** (Prof. T. Brailsford Robertson, Ph.D., D.Sc. University of Adelaide, South Australia.)

JACQUES LOEB, the greatest experimental biologist of his age, pioneer in every branch of scientific endeavour which he undertook, and one of the greatest discoverers of new and uncharted realms of fact who has bestowed the fruits of his genius upon the world, was born in Alsace in 1859, and died during his brief annual holiday in Bermuda early in 1924.

In 1713, in Langres, in the Province of Haute Marne, was born Denis Diderot, who was destined to become one of the intensely active group of journalists and propagandists who paved the way for that vast inversion of ideas, no less than of political forms, the French Revolution. There may seem, at first sight, to be little connection between two men living in such different ages, occupied by such divergent activities, yet it is my purpose to show that the character and ideals of the one afford the clue to the career of the other and to the impelling force which drove him and which, perhaps, by its impetus, enabled him to make such great discoveries.

To understand the origin of the connection between these two men one must realise something of the circumstances of Loeb's parentage and early life. He was of the Jewish race

and descended from members of that coterie of intellectually brilliant Jews who were compelled to leave Lisbon and seek refuge in Amsterdam in the Middle Ages in consequence of the persecution of the Inquisition. From this group sprang many of the foremost intellects of their age—Spinoza among them. Heine, to whom Loeb was distantly related, was descended from the same group. With it departed from Lisbon all the intellectual glory of Portugal. From Amsterdam Loeb's ancestors made their way along the Rhine and ultimately settled in Alsace. During the period of the French Revolution and the first half of the nineteenth century they were French subjects, passing under the dominion of Germany after the Franco-Prussian War. Loeb's father, who was a man of business in very comfortable circumstances, was strongly imbued with French sympathies and ideals, and appears to have regarded the German overlord with genuine detestation. As a boy Loeb was never permitted to speak German to his father; French was the language of the home, although, of course, in school German was the language in which instruction was imparted. Loeb seems to have been brought up upon the literature of the French Revolution, and the influence of this literature upon his outlook remained unabated to the end of his life.

In his book *The Organism as a Whole*, published eight years before his death, this influence is frankly avowed in the following dedication: "The book is dedicated to that group of free-thinkers, including D'Alembert, Diderot, Holbach and Voltaire, who first dared to follow the consequences of a mechanistic science, incomplete as it then was, to the rules of human conduct, who thereby laid the foundation of that spirit of tolerance, justice and gentleness which was the hope of our civilisation until it was buried under the wave of homicidal emotion which has swept through the world."

The central idea which emerged from all the writings of the pre-revolutionary period in France was that human nature was not originally bad, as the people were then taught to believe by their religious instructors, but originally good, in consequence of which the world they inhabit could be made a desirable abiding place, provided only man used his intellect to remove from his path those obstacles and stumbling-blocks responsible for his most painful experiences; the evil in the world, they believed, was attributable to faulty education, lack of intelligence and bad institutions, among which they reckoned the then-existing forms of government and dogmas of religion.

The most vigorous and popular exponent of these ideas through the medium of the great *Encyclopædia*, and his many other writings, was Diderot, and to Diderot's influence is mainly

to be traced, perhaps, the immense popularity which these ideas achieved. It was not Diderot, however, but Holbach, a somewhat younger man, who crystallised these ideas into one coherent system of mechanistic philosophy and gave clear form to the political and ethical aspirations which arose out of them. The modern world has lost that supreme confidence in the nature of man which the Encyclopædists avowed, very largely on account of the growth of evolutionary literature and the evolutionary method of thought. As John Morley has put it: "Like all others of his school, Holbach has no perception nor sense of the necessity of an explanation how the mental world came to be what it is, nor how men came to think and believe what they do think and believe. He gives them what he deems unanswerable reasons for changing their convictions, but never dreams of asking men in what elements of human character the older convictions had their root, and from what foundations for the conduct of life they drew the current of their sap."

We may state the objection, which perhaps most modern men may feel, in the following words: If human nature is essentially excellent, how does it arise that our institutions are evil, as it is stated they are, and that our education is bad, as it is stated to be? Somewhere in the collective mass of human instincts there must be instincts which lead historically to bad results, just as there are instincts which lead historically to good results. If, then, all institutions could be remoulded "nearer to our heart's desire," the natural operation of these human motives would in course of time produce institutions both good and bad, and the society living under them would experience its own difficulties and shortcomings, individuals would be thwarted then as now, personalities would be deformed by the operation of bad institutions then as now, and once more we would perceive that "heaven's gift takes earth's abatement."

It was perhaps some reluctant recognition of this fact which underlay the slight tone of acerbity in which Loeb always referred in conversation to evolutionary literature. Not that he disbelieved in organic evolution. No sane biologist disbelieves in it. But in his writings he rather pointedly ignored it. Moreover, he always distrusted speculations founded upon the evolutionary theory on the ground that such hypotheses are not accessible to experimental verification.

The affinity of Loeb to Diderot extended further than the capture by the one of the intellectual sympathies of the other. There was a truly extraordinary similarity between the outlooks and characters of the two men. Many of the accounts by eye-witnesses of the personality of Denis Diderot

irresistibly remind those who knew him of the personality of Jacques Loeb. The extraordinary rapidity and profundity of his thought, piercing obscurity like a ray of lightning, seems to have been in a great measure also a characteristic of Diderot. Marmontel, who knew Diderot, has written of him in words which might be applied almost without change to Loeb. Above all, when he says that "he who only knows Diderot in his writings does not know him at all," for indeed all who knew him realise that Loeb has left but an imperfect picture of himself in his writings. His gift of literary expression, although not inconsiderable, was far inferior to his gift of speech— instant, rapid, and voluble speech, with its extraordinary clarity and sagacity, and the unexpected angles from which he could throw new light upon old subjects. Then again, only those who were privileged to be associated with him in his laboratory could possibly realise Loeb's gifts of critical penetration and his power of extracting the very core of the matter from the many problems presented to him. Once more, what Marmontel has to tell us of Diderot may be said of Loeb: "Never was Diderot seen to such advantage as when an author consulted him about a work. You should have seen him take hold of the subject, pierce to the bottom of it and at a single glance discover of what riches and of what beauty it was susceptible."

On the other hand, in his writings, the influence of Holbach upon Loeb is perhaps even more clearly perceptible than that of Diderot. On the professional side Holbach indeed approached Loeb more closely than Diderot. For Holbach attempted deliberately to apply such science as then existed to the solution of human problems, and above all to the central problem of man's relation to the universe. Diderot was more discursive, more purely literary, appropriating such scientific scraps as chanced to float into his view, and since Loeb's own activities were expended upon scientific endeavour, when it came to the actual account of his work and its bearing upon human motive, we find Loeb adapting the conceptions of Holbach to the conditions of modern life and enriching them with modern scientific knowledge. Many passages in Loeb's *Comparative Physiology of the Brain* and in his *Mechanistic Conception of Life* remind us irresistibly of passages in Holbach's *Systema Naturæ*. When we recall the fact that the System of Nature terrified Voltaire and shocked Rousseau, it will not occasion surprise that the conceptions advanced by Loeb were in no respect conventional and that they terrified and shocked not a few of our own generation.

When we perceive a man of intellect devoting a life-time of the most strenuous and unrelenting endeavour to many and most varied works, we may, rightly or wrongly, suspect that under-

lying his diligence and unsparing expenditure of self there is some unifying and impelling motive and that the ends which he is immediately attaining are not regarded by him as ends in themselves, but as means towards some other end, some general objective towards which he is aiming. Loeb had a general objective, and the clue to it is to be found in the literature of the Encyclopædists. All would be well in this world, they held, if it were not for faults of education and environment created by false beliefs, mistaken prejudices, and the domination of ignorant and selfish despots. What is that, we may imagine young Loeb asking himself, which holds the masses most clearly under subjection? What is that which chains them to unwearying labour, the fruits of which others will reap? What is it that makes them bear their lot without complaining and fetters them to the task to which they were born? His answer was, Religious dogma and superstition, recognition of a higher will which holds out to them an illusory hope of a better world in the hereafter, and their consequent fear to assert their own individuality.

The next stage in the development of the point of view would be to inquire how these dogmas came to be believed. Through ignorance, naturally, and chiefly through our ignorance of the forces which move the world, and above all of those forces which move ourselves and which carry on the marvellous and miraculous-seeming processes of life. It is in a solution of the problems of life, therefore, that we must seek for enlightenment. We must strip away its mystery and show that living beings, no less than inanimate bodies, are machines impelled by physical and mechanical forces. This, we may imagine, was the starting-point of Loeb's career; and to these views and these objectives he held unvaryingly throughout his life.

Loeb took his degree of M.D. at Strasbourg in 1884, and the character of the then Professor of Physiology was admirably conceived to strengthen the attitude of mind in which Loeb was to approach the subject. Fick was the last of a great school of physical physiologists, of whom Helmholtz and Du Bois Reymond were the most illustrious examples. They were physiologists who were so thoroughly physicists that one hardly knows in which branch of science their contributions were most noteworthy. Fick himself is, in fact, remembered rather for his studies on Diffusion in Liquids than for his numerous most excellent researches in physiology. This was the Professor under whom Loeb studied the subject that was to become his life-work. For a brief time Loeb contemplated entering medical practice, but the routine of professional life speedily wearied him, and the lure of his wider objective beckoned him

irresistibly to research. Fortunately his father had left him with sufficient means to indulge his inclinations without necessitating endurance of the excessive hardships which have attended so many in this career.

It was at this period that Professor Goltz at Wurzburg was engaged in his classical researches upon the functions of the higher parts of the brain, the cerebral hemispheres, and he had just performed the startling experiment of removing the cerebral hemisphere altogether from an animal and had shown that it was possible for such an animal to carry on its physiological activities, to walk, eat and bear young, and since, if anywhere, the mental activities of man might be presumed to be dependent upon these portions of the brain, this experiment rendered evident the fact that a great proportion of our daily activities do not necessarily involve mentality, but are in reality automatic. Here seemed an opening into the ancient unsolved problem of the relationship of mind to matter, and here, if anywhere, prejudice, superstition and dogma felt safely entrenched in the unknowable. This seemed to afford an opportunity for that dispersion of obscurity for which Loeb longed, and he naturally proceeded to Wurzburg, where he studied under Goltz and conducted research on brain physiology. From 1886 to 1888 he acted as assistant to Goltz. By 1888, however, he had realised, probably, the limitations of these new experiments and the very fragmentary and insufficient character of the information which they revealed to us. Important though every fragment of information may be, yet Goltz's method of attacking so intricate and subtle a problem represented merely a beginning, the preliminary step towards much more elaborate analyses of the phenomena. In 1888 Loeb returned to Fick at Strasbourg as his assistant, and he spent the summers of 1889 to 1891 at the Zoological Station at Naples, with results which have permanently enriched biology.

One of those living phenomena about which mysticism most loves to linger is that development whereby an orderly growth of form and function evolves out of the simple undifferentiated jelly-like mass of protoplasm which constitutes an egg-cell. If this process could be shown to be guided by physical and chemical forces a severe blow would have been struck at the roots of the obscurantism which it was Loeb's main object to destroy. In the course of his experiments at Naples he showed for the first time that the development of the organs and structures of animals is, in fact, directed by physical and chemical forces. He showed that if the animal be placed under exceptional conditions involving a change of the usual physical and chemical factors, development becomes abnormal

in a direction determined by the new factors. In many of the simpler forms of life which inhabit the ocean there is a clear differentiation of function and structure between an anterior end in which is situated the mouth, provided with suitable appendages for facilitating the entry of food, and a posterior end which is frequently fixed to some solid object on the floor of the ocean. In these forms mutilation results in the regeneration or regrowth of the missing parts. If the posterior end be cut off, a new foot, or posterior extremity, is regenerated. If the anterior end provided with mouth and tentacles be cut off, a new mouth and tentacles are regenerated. It was first shown by Loeb that if these animals be simply inverted after injury, placed upside down, that is, and kept in that position, that end which is uppermost produces the mouth and tentacles, that end which is lowest produces the foot or means of attachment to solid objects. Always the lower surface developed means of attachment, the upper surface the tentacular organs and orifice for the insertion of food. And such developments were shown to be totally irrespective of their utility to the animal, since by suitable incisions slanting upwards or downwards it was frequently possible to produce more than one, or even several, mouths, all but one of which were useless to the animal ; or, conversely, it was possible to produce several organs for attachment to rocks, only one of which could possibly be of use. He showed that the polarity of many of these organisms is due to gravity, to the sinking of certain substances in their tissues to the lower levels, which substances prevent the growth of head appendages and facilitate the growth of foot appendages. So that by altering the ordinary direction of action of gravity it was possible to produce these organs in unnatural situations. This was the phenomenon which he designated Heteromorphosis, and many remarkable examples have since been discovered by many other workers besides Loeb himself. The most extraordinary is perhaps the discovery of Herbst that if, in certain Crustaceans, the eye be removed, instead of regenerating a new eye the animal regenerates in its place a new antenna, so that it has now three antennæ instead of two, and one eye. There is no utility in this, and consequently we cannot refer it to design, even if we were to suppose that the animal could control its own development. It is due to forces, not all of which are yet understood, but which we know to be of a physical and chemical character, and which the researches of Loeb have done much to unravel.

During these years at Naples Loeb discovered, however, a series of phenomena which were destined perhaps, in the immediate future, to prove even more important and even more

effective towards the promotion of his general objective than those which I have just outlined. It had always been a matter of common knowledge that many flowers, and most seedlings, turn towards the light, bend, that is, in the direction from which light is proceeding. It was also a matter of common knowledge that some animals, such as moths and other insects, are attracted by light.

Until 1890 nobody suspected any relation between these phenomena in animals and plants. The bending of plants towards light was commonly assumed to be of a mechanical character, since nobody had yet seriously proposed to endow plants with intelligence. The seeking of light by animals, however, even by insects, was commonly interpreted to mean that these animals possessed consciousness, and were gratifying a sense akin to curiosity. Almost all actions of animals until that date were interpreted in an anthropomorphic way, that is, in other words, human feelings, human consciousness, human recollection, and so forth, were supposed by the observer to be possessed by all animals in differing degree, and the actions of animals were interpreted in terms of their supposed sensations and their supposed desires inspiring voluntary effort. There was a large literature, culminating in the well-known work of Sir John Lubbock, which employed, completely to its own satisfaction, this simple method of accounting for every feature of the behaviour of animals which the physiology of that day did not yet suffice to explain. A great change was to come. We now study the behaviour of animals chiefly from the outside. We assume nothing. We ask *what* they do with a curiosity which increases rather than abates, but if we venture to say *why* they do it we no longer appeal to their emotions, their thoughts, their desires. We know nothing of these things. There is, in fact, no way in which thought, or desire, or feeling can be conveyed to us by creatures who have no knowledge of our conventional symbols for the conveyance of thought. Hardly, even, can two different races of men thoroughly comprehend each other's thoughts or feelings, and, in fact, the thoughts and feelings of any other individual are merely inferred, we have no immediate knowledge of them ; but of the behaviour of men and animals we have immediate knowledge, it requires for us no interpreter but our senses. This change of attitude of mind we owe to Loeb. As Brett has said in his work on *The History of Psychology*, "The kindly observers who from 1860 to 1890 entertained a large public with curious narratives were rudely silenced by the reports which Jacques Loeb published in the last year of that epoch. From this work arose a new type of comparative psychology, the Mechanistic School."

The attraction of animals towards light was shown to depend upon precisely the same factors that govern the bending of plants towards light. The difference was simply this—that whereas plants are fixed by their roots to the soil, animals are usually free to move. The plant bends because the tension of its protoplasm is greatest on the illuminated side. The animal flies, walks, or runs towards the light, because the tension of its muscles is greatest on the illuminated side, and the wing muscles, or leg muscles, therefore contract through a greater sweep on the unilluminated side and turn the animal mechanically towards the source of illumination. Many years of Loeb's life were devoted to the elaboration of this thesis. It was shown that both animals and plants exhibit many kinds of tropism besides heliotropism, or movement directed by light. Towards certain chemicals, animals, and some plant-cells, may exhibit chemotropism or attraction to, or repulsion from, the source from which the chemical is diffusing. Towards gravity many organisms show geotropism, swimming or crawling, or growing always upwards, away, that is, from the centre of the earth. Nor are these tropisms confined to usual or customary stimuli such as those to which the animal has become adapted in its environment. They are exhibited towards forces totally outside the normal experience of animals, for example, towards electrical forces, and in every instance the mechanism of attraction to or repulsion from the source of the stimulus is the same, namely, inequality of the tension of the muscles on the two sides of the body.

It was in these years that Loeb met the lady who became his wife, Miss Anne Leonard, who was then engaged in study at the University of Berne. Loeb was an enthusiastic mountaineer, and it was during one of his trips to Switzerland for the purpose of indulging this hobby that he met Miss Leonard. His wife was an American, and this, together with his hatred of the military spirit which dominated his own country, doubtless turned his thoughts towards the New World. In 1891 he accepted the position of Associate in Biology at Bryn Mawr College for Girls. He occupied this position for one year, and then was appointed Assistant Professor of Physiology and Experimental Biology in the University of Chicago, being promoted to Associate Professor in 1895, and full Professor in 1900. In 1903 he left Chicago to accept the newly created professorship of Physiology in the University of California, where I joined him as Assistant two years later. During the first ten years of his sojourn in America Loeb directed his energies mainly towards the application to biology of the newer knowledge of physical chemistry which had been given to the world by the labours of Ostwald, Nernst, van 't Hoff and

Arrhenius. A number of important facts were discovered of a somewhat technical nature, hardly lending themselves to brief description of a general character. Many of the things that interest us most in physiology at the present day, however, were first discovered or indicated in Loeb's laboratory during this decade.

About 1900 Professor Loeb's former colleague at Bryn Mawr, Prof. T. H. Morgan, noticed a curious phenomenon among the egg-cells of a sea-urchin with which he was working. The sea-urchin normally reproduces by the simple method of discharging its unfertilised eggs into the sea, where by chance they may meet with a sperm similarly discharged into the sea by the male. When this union takes place the egg develops a clear transparent membrane around it which prevents the entry of any more sperm, and then proceeds to divide and undergo development, until, after a long time, the mature form is again produced. Now, Morgan observed that a very small number among a large mass of sea-urchin eggs would occasionally, if the sea-water had evaporated slightly, develop membranes just as if they had been fertilised, and might even undergo one or two divisions. He showed this to Loeb, who immediately grasped the potentialities of such an observation and its immense value towards the furtherance of his general objective. The process of fertilisation which initiates the development of animals and plants was surely the most mysterious thing in the world. If he could strip the mystery from this and show that it was possible to imitate fertilisation by some physical or chemical agent, a deadly blow would have been delivered at that sort of mysticism which revels in our ignorance of natural phenomena. For several years he devoted himself to this problem, until at length he had succeeded in interpreting the accidental observations which Morgan had made, and in so improving upon the original chance experiment as to be able to produce at will a 100 per cent. of fertilised eggs from eggs which had never been fertilised by sperm. To-day the artificial fertilisation of these eggs is no more difficult than the development of a photographic film. From sea-urchins he proceeded to other forms, and artificial parthenogenesis, or fertilisation without the agency of the male element, has now been accomplished, thanks to Loeb's researches, in a great variety of species of animals. The highest form in which artificial fertilisation has yet proved possible is the frog, but many embryo frogs have been produced, and not a few have been reared to maturity without the agency of any male element. These researches afford a remarkable example of the fact that fundamental discoveries need not always be of an expensive kind. The laboratory

in which Loeb discovered the majority of these facts was a simple shed erected upon the sea-coast of California at Pacific Grove, close to the old Spanish capital of California, Monterey. The equipment required was of the simplest and least expensive character, and the results are among the most valuable in biology.

In 1910 Loeb left California to accept the position of Director of a department especially created for him at the Rockefeller Institute for Medical Research in New York. His work upon marine forms of life was continued on the Atlantic Coast at that remarkable collection of marine laboratories which forms the greater part of the little town of Woods Hole in Massachusetts. The fourteen years during which Loeb was associated with the Rockefeller Institute were devoted to the refinement and elaboration of all his previous work. Experiments, which were formerly qualitative and inexact, were here rendered quantitative and exact, and his work upon tropisms especially was greatly extended. The phenomena of fertilisation, through the aid of the artificial method which he had discovered, were largely interpreted, and, thanks to his researches and those of others who followed in his lead, we now possess some exact knowledge of the essential nature of the chemical changes which the spermatozoon induces in the egg and which initiate its development.

I have said that the personality of Jacques Loeb was imperfectly displayed in his writings. His point of view towards our social institutions is clearly and vividly expressed over and over again at every point at which his results bore, in his imagination, some application to human affairs and conduct. But the impression created by his writings is not a true one of the man himself. Above all they fail to reveal to the reader the candour and simplicity and openness to conviction which must always stand out, in the minds of those who knew him well, as his leading characteristics.

An amusing instance of his candour and simplicity I have often heard Loeb tell himself, not only in my presence, but in the presence of many others, so that I am revealing no confidence in repeating it. When Loeb accepted his first American appointment at Bryn Mawr he was assigned the duty of instructing students in embryology, a subject in which he had not previously taken any exceptional interest. After one of his lectures a student approached him with a question. His answer was highly characteristic. "My dear young lady, I cannot answer your question, because I have not yet read that chapter of the textbook myself, but if you will come to me to-morrow, I shall then have read it, and I may be able to answer you." This was the man who was destined in a few

years to become the leading experimental embryologist of his generation !

His openness to conviction is far from apparent in many of his writings, nevertheless it was very real. He no more hesitated to reconsider his opinions than he did to avow his ignorance when he felt it. He always expressed himself with vigour and pushed home an argument in vivid language to its logical conclusion without any restraint. Nevertheless, if replied to in the same vein he would duly weigh the argument, and if it appeared to him to defeat his own, would instantly admit the fact without any reserve whatever. The opening sentence of his recantation was invariably " You are right," the second, " I was wrong." Those who were unaccustomed to the vigour and vividness of his speech would often, not unnaturally, consider him dogmatic ; but did they press their views with the same enthusiasm, singleness of purpose, and intellectual insight as Loeb himself, they would shortly reconsider their first estimate.

Loeb was not a scholar in the ordinary acceptance of that term ; indeed great investigators are very rarely scholars. Great capacity for assimilating the results of others is rarely associated with great capacity for the attainment of new results hitherto undiscovered. Loeb's mentality was kinetic rather than assimilative ; the mere accumulation of knowledge for its own sake did not appeal to him. Nevertheless, he was a very widely read man in many branches of science, and particularly in physics. In fact, although his social ideals and aspirations were, as we have seen, largely French in origin, as a scientific man he owed more to the inspiration of English workers than to those of any other nation. He very greatly admired the English school of physicists, and he regretted that their methods and outlook had not yet influenced biology to the extent that he considered desirable. He was not only well read in scientific literature, but also much better read in philosophical literature than his writings indicate. I well recollect an occasion when I was complaining to him of some events with which I was very discontented, and he replied, " You have read Spinoza. Do you not remember," and then quoted the whole of the opening paragraphs of Spinoza's *Treatise on the Improvement of the Understanding*, paragraphs which breathe an almost Sophoclean calm and indifference to ordinary human joys and woes. I hasten to add that Loeb did not himself obey his own injunctions. His attitude of mind was far removed from that philosophic indifference which Spinoza sought to cultivate.

In general literature, at the time I knew him, he did not appear to me to possess any especial favourites, but one who

knew him in the later years of his life has stated that his favourite author was Anatole France. Considering his own attitude towards social and religious dogmas, one could well imagine that the satirical treatment of these subjects by this modern Lucian would strongly appeal to him, more especially since his own humour was frequently cast in the satiric mould.

He did not share that love for administrative work for its own sake which the majority of Americans possess in such high degree, nor did he relish attendance upon the perennial committee-meetings in which the academic soul delights. He was an uncomfortable member of a committee, because he always insisted upon dragging fundamental principles into the discussion. There is nothing which so shocks your good committee-man as a naked first principle. He hastens to clothe it in a garment of compromise, no matter how patched and threadbare the improvised covering may be. Promptly Loeb tore it off again, once more revealing the principle in all its utter truth and simplicity, a proceeding which did not invariably tend towards the production of harmony.

As a teacher Loeb was inspiring, as a lecturer at once brilliant and easy to understand. As a teacher he was best appreciated by those who approached the subject from the single-minded point of view of interest in science for its own sake. To these his inspiration was valuable beyond measure. By others who approached the subject as a duty, or as a means to some end which they esteemed useful, he was necessarily regarded as unpractical. This was particularly true of medical students, who found his lectures singularly disconcerting. He lectured on no fixed plan but dealt with current problems of paramount importance in a marvellously illuminating manner, always seeking for fundamentals and underlying principles, approaching a subject with a view to analysing it as a physicist or a chemist would analyse it, without any reference to its immediate utility for the purposes of professional life. The average medical student approaches his lectures from the point of view of a would-be artisan rather than that of a would-be scientific man, seeking recipes and methods rather than general underlying principles and a scientific comprehension of the phenomena as a whole. There is always this conflict between the point of view of the scientific man and that of the narrowly practical man. To the purely scientific type of man it seems obvious that application will flow naturally out of complete and comprehensive knowledge and may therefore be left, in the main, to the initiative of the informed individual. It is for this reason that when one ceases merely to seek utility one most surely finds it. But to the so-called practical man

applications are all-important, and comprehension is merely an intellectual hobby. So the medical students who desired to learn a recipe for the treatment of measles and the way to perform a tracheotomy were forced to listen to a brilliant and fascinating exposition on the general principles underlying the permeability of living cells to chemical agents ; or on the oxidations proceeding in the nucleus ; or on the fundamental laws underlying and governing the regeneration of lost parts in the lower marine animals, or in plants. It is, in fact, as Loeb foresaw it would be, out of these fundamental considerations that the new medical science is growing at the present time, and possibly some of the students who felt impatient twenty years ago at being forced to study such things, would esteem it a privilege to hear those lectures again to-day.

I have sought to show that Loeb possessed that intellectual generosity which enabled him to admit the justice of the arguments of another even when they contravened his own. Not only in these, but in more material respects, he was generous also, and many among his students owed their ability to continue their chosen career to his kindly and timely help, of a very practical kind, always so unobtrusively exercised that the greater part of it remained unknown.

As an experimenter Loeb was unrelenting, tireless. Those who, in the decade from 1900 to 1910, read the vast outpouring of results from his laboratories could not believe that so much could have been discovered by a single man in so limited a space of time, or that so many experiments could have been conducted with adequate care to ensure their accuracy. As a former member of his staff for five years, I can testify that Loeb was meticulously careful to avoid even the appearance of appropriating the results of any other man, particularly of members of his own department. He never even allotted us tasks for the alleviation of his own heavy labours. On the other hand, any suspicions that may once have been felt concerning the essential accuracy of his results (whatever may be thought of his interpretation of them) have long since been dispelled and made to appear ridiculous by the mere natural growth of those regions of scientific knowledge in which he laboured and in which he was, at that time, so frequently a lonely pioneer.

The secret of his productivity as an investigator lay in his immense industry, and in the extraordinary sagacity with which he designed his experiments to elicit an unequivocal answer to the questions which he had in mind. To him his work was a first and paramount interest, and it was therefore at once a labour and a relaxation. His holidays were brief and infrequent. His working hours comprised the greater part of his waking

life. Gifted with a strong physique, he rarely exhibited any sign of the immense strain which other men might have felt, had they performed half his labour. His experiments were multitudinous, and great as was his published output of experimental results, they represent but an inconsiderable fraction of the experiments which he actually performed. He never tired of repeating well-established experimental results, to assure himself once more of their validity, and that nothing of possible significance had been overlooked.

And now what shall we say of his main theses, culminating in the view that man is an automaton, moved wholly by physical and chemical forces? In his writings he does not seriously grapple with the problem of the relationship between mind or consciousness and matter. The writer of the obituary notice which appeared in *Nature* in April 1924 was evidently one who knew him well, and, as he says, Loeb waved aside all objections to his mechanistic view which were based upon the existence of consciousness "with a smiling epiphenomenalism which was impervious to argument." By epiphenomenalism we mean the simple statement of the obvious fact that consciousness and material changes in our body proceed parallel to one another, consciousness reflecting accurately in its own world that which happens in the material world. The illustration employed by Huxley, although very imperfect from a logical point of view, may convey some idea of the attitude of mind of the epiphenomenalist. He says that consciousness accompanies bodily events as the shadow of the wheel accompanies the wheel. The defect of the illustration is this—that there is, in fact, a physical connection between the shadow and the wheel, the physical connection of light, whereas the epiphenomenalist assumes, in such connection, merely the parallelism. He makes no assumptions. His point of view is agnostic. He states a fact and leaves the matter there.

So far as his writings go Loeb certainly may be classed as an epiphenomenalist. But undoubtedly he thought much more about this great central problem than he chose to express in his writings. He would probably have said that it was no business of his, as a scientific investigator, to throw frail bridges of hypothesis over abysses of ignorance. As for his doctrine that men and animals are machines, our attitude towards this will depend upon what we mean by a machine. As he conceived a machine, probably there are many who would disagree with him. But it may be questioned whether a machine is, in fact, anything at all other than something that we understand, understand fully in its origin, in the way it happens and in its effects. Directly we achieve such comprehension

events appear to us to acquire a certain inevitability. They are pervaded by necessity and volition disappears, but it is possible that this impression arises merely from the relativity of human intelligence.

Whatever we may individually think of the ultimate conclusions which he drew from his investigations, there can be but one opinion of Loeb's immense serviceability as an investigator. With his departure from our midst an immense intellectual force has been taken from the world and his disciples and those who disagree with him alike are permanently impoverished by its loss.

## ESSAY-REVIEWS

**MODERN EUGENICS.** By R. A. FISHER, Sc.D., Fellow of Gonville and Caius College. Being a Review of **The Need for Eugenic Reform**, by LEONARD DARWIN. [Pp. xvii + 529.] (London: John Murray, 1926. Price 12s. net.)

THE subject of Eugenics, more than any other in contemporary thought, appears to defy formal classification. Not a political creed, it bears intimately upon a dozen political questions ; not a religion, it emphasises moral responsibilities almost neglected by the Churches ; not a science, it draws its inspiration from the great biological advances of the last two generations, and in a very special sense is a product of the evolutionary theory.

An appreciation of the similarity of organic beings, including mankind, and an inkling of the possibility of their origin by gradual transmutation, was familiar to Greek thinkers ; and even at their vaguest such ideas must prompt questions as to the future of the human race. Such indistinct analogies as were available might suggest equally either boundless hopes or the gloomiest surmises, but, in the absence of an exact elucidation of the chain of causes by which the present situation determines the future state, could provide no basis for moral endeavour, or for concerted action. The process must be envisaged at this stage as predetermined and automatic, overruling the accidents both of human knowledge and of human effort.

What made the practical difference, and marked an epoch, the importance of which it is even now difficult to gauge, was not the acceptance of organic evolution as an historical fact, but the discovery in natural selection of a means whereby, through the action of known causes, existing conditions, capable of human adjustment and control, produce organic changes. The middle nineteenth century was a period of confidence and enterprise. It is characteristic of the time that in the absence of a practical working cause, philosophical speculations as to organic evolution as an historical fact were treated with coldness and indifference ; whereas, as soon as the effective agency was discovered, the most advanced thinkers of the time hailed the theory with a boundless enthusiasm which overcame all opposition.

The task of applying the new knowledge to man fell to

Francis Galton, a man of restless versatility ; lion-hunter and explorer, meteorologist and statistician, his ingenuity in devising new methods of research found its last and most lasting outlet in the measurement of human characteristics. Psycho-physical measurements and mental testing excited his interest, and owe to him much of their present importance ; in addition, so well did he lay the foundations of the study of human heredity that the physical and mental characters in man are still among the best understood of the heritable quantitative characters. His work in fact left no room to doubt that if the much-abused " methods of the stock-yard " were applicable to mankind, the human race could be improved in any desired direction, within a short historical period, to an extent exceeding existing differences between widely different races. Incidentally, it was Galton who provided eugenics with a name ; a slightly pedantic one, with high-brow affinities, yet not inappropriate in suggesting the somewhat severe and intellectual detachment with which it is necessary to approach difficult questions, especially those of intense human interest.

Quite distinct from the line of thought suggested by evolutionary theory, and touching a much wider circle of educated opinion, is the enigma presented by the fall of once powerful nations ; and even more insistently, by the decay of far-reaching and well-established civilisations. The lessons of the past are perplexing and, at first sight at least, discouraging. It is not merely that history shows us no fool-proof organisation of society ; it shows us no great nation which does not seem to lose, after a few centuries of progress and achievement, almost every quality which could warrant our admiration. The anxiety aroused by this spectacle was met in the eighteenth, and to a decreasing extent in the nineteenth centuries, by the theory that personal liberty was the panacea. The Romans were represented as having lost, by some accident of internal dissension, their birthright of a popular government ; and their decay could be ascribed to the inevitable consequences of a despotic regime. Few now have such faith in political institutions, and the anxiety of the thinking public is chiefly allayed by the fact that few in a modern State have occasion to feel responsible for the consequences of our corporate acts.

Whether this enigma will find its solution in the study of human inheritance and of the selective influences characteristic of civilised life is a question of which the future will judge. For the present, it is certain that the great impetus which Eugenics has received in modern times is due to the injuries which modern legislation seems liable to inflict upon the hereditary qualities of our immediate posterity. Many pieces of modern administrative machinery strike those who have occasion to study their

workings with the forcible impression that they might have been designed to repress parenthood among the self-respecting while encouraging boundless fecundity among ne'er-do-well, or deliberately parasitic, groups. Impressions formed thus, after long and impartial experience of the actual workings of the most benevolent types of state action, aimed especially at the relief of hardship and poverty, will not be easily effaced by the facile assurance that all we have to do, to relieve the nation of what appear to be hereditary defects and disabilities, is to "clear away the slums," and to "ensure to everyone a good education." These things are more easily said than done, and will only be said by those who, determined to shirk the eugenic question, are willing to make the most extravagant claims for the benefits of such institutional environment as it is possible to provide. We are told to provide "a good education," but if father and mother are criminal or dissolute by what means is the State to provide a *good* substitute for a *good* father and mother? Is the effective agency to be the educational service, the police, or the poor-law? Those who would persuade themselves that all mental and moral disabilities are ascribable to early environment are building up an unanswerable case for the discouragement of reproduction in bad homes.

In considering an extensive work we are concerned with the qualifications of the author. Major Leonard Darwin can look back to long experience of administrative and public work. Without being a scientific specialist, he has the somewhat rare qualification of a lifelong and detached interest in the natural sciences. No reader of this book will deny his patience in weighing opinions, and deliberate caution in decision—qualities which appear to have built up and matured a power of profound and wholly rational judgment. The book is a storehouse of arguments, and Major Darwin seems to have inherited in full the power of stating carefully and sympathetically arguments in opposition to his own views. Examples of arguments peculiarly his own, which should have a lasting effect on eugenic policy, will be given below, and these must serve to convey an idea of the contents of the book.

There are five ways in which the activity of the present generation may possibly benefit mankind in the future: (*a*) environmental reform, by tradition or permanent physical improvements, (*b*) by the care of prenatal life, (*c*) by preventing the permanent injury of the germ-plasm by (so-called) racial poisons, (*d*) by the possible inheritance of acquired characters, (*e*) by selection. Of these (*a*), though of immense importance, is not the subject of this book; it is, however, stressed that improved environment will always be welcomed by eugenicists, not only for its own value, but also because the less evil can be

ascribed to bad environment the more obvious will the need of racial improvement be made, and the more easily will it be effected. Similarly, the sympathy with which we regard the care of prenatal life must not allow us to neglect the special province of eugenic reform. The racial effects of the so-called racial poisons, syphilis and excessive alcohol, are too uncertain to strengthen at all the case for combating these scourges with all our power, and should not influence our social policy. The inheritance of acquired characters, if established as a fact, would fall, theoretically, within the field of eugenics, yet since it is acknowledged by those biologists who take the Lamarckian view, that the racial effects of any possible improvement in human education would be at most extremely slow, we cannot rely upon them as a practical eugenic policy. We may regret that it is not within our power to combat mental and moral deficiency in this way, yet we must rejoice on the contrary that the effects of environmental demoralisation will not appreciably, if at all, afflict the inborn quality of future generations. To the practical man, therefore, the field for eugenic action is limited to the encouragement of the well endowed and the discouragement or prevention of the defective, in handing on their qualities to future generations. (Chaps. v., viii.)

A point of the greatest practical importance developed in chapter x is the distinction that must be drawn, both in our aims and in our methods, between the single and the multiple factor characters. In man, a number of characters are known, for the most part rare defects, and in no known case a desirable quality, which depend upon a single mendelian factor, dominant or recessive. Of these, the most important is feebleness of mind, if the view be true, of which there is considerable but by no means conclusive evidence, that a large proportion of the feeble in mind owe their defect to a single mendelian recessive. The other cases, though often serious personal afflictions, are sufficiently rare to be comparatively unimportant in relation to the eugenic progress of the population as a whole. On the other hand, the majority of important human characteristics, which distinguish the valuable or desirable citizen from the undesirable, and in particular those which distinguish the gifted from the mediocre, are certainly due to a number, and probably to a considerable number, of separate heritable factors. These show a type of inheritance similar to that of human stature, the study of which, by biometrical methods, provides abundant evidence that the inheritance is in reality due to a large number of mendelian factors, each having effects so small compared to the general variation that they cannot be individually recognised. In the case of defects due to a single factor it is

possible, in any case of sufficient importance, to proceed by individual selection; the number of afflicted individuals is small compared with the general population; the defect can usually be recognised with certainty, and in the case of dominant defects depends for its continuance solely upon the procreation of defective parents. A dominant defect can, therefore, be abolished without serious difficulty in the course of a single generation, and even with recessive defects, where the taint is carried by a much larger number of normal than of defective individuals, the decrease of its incidence which can be certainly achieved is more rapid than is usually imagined.

Of much more importance to the population as a whole are those characters of body and mind which depend upon a large number of heritable factors. In these cases the factors are not, individually, recognisable, and individual diagnosis can only tell us the degree to which a desirable or undesirable quality is developed. The undesirable factors will be spread with varying concentration throughout the whole population and, though extreme types may be recognised, any practical influence which we can exert upon the birthrate of these rare and extreme types will have disappointingly little influence upon the average degree in which the character shows itself in future generations. The effect of encouraging a small degree of additional fertility among men of genius is aptly compared to the effect of distributing among the general population the wealth of a few millionaires, a procedure which would certainly lead to general disappointment. With multiple factors we are always led, in fact, to attach too great importance to levels of ability which attract attention by their rarity; whereas our attention ought to be concentrated upon the great body of citizens, somewhat above and somewhat below the general average respectively. If, in practice, we are considering the eugenic or dysgenic effects of legislation which may affect the fertility of whole classes, it is classes not very much above or below the average of the general population in social status that are of the greatest eugenic importance. The two levels to which, on theoretical considerations, Major Darwin calls especial attention comprise, on the one hand, the elementary school teacher, the highly paid artisan, and the better-paid foreman; while, on the other hand, the class of the greatest importance below the general average is roughly represented by the urban labourer. Any legislative action, or social tendency, which affects fertility at these two levels may be of immense importance to the hereditary endowments of the nation in the future; and it is because the differential birthrate is not a phenomenon of the social extremes, but influences the great classes into which the mass of the population may be divided,

that its consideration must long be of overwhelming importance in all questions of eugenic reform.

It is necessary to pass over with a bare mention the most interesting chapter (xvii), in which the magnitude of the economic burden which the less fit of the nation throw upon the efficient citizens of every class, and in which some of the ramifications are traced by which this burden is distributed ; nor can we say more of the chapters on special social types, the feeble-minded, the criminal and the insane, than that the reader of any of these chapters will, before long, light upon some new thought, which on reflection will strike him as singularly well considered.

The central problem of the elimination of the less fit, with its equally difficult counterpart, the encouragement of multiplication among the more fit, is tackled in chapters xxi and xxii. Here the casual reader may gain an impression of vacillation, though my final impression is that the author is steering a narrow and tortuous course amid real difficulties. First, it is argued that any effective control over the rates of multiplication of different sections of the people can only be exerted through family limitation—a conclusion it is difficult in present circumstances to avoid, though perhaps more weight could be given to those agencies which influence the frequency and age of marriage. Next, it is shown that voluntary limitation involves, and must always involve, certain dysgenic selective elements, and that in the immediate past as well as at the present time its influence has been in a high degree dysgenic. As to the effects of a further spread of these practices, it is shown that this must involve both advantages and disadvantages to the race. The author, for once allowing his convictions to draw a firmer conclusion than the argument he presents seems to warrant, inclines somewhat strongly to the view that an extension of contraceptive practices would now tend somewhat to mitigate the evil. Finally, it is urged that purely voluntarily family limitation can never be eugenic in its effects, but might become so if reinforced by some measure of pressure or compulsion. Such pressure would, of course, be applied only to the minority of extreme cases, but might act as a deterrent throughout a considerable body of the less efficient citizens. The machinery proposed would be based upon the receipt of an exceptional amount of relief through the poor-law or charitable sources, and would consist of warnings where it seemed probable that a family of more than two would be produced without the means of self-support in tolerable conditions. Such warnings, if disregarded, should be followed by an actual segregation of the defaulters. " It will be said, and very likely with truth, that any such reforms as are here suggested are utterly Utopian.

But, if this be so, I hold that to hope to prevent the decay of our civilisation is Utopian also " (p. 388).

With respect to the multiplication of the more fit, it is a matter of particular personal gratification to the reviewer that Major Darwin has accepted the principle that one of the causes of the low rate of multiplication of the upper and middle classes lies in the continual social promotion of persons and families characterised by hereditary tendencies favouring low fertility, and that the most important of these tendencies lie in the mental and moral characters favouring, on the one hand, late marriages, and on the other hand family limitation. Nevertheless, he gives reasons for some confidence that a determined campaign, based upon both patriotic and religious sentiments, should have a considerable and lasting success in checking the elimination of superior types ; while the concluding pages of chapter xxii, in which he summarises the moral considerations which should weigh with all right-minded persons in this matter, should be very carefully read, especially by those who share the too prevalent opinion that Eugenics is essentially a birth-control movement.

Of the economic reforms by which such a campaign should be supported there are several valuable suggestions, among which may be noted reforms in the allocation of state-aided scholarships, in the extension of relief from income-tax to parents, and, by far the most important in the scope of its application, the institution of family allowances.

The last topic will certainly provoke increasing discussion and consideration in this country, and it is as well to be put on our guard that the eugenic consequences of the different possible systems may be of sufficient magnitude to outweigh even the economic aspect of the question, important as this latter is. It is much to be feared that, unless the great body of educated opinion informs itself rapidly, and from impartial sources, on this important movement, schemes may be framed in disregard of the racial consequences and an opportunity lost of performing, for the benefit of future generations, a service of the first magnitude.

**DELINQUENTS.** By  $\beta_6$ , being a review of **The Young Delinquent.**  
By CYRIL BURT, M.A., D.Sc. (Oxon). [Pp. 20 + 223, with 24 illustrations.]  
(London : University of London Press. Price 14s. 6d. net.)

THIS book is an extremely interesting study of young delinquents. The offences committed comprise, for the most part, such breaches of the law as would be punishable in an adult by penal servitude or imprisonment. There is, as Dr. Burt points out, no sharp line of cleavage between the delinquent

and the non-delinquent ; the moral faults of children run in an uninterrupted series, from the most heartless and persistent crimes to occasional naughtiness. Delinquency is regarded by the author as, at the bottom, a social rather than a psychological concept. The study is of 123 boys and of 74 girls between the ages of 18.0 years and 7.0 years for the girls and 5.0 years for the boys, and there is a control series of 400 children. For this reason alone this work is ahead of much previous work on delinquency ; there have been many studies of delinquents, but though the need of a control series has been noted it was not provided. Dr. Burt has taken 400 children belonging to the same age groups, the same social class, living usually in the same streets and attending the same schools as the delinquents, and these children were tested, medically examined, and reported on by teachers and visitors in the same way as the delinquent children. The surroundings of the child were considered under various headings, hereditary, environmental, physical, psychological. In considering hereditary conditions, Dr. Burt dealt with the father and mother of the child and its near relatives and considered their condition under the headings, physical, intellectual, temperamental (pathological symptoms) and temperamental (moral symptoms). The first three can hardly be regarded as hereditary with regard to delinquency in the children ; the first would be an hereditary characteristic if the physical condition of the child were being considered and the second if the intellectual condition of the child were under consideration, but it seems rather a pity to include these in an estimate of the force of heredity when the actual factor under consideration is delinquency. An examination of Table III, p. 53, will show that the inborn emotional character of the child is more closely associated with delinquency than any other physical or psychological condition ; it is this character in the child that corresponds most closely to what is described in the parents as temperamental with moral symptoms, and this alone is an hereditary condition in the strict sense of the term. Height and eye colour are hereditary in the same way as the physical condition of the parent, and one would not anticipate that either of these characteristics would be associated to any extent with delinquency ; but add them to the list of hereditary influences and the average result would be still further decreased until in the end, by multiplying such factors, one could say that heredity was of no importance in causing delinquency ; but this is not the view held by Dr. Burt. He considers that crime in itself is not inherited, and that the hereditary constitution of the criminal has at the most an indirect effect, but, at the same time, he considers that where inborn or hereditary defects are prominent the outlook for

reform is not hopeful, and the more powerful the part played by hereditary factors, the less is the likelihood of successful reform. Intellectual faculties in the parents are associated with youthful delinquency, but this again is an indirect effect of heredity, and exists because there is an association between the intellect of parent and child and between the intellect of the child and delinquency.

In considering environmental conditions, Dr. Burt does not regard poverty as a factor of great importance in producing delinquency ; a certain amount of youthful crime may follow poverty, but it is unlikely to do so unless there is something lacking in the child or in the home. Defective discipline at home is the environmental condition most closely associated with delinquency, and Dr. Burt finds this the most important factor of any in producing youthful crime. Emotional inborn psychological condition comes next in importance and then inborn general emotionality. One wishes throughout that the actual figures on which the percentages are based were given, as without them it is difficult to know what weight to attach to the differences in percentages which are found. It will be seen in Chapter vii that the young delinquent is below his fellows in inborn ability and in educational attainments, but Dr. Burt does not find such a high percentage of mental defectives as has been found among adult criminals by Goring or frequently in American statistics, though he finds an excess of the dull and backward, and suggests the advisability of special classes for these children and an extension of the principle of guardianship or establishment of hostels for dull delinquents.

The young delinquents are very little inferior in their pathological physical condition to the children in the control series, but differ from them in their developmental physical condition, showing, for example, an excess of premature puberty.

In Chapter viii the delinquent with special abilities is considered and various cases are described, and it is shown how these young delinquents can be helped and given a chance.

Moral tests are still very largely in the experimental stage and Dr. Burt believes in the personal interview, though he uses tabular schedules and rating-scales and has his own set of questions for use. There is a very interesting and helpful section on children's lies which will appeal to anyone whose work is among children.

Throughout the book details are given of many cases, and one realises the amount of work expended in collecting the information and the amount of sympathy and consideration expended on each case in order to decide on the method of treatment and how far that treatment can be carried out.

In Chapter xiv Dr. Burt gives his conclusions ; he regards crime as being due to a multiplicity of factors of which one major factor can generally be discovered ; on p. 606 he gives fifteen factors in order of their importance, and of these the first seven, judging by their coefficients of association, seem to be of practically equal importance. Dr. Burt points out (1) that crime in the young must be handled by police, magistrate, care committee workers and teachers working in conjunction with all the voluntary and public agencies that seek to better the life of the child ; (2) that the youthful delinquent must be treated as an individual and that the physical, mental, and psychological conditions of that individual must be studied and the home conditions considered before the remedies can be applied. He pleads for fuller knowledge both of the causation of crime and of the efficacy of different remedial measures and of a great extension of after-care when the young delinquent is returned from institution to society.

This book can be recommended heartily to all interested in young people as well as to those concerned with young delinquents, even though one cannot agree with all the conclusions reached.

B.

#### **MOSQUITO-CONTROL: THREE BOOKS BY ENGINEERS.**

By SIR RONALD ROSS, being a Review of:

(1) **Mosquito-control in Panama**, by JOSEPH A. LE PRINCE, C.E., A.M., and A. J. ORENSTEIN, M.D. [Pp. xvii + 335.] (New York and London: G. P. Putnam's Sons, 1916) ;

(2) **Mosquito Eradication**, by W. E. HARDENBURG, M.A.A.E. [Pp. ix + 248.] (New York and London: McGraw-Hill Book Company, 1922) ; and,

(3) **The Engineer and the Prevention of Malaria**, by HENRY HOME, M.Inst.C.E. [Pp. x + 176.] (London: Chapman & Hall, 1926. Price 13s. 6d. not.)

MEN have always been so proud of their superior intelligence that they attribute a supernal origin and destiny to themselves in consequence of it ; but the philosopher who remembers many instances of their obtuseness tends to admit neither the fact nor the deduction. Among other examples there is the case of their surprising subjection to the crowds of vermin which have infested their persons, their houses, their cattle, and their crops from time immemorial, and still do so ; and scarcely ever, from Sulla and Herod to the present, have they even attempted any general measures against these petty enemies of theirs. Such indifference to their own comfort and prosperity shows how little the reason of which men boast has really governed their actions ; and only recently, since science has proved that both the vermin and their victims are

often the alternating hosts of still lower classes of parasites which often destroy us by millions, are we beginning to realise how much all kinds of vermin-control really mean for us.

Among the worst of these vermin are mosquitoes—which have probably tormented men or their progenitors, with little risk to themselves, from the beginnings of time, and are now known to carry three or four of the most terrible diseases which which maim or kill us. Regarding the mere annoyance caused by them, scarcely any efforts to control them seem to have been made before the present century. Mosquito-nets (*conopseum*) were used by the ancient Romans, and, more recently, by other races, and some local efforts at mosquito-control were attempted last century in America. When I arrived in India in 1881 no one appears ever to have even dreamed of reducing mosquitoes there, and we were bitten unmercifully day and night everywhere, unless we placed ourselves under punkas or bed-nets. The first attempt at control known to me or ever heard of by me, in India at least, was in 1883, when I banished most of the *Stegomyia* and *Culices* from my bungalow in Bangalore simply by upsetting the tubs and vessels of water in which they were breeding; and I always adopted similar measures wherever I lived subsequently in India and the tropics (see my *Memoirs*: Murray, 1923).

In 1877 P. Manson traced the early development of *Filaria bancrofti* of man (elephantiasis) in mosquitoes—the first incrimination of these insects. Twenty years later, in 1897, I cultivated one of the malaria parasites of man in its earlier stages in two species of *Anopheles*. Next year I demonstrated the whole type life-cycle of a malaria-parasite of birds in a *Culex*; and in 1899 I demonstrated all three species of the human malaria-parasites in *Anopheles costalis* and *A. funestus* in Sierra Leone. At the end of 1900 W. Reed and his associates proved at Havana that yellow fever is carried by *Stegomyia fasciata* (*Aedes calopus*), and subsequently dengue has been also traced to mosquito-carriage. These first observations have now been confirmed and extended by many workers in many countries.

I do not think that Manson ever even suggested mosquito-control against *Filaria*; and apparently this measure was first publicly advocated as a sanitary principle by myself in a report to the Government of India (*Indian Medical Gazette*, July 1899). In my Inaugural Lecture at Liverpool, published in the same month (*Brit. Med. Journ.*, July 1, 1899), in my series of four (anonymous) reports to *The British Medical Journal* (September 9 to October 14, 1899), and in my subsequent report on our visit to Sierra Leone (University Press, Liverpool, February 1900), I enlarged much on this theme and

advocated mosquito-control against malaria in all suitable localities, especially towns. Early in 1901, after some research expeditions from Liverpool had failed to stimulate action, I raised private funds to commence a mosquito-control campaign myself, and left England in June, with M. Logan Taylor, for Freetown, Sierra Leone, where we speedily reduced the pests within the area dealt with. Meanwhile the yellow-fever discovery at the end of 1900 stimulated the Americans to commence similar work in Havana, which they did early in the same year, 1901, with large public funds behind them. The same year, also, I proceeded to Lagos and the Gold Coast for the same purpose; and mosquito-control work was started in Hong-Kong, the Federated Malay States, New York, and several other places; and in 1902 Ismailia was finally cleared of mosquitoes by the Suez Canal Company, at almost no expense, after my visit there. In the meantime I had published two little works, namely, *Instructions for the Prevention of Malarial Fever* (University Press, Liverpool, 1899, with many subsequent editions), which is still selling, and *Mosquito Brigades and How to Organise Them* (G. Philip & Son, London and Liverpool, 1902); and my large book, *The Prevention of Malaria*, with contributions by twenty leading workers on the subject, was first published in 1910 (Murray, London). Since leaving India in 1899, I have paid twelve visits to malarious countries on this quest, and the letters and articles which I have written on the subject would fill several volumes. Nevertheless, many large areas and towns in the tropics remain apparently just as full of malaria and mosquitoes as they were a quarter of a century ago; and up to the present, I fear, mosquitoes have won the war against humanity, except in a few small tracts. In one direction, however, progress during this period has been most praiseworthy—in the classification and study of mosquitoes themselves and their anatomy and habits, commenced in 1900 by Giles, Theobald, Howard, Christophers, and others, followed by a host of capable workers in many countries. The mere knowledge that mosquitoes may communicate diseases has led the more civilised towns and householders to reduce them to some extent in their own neighbourhoods; for example, I was not conscious of having been bitten once, during a recent six-weeks' stay in Ceylon, either in hotels or in private houses. Lastly, in the exceptional tracts where control has been thoroughly tried, it has been most successful—Ismailia, Havana, the Federated Malay States, Khartum, Panama, Port Said, several American cities, and many smaller areas; and the time will arrive, though slowly, when it will be adopted in all the principal centres of population, at least—just as I advocated twenty-seven years ago.

A very large number of publications on the subject have appeared since that time ; and doctors, parasitologists, entomologists, engineers, and even mathematicians have worked on special details. It cannot be said, however, that any very new facts have emerged. The broad rule that *Anopheles* breed chiefly in terrestrial waters—unlike the domestic *Culex* and *Stegomyia*—has been generally supported ; but, as we showed first in the case of *A. funestus* in Sierra Leone (1899), they can often live in running water ; and a few species sometimes affect cisterns and water-holding plants. In the same year I inferred, hypothetically, that because of these terrestrial habits, malaria, which has long been known to abound under marshy conditions, may be carried only by *Anopheles*, in two species of which I had found the human parasites in 1897 ; and in no case have these parasites been found yet in any other genus of the Culicidæ. We owe to Koch, Stephens, and Christophers the fact (which I had overlooked) that native children are the principal “ reservoirs ” of the infection. The actual species of *Anopheles* which carry malaria have been incriminated by much careful microscopical work in many countries ; and their specific bionomics have been recorded and compared by many observers. It has been found (as was indeed obvious from the first) that mosquito-control requires, or may require, the combined efforts of public health officials, entomologists, and engineers.

This brief history of the subject has been given here because none of the three books under review contains one. They are all technical works, chiefly on the engineering side of the subject. The smaller breeding-waters (pots, tubs, etc.) are generally dealt with by local sanitary staffs ; but the larger ones (streams, pools, marshes, etc.) often require the services of trained engineers ; and some problems (rice-fields, wells, borrow-pits, etc.) occasionally foil the efforts of both, at least when considerable funds and difficult legislation are required. It is important, therefore, that engineers should have textbooks to help them in such work ; but their troubles are more often due to economic or administrative causes than to natural ones, such as levels and soils. All the three books will, or may, assist them ; but common sense should be their strongest ally ; and they must always be guided by biological studies of the pests from which they endeavour to protect the public.

The first book (1) contains one hundred illustrations, mostly photographs, and an introduction by that distinguished entomologist, L. O. Howard. Mr. J. A. Le Prince, the leading author, was Chief Sanitary Inspector to the Panama Canal Commission from 1904 to 1914. He was starting his labours when I visited the Canal Zone in 1904, by request of the American

Government, and showed me the work then commencing. Of course, with their large funds, the Americans could do much more at Panama than I was able to attempt in Freetown, with privately collected money. Colonel Gorgas was not in Panama during my visit, but had kindly said farewell to me when I left New York for the purpose. The Americans, with unlimited funds behind them, were using not only mosquito-control, but also complete wire-gauzing to the houses of all operatives, and also street-rectification, quinine, and in fact everything that could be employed against mosquito-borne diseases. The work (1), therefore, should be a very thorough essay on mosquito-control, both against malaria and yellow fever, and should certainly be in the hands of everyone undertaking such labours. Several details which we could not employ, such as extensive pumping, concrete drains, silt-submergence of dangerous areas by means of dredged mud from creeks, were used. Attempts to measure the flight of *Anopheles* and their numbers by traps (which I remember I suggested at the time) were also tried and reported on, as well as various "culicifuges" and "culicicides." The magnificent results finally obtained are known to everyone; but the book contains no history of the subject of mosquito-control and no bibliography. The authors say on page 5 that "no antimalaria work had been attempted elsewhere previous to the Cuban anti-malaria campaign. Ross started his practical field of work in the East at the same time that work was begun in Havana." This is true, but it was not my fault. We did our best for Sierra Leone, but the authorities gave little help before 1901, and in fact only employed one native "mosquito-man" for the whole town! On the other hand, I had produced no less than five papers or books advocating mosquito-control (as stated above) from July 1899 to February 1900; and when the authors go on to state that "there was no previously acquired information available on the subject nor any known practical methods that could be followed or that could assist those in charge of operations," surely they have overlooked all these publications. When they add that "at that time very little was known regarding the habits and life-history of *Anopheles*" they again ignore the same publications. We already knew the broad distinctions between the breeding habits of *Anopheles*, *Culex*, and *Stegomyia*, since 1898, or earlier. My friends, the Americans, tell me that they were not acquainted with these papers of mine, but as a whole series of articles by me had appeared in *The British Medical Journal*, in September and October 1899, and as I had sent a report of our Sierra Leone work to Gorgas in 1901 (see his letter to me in my *Memoirs*, page 453), we can only infer that our contributions

had remained unread. Even my big book, *The Prevention of Malaria*, which preceded (1) by six years, and contains articles by Gorgas and by Le Prince himself, is only mentioned in it in a foot-note in connection with larvacides, on page 151. The same book of mine (second edition, now sold out) is not mentioned at all in the two other books under review. I suppose that the mathematics which I used to demonstrate the laws of mosquito-diffusion were, perhaps, too difficult for the authors.

The second work (2) aims at being a more general *vademecum* on mosquito-control. It supplies more details and plates regarding mosquitoes for the use of engineers and laymen, but does not contain much more than (1) does, except perhaps on some smaller technical points, such as specifications for sluice-gates and details of inland-drainage, of tree-clearing, ditch-blasting, etc. There is a chapter on "fish-control"—that is, the use of fish for destroying larvæ. This work does contain a bibliography, but it is almost entirely confined to American papers. It is amusing to see many older and familiar observations and statements made elsewhere attributed to much later American authors as being original with the latter. There are 146 figures, mostly photographs. The mosquito-control campaigns referred to are numerous and interesting, but almost entirely American ones only. No reference is given to my very numerous papers on the subject, nor to the campaigns in the Federated Malay States, Ismailia, Italy, or Port Said.

The third work also contains no bibliography, and the historical references are still more brief and incorrect. We are told that "the labours of a number of investigators, Grassi, Laveran, Manson, whose research is specially mentioned, culminated in the discovery by Ross, in the year 1898, that the cause of malaria is a blood parasite, which is always carried from man to man through the medium of certain mosquitoes." I am afraid that I must deprecate the highly flattering reference to me, since it was Laveran who discovered the cause of malaria in 1880. One also wonders what the shades of Laveran and Manson would think if they could see their names preceded by that of the ingenious Grassi, who is put in the first place over their heads!—see my article in *SCIENCE PROGRESS*, for October, 1925, page 311. Nevertheless, this book may also be useful to workers, especially as regards the classification of lands in connection with drainage problems. There is a brief chapter with illustrations on mosquitoes, but it is scarcely detailed enough for practical work; and there are some useful appendices, especially one on the  $P_h$  values of water and another on mosquito netting. There is an index but no bibliography, and the works (1) and (2), not to mention my very humble but copious

monograph of 1911, or Malcolm Watson's fine book (Murray, 1921), are not even referred to. I can find no reference to the first campaigns against mosquitoes previously mentioned. The plan of Clairfond Marsh in Mauritius, opposite page 23, was made under my orders and appeared first in my report of 1908, but is attributed to the officer who assisted me there. The publishers tell us that the author gained his experiences in America, the West Indies, Africa, Egypt, Syria, etc., but not in what capacity he did so. We can scarcely expect engineers to be learned in biological and hygienic literature ; but still the authors of these books might have read more on their subject before professing to teach others regarding it.

## REVIEWS

### PHYSICS

**Crystalline Form and Chemical Constitution.** By A. E. H. TUTTON, F.R.S.  
[Pp. xii + 252, with 72 illustrations.] (London: Macmillan & Co.,  
1926. Price 10s. 6d.)

WITH Laue's discovery in 1912 of the diffraction of X-rays by crystals commenced a new era in the science of crystallography. Before 1912 may be said to comprise the period of classical crystallography; subsequent to that date the period of modern structural crystallography. Dr. Tutton was a prominent figure in the classical school and justly famous for the remarkable accuracy of his results in the long and laborious researches which he has been so courageous as to undertake. Few men could have maintained sufficient patience for this work, but Dr. Tutton has had the satisfaction of seeing his convictions verified by X-rays. From his careful measurements of isomorphous series he formed the conclusion that something very like the ordinary chemical molecule must be considered to have a separate existence in the crystal. He estimated the relative dimensions of the respective unit cells and, from the progressive changes which these undergo, the probable orientation of the molecules. On the discovery of the diffraction of X-rays by crystals, he hastened to make himself conversant with the new methods—unfortunately, this cannot be said of all crystallographers of the old school—and since then has always included in his writings accounts of recent progress in modern structural crystallography.

Dr. Tutton's writings have always this characteristic, that they endeavour to combine the viewpoints of the old and the new. *Crystalline Form and Chemical Constitution* is no exception. It is written in the author's well-known pleasant and enthusiastic style. A large part of the book deals with the author's research on the isomorphism of the alkali sulphates and selenates, etc., and with the apparatus which he designed for carrying out the work in all its phases—material with which readers of his other books will be more or less familiar. In fact, this smaller volume, the substance of a series of lectures delivered at Cambridge, may be said to be an extract of the chemical-crystallographical sections of the larger and more technical volumes, brought up to date as much as is possible within the scope of the book. This does not mean that anything like an exhaustive account of recent results is given—rather only such crystals are discussed as the author considers necessary to illustrate his argument. The first chapter of the book is a concise crystallographical introduction to the sequel. It is to be regretted that in this introduction accuracy is sometimes sacrificed to conciseness. Certain corrections are necessary.

The study of structural crystallography is advancing by leaps and bounds, and as a consequence of this it is difficult to make hard-and-fast statements about many intriguing questions, however much we may feel desirous of having a settled opinion. Much that was held to be probably true a few

years ago is now doubted. To take only two points of which Dr. Tutton speaks confidently: (1) that no case has yet been observed in which the number of molecules per cell is greater than the symmetry number; and (2) that there are four electron orbits all alike in carbon as it exists in diamond. Quite a number of cases have now been observed which do not agree with (1), and with regard to (2), it is difficult to feel happy now that spectroscopists are beginning to believe that—at least, under spectroscopic conditions—the four electron orbits of carbon have not after all a tetrahedral symmetry.

To workers in X-ray crystal-analysis there are many distressingly obscure problems awaiting solution, and it becomes almost impossible to discuss them adequately in a volume of the size of the one under review, much less in the review itself. The present writer, while sharing the enthusiasm and optimism that Dr. Tutton feels with regard to the future of chemical and physical crystallography, yet finds himself unable to associate himself with certain of the opinions which the author expresses in *Crystalline Form and Chemical Constitution*. There seems little doubt that most molecules pass nearly unchanged into the crystal state, yet, though the distortion they suffer be ever so small, they can *not* be said to pass with unchanged *symmetry*. Rather experiment indicates the direct opposite—that in most cases there is just enough distortion to make the molecules less symmetrical and often asymmetric from the purely geometrical point of view. It is this circumstance which makes crystallographic research in complex crystal structures so difficult and perplexing—in fact, the great problem before us now is to estimate the *approximate* symmetry in such cases. Chemistry will often tell us what may be described as the “free” symmetry, but what is the precise nature and extent of this annoying small distortion which takes place on crystallisation? This is the question that has to be answered in individual cases before we can feel at all happy. Of course, the solution lies in a solution of the intensity problem, but this latter is still baffling in the extreme for all but comparatively simple cases.

It is difficult, too, to maintain that a chemical molecule, as the term is generally understood, can always be said to exist in crystal structure. There is no really conclusive experimental evidence on this point, and space-group considerations do not allow us to point to certain details of structure in the so-called “ionised crystals” and say, “Such and such are chemical molecules having a distinct and separate existence in the crystal.”

Messrs. Macmillan are to be congratulated on the production of a very handsome volume.

W. T. A.

**Marvels of Sound, Light, and Electricity.** By P. G. BURL, M.A. [Pp. vii + 221, with 100 diagrams and original photographs.] (London: George Routledge & Sons. Price 6s. net.)

WHEN the reviewer received this book he hoped to have the pleasure of reading an accurate account of the laws and phenomena of sound, light, and electricity, which would appeal to young students and general readers. Unfortunately, however, the book is not characterised by any remarkable genius of exposition, and a number of inaccuracies are only too apparent. For example, the author includes electrons in a diagram showing the scale of ether waves. Many diagrams are faulty, in particular that of the valve, and those of X-ray tubes.

L. F. B

## SOME NEW EDITIONS

**Exercises in Practical Physics.** By SIR ARTHUR SCHUSTER, F.R.S., and PROF. C. H. LEES. [Fifth Edition. Pp. ix + 373, with 136 figures.] (Cambridge: University Press, 1925. Price 12s. 6d. net.)

**The Dynamical Theory of Sound.** By HORACE LAMB, F.R.S. [Second Edition. Pp. viii + 307, with 86 figures.] London: Edward Arnold & Co., 1925. Price 18s. net.)

**General Physics, and its application to Industry and Everyday Life.** By ERVIN S. FERRY. [Second Edition, Revised. Pp. xix + 807, with 600 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1925. Price 20s. net.)

SCHUSTER and Lee's *Practical Physics* is a book so well known in the physics laboratory that, in reviewing the fifth edition, it is hardly necessary to do more than indicate the chief changes that have been made since it was last reprinted in 1915. In the chapters devoted to General Physics, much elementary theory has been omitted, and "falling drop" method for the measurement of surface tension inserted as an addition to the capillary rise and balance methods. This addition is really somewhat astonishing, and was possibly added in a moment of weakness for the benefit of the chemists! In the section on Heat, hygrometry has been omitted, and no important additions have been made, so that the section still lacks a description of any method for measuring thermal conductivity—a surprising gap in view of the well-known work by Prof. Lees on that subject. The short section on Sound has not been lengthened, but a few small additions appear in Light; e.g. Cheshire's focometer method for measuring the focal lengths of converging lens systems is given, a description of the usual Newton's Rings experiment has been added, and the measurement of resolving power replaces that of magnifying power.

In Magnetism the experiment describing the magnetic survey of a laboratory is replaced by one dealing with the magnetisation curves of iron. In Electricity the experiments designed by Kelvin and Mance for the measurement of galvanometer and battery resistance remain, but the Carey Foster-bridge experiments have been replaced by one involving the use of shunts in accordance with modern practice. Experiments have been added dealing with the standardisation of the ballistic galvanometer, the comparison of self and mutual inductances, the characteristic curve of the triode valve, and the quadrant electrometer.

These alterations are all a matter of detail and do not in any way affect the general character of the book, which provides a reasoned account of a small but adequate number of experiments of a fundamental character well covering the syllabus of the pass degree. It is noteworthy that, in printing this new edition, the publishers have used new and very pleasant type, an example which might be followed with great advantage by others of their craft.

Prof. Lamb's treatise on Sound is almost entirely mathematical in its scope, and is intended to serve as a stepping-stone to the writings of Lord Rayleigh. It forms an outstanding example of the manner in which the mathematics of a science may be presented so as to be intelligible to the physicist, who is chiefly concerned with its experimental side. The book may be read in comfort (if not in an armchair) by many who can only admire Rayleigh's classic. The new edition remains substantially the same as the last. A few errors have been corrected and a few paragraphs dealing with matters which have come into prominence during the last few years have been inserted. Such additions include brief references to the acoustic properties of buildings, to the propagation of explosion waves, to double

resonators, and to the hot-wire microphone. A companion textbook dealing with the experimental side of the subject is now needed very badly, for there is no connected account of modern experimental work available to the English student.

It is now nearly five years since the first edition of Prof. Ferry's textbook of *General Physics* was published, and continued reference to it during that period has served to confirm the favourable impression which it produced on the reviewer when it first appeared. It is unfortunately not well suited to any ordinary examination syllabus in this country, and it is doubtful whether, in consequence, it has been much used in England. Nevertheless, a student working through it would obtain a far better insight into the fundamental principles of Physics than he could obtain from any of our orthodox textbooks, and an incomparably better knowledge of the importance of those principles in civilised life. Its main defect, from the examination point of view, is the absence of any description of laboratory practice, *e.g.* one seeks in vain for any experiments dealing with the measurement of the mechanical equivalent of heat or the expansion of a liquid. In spite of such things the book contains a thoroughly scientific exposition of its subject, and is full of points calculated to make the student think.

The new edition has been very largely rewritten, and the collection of numerical questions at the end now contains 1,440 problems, occupying no less than 150 pages of print. The section on radio-telephony has, of course, been extended very considerably, and the examples of the industrial applications of physics include many which have only been devised (or, at least, become known) during the last five years.

Prof. Ferry writes in a pleasant style free from American idiom, and his book is worthy of perusal by every teacher of physics. The price has been reduced from 24s. to 20s. D. O. W.

**The Fundamental Concepts of Physics in the Light of Modern Discovery.**  
By PAUL R. HEYL, Ph.D. [Pp. xii + 112.] (Baltimore: The Williams and Wilkins Co. Agents: Baillière, Tindall & Cox, London. Price 9s. net.)

It is unfortunate that this little book, which is so certain to give much enjoyment to the general student of physics, is practically beyond the reach of the English public on account of its high cost. It is difficult to understand why such a small volume, even if it be *sans tache*, is priced at nine shillings net.

In this book Dr. Heyl reproduces, in a convenient form, three lectures which he delivered at the Carnegie Institute of Technology, Pittsburgh, in 1925. We are thus able with his guidance to view the progress of our science through the materialistic glasses of the eighteenth century, then, with him, we witness the correlation of the several concepts inherited from that century, together with the introduction of the concept of energy, and, further, we enjoy his survey of the work accomplished in this brief portion of the twentieth century through which we have lived. There is a bright vein of optimism running through the whole book, and we can heartily assure Dr. Heyl that we are not too grown-up to enjoy the delicious fairy story with which he closes his final lecture, and we wish his book were likely to have a wider circle of readers than that to which its present price will limit it.

L. F. BATES.

**Vacuum Practice.** By L. DUNOYER. Translated by J. H. SMITH. [Pp. x + 228, with 80 illustrations.] London: G. Bell & Sons, 1926. Price 12s. 6d. net.)

MESSRS. BELL are doing a real service to science by their publication of translations of foreign scientific works, and they are fortunate in their choice

of the first work, because great progress has been made in high vacuum technique during recent years. The advance of atomic physics has been assisted by the ever-increasing perfectness of vacua and, on the other hand, the theoretical development of the subject has aided and encouraged the design of the extremely ingenious devices used in the production and measurement of these low pressures.

The principal object of this practical manual is the study of the technique, and although the theoretical treatment is abbreviated, there is at the end of the volume a full bibliography of papers dealing with the various phenomena associated with the subject. The references quoted are commendably complete, and practically all of the important work in this branch of science is given some mention.

The different types of mechanical, molecular, and mercury vapour pumps are well described and illustrated in the first chapter, and the physicist or engineer will find no difficulty in selecting a pump for his own particular requirements; but the efficient all-metal pumps designed by Kaye, Gaede, and Parsons, as well as a simple type of mercury aspirator, might with advantage have been included in this section.

The second chapter, dealing with the various gauges and manometers used in measuring very low pressures, contains a detailed account of the difficulties encountered with the often condemned, but extensively employed, McLeod gauge. As this manometer is still the standard instrument for the practical measurement of high vacua, the extended treatment accorded will prove helpful to all vacuum workers. This chapter emphasises the still unsatisfactory state of such measurements. There is at present no absolute manometer which is efficient, easy to construct and calibrate, and suitable for use with all gases and vapours.

The problem of eliminating occluded gases and vapours is, probably, the most troublesome and variable one encountered in the process of evacuation, and one welcomes, therefore, the full and interesting account, given in the third chapter, of the special difficulties met with in driving off such gases and vapours from the walls and contents of vessels. The various cements and greases which may be used for vacuum joints are described. There seems to be no serviceable cement of negligible vapour pressure which is able simultaneously to withstand a high temperature and maintain a vacuum.

The improvement of vacua by absorbers and by the electrical discharge is dealt with in the last chapter, but the small amount of space devoted to adsorption by charcoal would not suggest that this method of maintaining low pressures is very important in the case of liquid air containers.

This book will be welcomed by all interested in the subject. The ground covered is of considerable extent, the treatment is lucid, and the volume abounds with practical information. The expression of pressures in terms of mm. of mercury instead of in the less familiar term  $\mu$ , the inclusion of a list of symbols, and the systematic numbering of tables, in future editions, should prove helpful to many readers.

F. H. N.

## CHEMISTRY

**Introduction à l'Étude des Colloïdes.** Par W. KOPACZEWSKI. [Pp. vii + 226, with 36 figures and 2 portrait plates.] (Paris, Gauthiers-Villars et Cie, 1926. Price 16 frs. net.)

THE object of this work, as stated in the preface, is the diffusion of knowledge concerning the colloidal state of matter in scientific as well as in industrial circles. This object, the author considers, is best achieved by presenting well-established facts, grouping them, and indicating the applications of the new science. "All interpretations of the experimental results

have been omitted, all hypotheses and theories concerning colloids passed over in silence."

Sixty-five pages are devoted to the description of such established facts and the balance to applications. This curious ratio appears to be becoming a feature of works on colloids, more particularly of those of transatlantic origin, and must be accepted by the reviewer, even though he finds it difficult to decide whether the emphasis on applications is meant as an apology or an advertisement for the subject.

Even with all hypotheses and theories omitted, a space of sixty-five pages is not much for an outline of the well-established experimental results, but the author has made the best of it. The applications are treated under two headings: the colloidal state and its industrial applications, and the colloidal state and life. The industrial applications cover a very wide field, the extent of which is indicated by the subheadings: cellulose and derivatives, colours and dyes, tanning materials and leather, resins, casein and milk, gelatin, artificial pearls, humus, clay, asphalt, starch, beer, etc. The treatment is skilful, and the author manages to show with remarkable clearness what problems have been solved, or remain to be attacked, by the methods of colloid chemistry.

In the second part a good deal of space is devoted to the experiments of Stephane Leduc which, however interesting as examples of controlled diffusion and precipitation, can hardly be taken seriously as models of living organisms. The author, a medical man, then treats some subjects foreign to the general reader, such as anaphylactic shock, blood transfusion, etc., at a length which is out of proportion to the scale of the work.

The book appears to be intended largely as propaganda, and may fulfil this object. It contains two portraits, one of F. Selmi and the other of M. Smoluchowski. A small Selmi "boom" has become noticeable of late in other quarters, but the inclusion of the Polish physicist's portrait must have been determined in part at least by patriotic considerations.

E. H.

**Intermediates for Dyestuffs.** By A. DAVIDSON, B.Sc., A.I.C. [Pp. xiv + 256.] (London: E. Benn. Price 36s. net.)

ALTHOUGH popular interest in the synthetic dye industry has now somewhat abated, the industry itself has assumed a more settled condition than was possible during the war and the early days of peace, so that steady progress both in research and production is now the order of the day.

Most text-books on dye chemistry have, of necessity, to limit the space devoted to the intermediate products to the bare minimum, so that Mr. Davidson's summary of the present state of knowledge of these aromatic substances which form the basis of the dye industry, and of the organic chemical industries generally, is, therefore, a useful and timely production, which can be read with profit by all those who have to deal with the manufacture or investigation of these bodies.

On one or two points a little fuller information would have been welcome; thus the eutectic of *ortho* and *meta*-nitro-benzaldehydes (p. 124) contains 40 per cent. of the *meta* isomer and reference might have been made to Dr. Brady's recent paper on the subject; again, on p. 125 details might have been given of the preparation of *meta*-hydroxybenzaldehyde, which are given, for instance, in U.S. Patent, No. 1419695.

All the usual types of substance—nitro compounds, amines, sulphonic acids, anthraquinone derivatives and so on—are adequately dealt with, and many chemists will be grateful to Mr. Davidson for his labours. One could wish it had been possible to produce the book at a lower price, but that is another story.

F. A. M.

**A Comprehensive Treatise on Inorganic and Theoretical Chemistry.** By J. W. MELLOR, D.Sc. Vol. VI, C. (Part II.); Si, Silicates. [Pp. x + 1024, with 221 diagrams.] (London: Longmans, Green & Co., 1925. Price, £3 3s. net.)

It may be imagined that Dr. Mellor has felt thoroughly in his element in this, the sixth, volume of his inorganic chemical encyclopædia, as about seven-eighths of the book deals with silicon and the silicates in all their manifold varieties, so that for most practical purposes it constitutes a textbook of chemical mineralogy.

In the concluding section on carbon (Part II), carbon dioxide is dealt with and the percarbonates, carbon disulphide, thiophosgene, and the thio-carbamates. The information and references are as full as ever, ranging from Pliny's *Historiæ Naturalis* (A.D. 77) to scientific work published in 1924. The simpler hydrocarbons and their derivatives and the cyanides are not included, but may be discussed in a supplementary volume.

As to the bulk of the book, what is one to say? Presumably—the distinguished author being but human—there may be some few faults and omissions here and there, but when one has glanced through a few of the names, such as calcium dimanganialuminumhydroxytriorthosilicate, tetrasodium isotetrahydrosilicododecatungstate, or tripotassium pentahydro-silicododecatungstate one feels there is nothing more to be said or done, except to take a deep breath and start again! The analyses are vouched for by reliable scientific evidence, or else one would feel inclined to agree with the small boy who saw a giraffe for the first time—"I don't believe there are such things!"

For the benefit of those who may intend looking up the literature of the aluminium alkaline-earth silicates it may be mentioned that there are thirty-four pages of references in small type on this section alone.

Vol. VI is fully entitled to share the best shelf with its five elder brethren, and will prove a mine of information to mineralogists and others interested in the vagaries of silica and the silicates.

F. A. M.

**L'Industrie Chimique des Bois, leurs dérivés et extraits industriels.** Par P. DUMESNY and J. NOYER, Second Edition. [Pp. vi + 432, with 105 figures.] (Paris: Gauthier-Villars et Cie. Price 50 frs.)

ONE of the many "discoveries" of the Great War was that a healthy and progressive forest-products industry is an essential for a civilised community both with regard to the substances that may be obtained from the fresh materials, such as dyes and tanning agents, and to those secondary products obtained by processes of destructive distillation.

MM. Dumesny and Noyer have adopted the unusual but praiseworthy plan of including in one volume the available information upon both branches of the industry, with special reference to the needs and requirements of France and the Latin countries; in particular some space is devoted to possible methods for the utilisation of olive residues as a source of acetone and methyl alcohol.

The book tends towards the severely practical side, and the theory is reduced almost to a minimum; to-day when the age-old industry of the charcoal burner is threatened by synthetic organic chemistry it is more necessary than ever that full use should be made of scientific principles; one has only to note the figures for the importation of methyl alcohol into France for 1922, already nearly four times that for 1921, to realise the revolutionary changes which are taking place elsewhere.

The book is divided into two sections, the first dealing with wood distillation and the analysis of the products so obtained whilst the second half

contains details of the more important extracts such as tannins, log-wood extracts, etc. It is well printed and illustrated and should be of value to all those connected directly or indirectly with forestry, wood-distillation, tanning or related industries.

F. A. M.

**A Dictionary of Applied Chemistry.** Edited by (the late) Sir EDWARD THORPE, F.R.S. Vol. VI, S-Acid to Tetryl. [Pp. viii + 791, illustrated.] (London, 1926: Longmans, Green & Co. Price £3 net.)

THE name of "Thorpe" is such a household word in chemical circles that it is a little difficult to realise that the distinguished editor of the *Dictionary of Applied Chemistry* is no longer with us, as he died before the present volume was completed. A publisher's note informs us, however, that much progress had been made with the last two volumes, and the final revision for the press has been carried out by Dr. Foster Morley (who was a joint editor of Watt's *Dictionary of Chemistry* and Director of the International Catalogue of Scientific Literature, so that the mantle of Sir Edward Thorpe has fallen upon the right shoulders, and Dr. Morley's wide experience in other fields stands him in good stead in the difficult task of completing the work of another.

It is hoped that vol. vii, containing the much-needed index to the whole work, will be published before the end of 1926.

Where so much is excellent it is almost invidious to single out special sections, but reference must be made to the valuable article on Starch by Prof. Ling and Dr. Nanji, to Dr. Barger's excellent summary of the present position regarding Synthetic Drugs, and Prof. Rowe's really useful monograph upon Tetrahydronaphthalene and its derivatives, which certainly fills a serious gap in chemical literature.

One omission is that there is no article on the Sesquiterpenes, which might be expected now that Ruszicka's brilliant work on this group has given us such an insight into their structure; justice also is hardly done in six pages to the vast subject of Artificial Silks, and space might be saved here and there by omitting references to unimportant subjects such as Tetradecylmalonic acid. The lack of cross-references and index will, no doubt, be overcome by the promised index to vol. vii, which will enhance the value of the Dictionary very considerably.

Volume vi certainly maintains the high standard of achievement of the earlier volumes and will help to complete a fitting memorial to Sir Edward Thorpe.

F. A. M.

**Recent Advances in Physical and Inorganic Chemistry.** By ALFRED W. STEWART, D.Sc., Professor of Chemistry in the Queen's University of Belfast. Fifth Edition. [Pp. xi + 312. With 29 figures, 5 plates, and 1 chart.] (London: Longmans, Green & Co., Ltd. Price 18s. net.)

A WELL-KNOWN physicist once stated that he liked to think that every research student working in the physical laboratory with which he was associated, carried the Nobel prize for physics in one pocket of his laboratory jacket and the Nobel prize for chemistry in the other. Certainly, the many contributions made by physicists to this useful volume bear eloquent testimony to the correctness of this view.

This book will be specially welcomed by that large body of students who undergo a general course of training in physics and chemistry. All the chapters on the contributions of physicists are extremely well written.

There are excellent accounts of X-ray spectra, the phenomena of radio-activity and the disintegration theory, isotopes, isobares, the analysis of positive rays, and the mass spectrograph. It is suggested, however, that in the chapter which deals with the radiations from radioactive substances it is advisable to dispense with the term "Becquerel rays," because the author frequently discusses the action of a beam of "Becquerel rays," *i.e.* a composite beam of  $\alpha$ ,  $\beta$  and  $\gamma$  radiations, when it is more convenient to discuss the effects of the several components separately. The use of this term is undoubtedly responsible for a very misleading statement on page 57. In the same chapter it is pleasing to note that a good description of the Rutherford and Geiger measurement of  $e$  is given.

A very reasoned and restrained exposition is given of the present state of our knowledge concerning the phenomena of artificial disintegration and the structure of the nucleus. In the discussion of the structure of the outer portions of the atom, the views of Kossel, Lewis, Langmuir, and Bury are adequately presented. To the average physicist, however, the discussion of the Bohr atom will probably appear rather scanty, and it is hoped that some mention of Stoner's suggestions will be made in a future edition.

Other chapters, of more immediate interest to the chemist, deal with the elements of the rare earths, hafnium, active hydrogen, active nitrogen, many new hydrides, the hydrides and the periodic system, some of the effects of intense drying, and Tesla-luminescence spectra. Very full references to original papers are everywhere given.

It is to be regretted, however, that the author has excluded from this edition much recent work which would have provided several additional and useful chapters. Thus we find no mention of recent work on strong electrolytes, and no reference to the work of Debye. Photo-chemical reactions are scarcely mentioned, and we find no reference to Einstein's law of photo-equivalence or to work on collisions of the second kind.

L. F. BATES,

## GEOLOGY

**An Introduction to Physical Geology; with Special Reference to North America.** By W. J. MILLER. [Pp. xvi + 435, with 351 illustrations.] (London: Chapman & Hall, 1925. Price 13s. 6d. net.)

THIS book is complementary to the same author's *Introduction to Historical Geology* (reviewed below), and the two works may be procured bound into a single volume under the title of *An Introduction to Geology*. This may account for the insertion of chapters on minerals and rocks, and on economic geology, which appear to have a very tenuous connection with the main topic of the book. The information in these chapters is scrappy and largely useless. It is difficult to see what purpose is served by the insertion of these insufficient notes, when the same end would be better attained by referring the student to the numerous excellent textbooks on these special subjects. Apart from this the book appears to be well done, and to serve its main object fully. It includes chapters on rock weathering; the instability and structure of the earth's crust; the work of streams; glaciers and their work; geological action of the wind; the sea and its work; volcanoes; subsurface water; mountains, plateaus, and plains; and the origin and history of lakes. The topics are clearly treated, in a style plain to baldness. The lucidity of the descriptions, and the value of the book, are greatly enhanced by a beautiful series of photographic illustrations, the majority of which are new.

G. W. T.

**An Introduction to Historical Geology ; with Special Reference to North America.** By W. J. MILLER. [Pp. xvi + 399, with 238 illustrations.] (London: Chapman & Hall, 1925. Price 13s. 6d. net.)

THIS is a second printing of the second edition of an American textbook on Historical Geology, which term includes stratigraphy in the narrow sense, combined with palæontology and palæogeography. As is appropriate in a textbook intended for American students, the stratigraphical geology of North America is the main topic. Nevertheless, the linking references to foreign stratigraphy are shorter and scappier than they should be in a work of this nature, and are somewhat ill-balanced. Thus, there is no definite mention of the gigantic Caledonian (late Silurian) orogeny of Western Europe. The claim of the preface that more space is allotted to a discussion of the broad fundamental principles of historical geology than is customary in textbooks, is hardly justified by the 34 pages devoted to this subject in 400.

After the first two chapters on general principles there follows one on the origin and pre-history of the earth. The various systems in turn are then dealt with in succeeding chapters; and there are intercalated in the appropriate places summaries of Palæozoic and Mesozoic history. Much use is made of Bailey Willis's excellent palæogeographic maps of North America. A word of praise is due to the numerous beautiful and unhackneyed illustrations. The book provides a good summary of American stratigraphical geology for European readers, and unquestionably makes an excellent text on general stratigraphy for American students. G. W. T.

**Engineering Geology.** By H. RIES and T. L. WATSON. Third edition, revised. [Pp. vii + 708, with 253 figures and 87 plates.] (New York: J. Wiley & Sons; London: Chapman & Hall, 1925. Price 25s. net.)

IN this, the third edition of a well-known text on engineering geology, there have been a few changes from the second edition (published in 1915) in response to requests by teachers; and the book has been brought up-to-date both as to subject-matter and the reading references listed at the end of each chapter. The authors appear to have overlooked the discrepancy between the plan and section in Fig. 116 (p. 106), which dates from the first edition. Moreover, they have omitted to number and describe this figure. In Fig. 64 there is an extraordinary lack of deformation in the strata adjoining a broad zone of fault breccia.

The first two chapters of the book deal effectively with the mineralogy and petrology necessary for the adequate study of the subject. They are followed by chapters treating the structural features and metamorphism of rocks, and with rock weathering and soils. Chapters V. to X. deal more directly with the practical aspects of geology in relation to surface and underground waters, landslides, waves and shore-currents, lakes, and glacial deposits. The remaining chapters demonstrate the importance of geological principles in the winning and selection of building stones, limes, cements, clays, coals, petroleum, road metals, and ore deposits. The book closes with a useful chapter on Historical Geology.

This work maintains its position as a most comprehensive and valuable exposition of geology as affecting engineering operations and materials.

G. W. T.

## BOTANY

**Flower Scent.** By F. A. HAMPTON, M.A., B.M., B.Ch.(Oxon). [Pp. 135, including 3 appendixes and an index.] (London: Dulau & Company, 1925. Price 6s. net.)

HORTICULTURISTS and garden lovers will be grateful to Dr. Hampton for this excellent little monograph, which embodies in its 135 pages a wealth

of information about a subject regarding which the average person knows very little and which is of very great interest to all. The author, judging by the bibliography he gives at the end of each chapter, has ransacked many volumes and journals in which his facts lay buried to all intents and purposes, and has brought them together into this small, compact little book. No garden lover's library should be without it.

The author tells us that "there are very few scented flowers of pure blue, probably because blue is the rarest of flower colours and is favoured by bees," the latter being less dependent on scent than colour. The bee flowers are therefore predominately purple, violet or blue, as the bees being colour blind they are unable to distinguish red from green and confuse both these colours with grey. Butterflies on the other hand "favour the reddish colours, and their preference brings up the percentage of scented red and pink flowers." Whereas birds making "no use of their sense of smell, none of the flowers habitually fertilised by them are fragrant."

As the chemical and physical factors which determine the quality of a smell are not known, the classification of flower scents has to be made according to the impression produced upon the individual, which is profoundly influenced by personal association. The author suggests an entirely new classification based on that of Kerner's.

A curious fact mentioned by the author is that indol, one of the products of putrefaction and found in excreta, is present in the essential oil of many heavy-scented flowers. It is found in many foul-smelling flowers which invariably attract carrion and dung-flies, and about one of these, the North American Skunk Cabbage spiders construct their webs in order to profit by the swarm of flies which it attracts.

The reason for our appreciation of a sweet scent seems to be due to its ability to stir the instinct of courtship. It appears that certain heavy "scents have been evolved to match the scents of the moths and butterflies that visit them, and that the scent of butterflies plays a part in their courtship and serves to stimulate the mating instinct." In speaking of the preference of Orientals for the heavy animal scents the author states that the natives of Somaliland use crocodile musk. Although musk is emitted by the alligator of Central America the reviewer is not aware that the African crocodile likewise produces it, and he certainly never met with any Somalis who showed a preference for any scent beyond myrrh and frankincense, both of which are indigenous in the Somali country. Even some of the sweet-scented bdelliums found in Somaliland are rarely if ever collected or used by them for any purpose.

Amateur gardeners will find a good deal of excellent advice in the chapter on Scent in the Garden.

Methods of the extraction of scent occupies a whole chapter, and the author gives a short, clear account of the various processes in vogue at the present day, while the book ends with an historical chapter which is both interesting and helpful, together with three short appendixes and an index.

R. E. DRAKE-BROCKMAN.

**The Science of Soils and Manures.** By J. ALAN MURRAY, B.Sc. Third Edition, revised and enlarged. [Pp. xiv + 298, with 45 figures.] (London: Constable and Co., Ltd., 1925. Price 12s. 6d. net.)

DURING the last few years very considerable advances have been made in our knowledge of soil chemistry and physics, and the relation between soil biology and plant growth. The more important of these are incorporated in the third edition of this textbook, replacing accounts of certain ideas and methods which are now out of date or which have proved less useful than they promised to be. The various chapters deal with the origin, physical

properties, chemistry and biology of soils, fertility and function of manuring, phosphatic, nitrogenous and potash manures, the valuation of artificial fertilisers, and finally with organic and farm-yard manures. The general standard of the book is that of the advanced University student, but the inclusion in the appendix of certain mathematical data increases its value without rendering the body of the book unsuitable for the practical worker. Special attention is given to the needs of horticulturists, manurial tables adapted for the growth of various classes of garden crops being included. The methods of manufacturing artificial fertilisers are described and illustrated by sketches of the actual plant used, thus impressing on the student mind the origin and significance of the products used for increasing crop growth. The important question of fertiliser valuation is discussed, illustrated by concrete examples, and the relative values of different methods of calculation are compared, the aim being to obtain a figure that most nearly represents the financial position to the farmer.

W. E. BRENCHEY.

**The Classification of Flowering Plants, Vol. II. Dicotyledons.** by Dr. A. B. RENDLE. [Pp. xix + 636, with 279 figures.] (Cambridge: at the University Press, 1925. Price 30s. net.)

THE author apologises for the delay of twenty years since the appearance of the first volume of this work, which dealt with the Monocotyledons, but his amend is the production of the volume before us, in which the plan of its predecessor has been closely followed. During this interval, however, the publishers would appear to have forgotten some details of the original format, with the result that the two volumes make a rather ill-assorted, not to say discordant, pair upon the shelf.

As already indicated, the text follows the same method of treatment as the earlier volume on the lines of Engler's system of classification, but departing therefrom in some rather important respects. Thus the Archichlamydeæ are separated into two Grades based on floral structure, viz. the Monochlamydeæ and Dialypetalæ. The former includes the first seventeen orders of Engler (Casuarinales to Centrospermæ), omitting however the two monotypic cohorts of the Leitneriales (2 spp.) and Batidales (1 sp.), whilst the Myricaceæ are placed in the Juglandales and the Balanophoraceæ in the Santalales. The sequence too differs from that of Engler's system entirely.

In the second Grade, which includes the Englerian cohorts eighteen to thirty with the exception of the monotypic Pandales (1 sp.), not only is the sequence again considerably altered, but ordinal rank is given to the Cucurbitales (Cucurbitaceæ, Begoniaceæ, and Datisceceæ), Guttiferales (Dilleniaceæ, Ochnaceæ, Marcgraviaceæ, Theaceæ, Guttiferæ, and Dipterocarpaceæ), Rutales, Celastrales, and Tricocceæ (Euphorbiaceæ, Buxaceæ, and Callitrichaceæ). Polygalaceæ is placed separately as an anomalous family. A curious omission is that of the Calycoraceæ, which not only contains some forty species, but is of considerable interest from its resemblances to and its differences from the Compositæ.

As a work suited primarily to the needs of students the marked differences from the familiar systems are perhaps to be regretted whatever the merits or demerits of the changes themselves, the more so that the reasons for these departures are not elucidated.

The student will find in this volume a useful reference book containing a large store of information not readily available in English elsewhere. Apart from the more formal taxonomic details there is much of general interest, whether it be respecting the geographical distribution of the species of *Acer*, the embryo-sac of the Plumbaginaceæ or the economic importance of the Guttiferæ.



Bibliographical references are appended to the accounts of a few of the families.

Of the many excellent illustrations, which are not over-reduced as in the earlier volume, more than one-third are original.

E. J. S.

## ZOOLOGY

**Animals of Land and Sea.** By AUSTIN CLARK. [Pp. xxxiv + 276, with 740 figures.] (London: Messrs. Chapman & Hall, 1926. Price 15s. net.)

THIS book belongs to the "Library of Modern Sciences" which treats of the influence of the various branches of science on the development of civilisation. It is a natural history on new lines, being a survey of the whole animal kingdom, emphasising the interrelationship of all living things, including man. The author, who is the Curator of the Smithsonian Institution, with world-wide experience and unbounded opportunities of studying his subject, is eminently successful in arranging his matter in a readable form, and so much information is contained in this volume that it is a valuable book of reference for any biologist.

Food is the all-important subject, and we have chapters on the animal food of man, including very many queer things which are not usually eaten, such as various insects and reptiles, man as food for animals, many parasitic beasts, and the innumerable ways in which animals are adapted for catching their prey and are protected from their enemies.

To bring home certain facts special illustrative statements are frequently used, of which the following is an example: "The difference in relative area of the wings between a mosquito and a stork may be appreciated when it is realised that if a stork had wings proportionately as large as those of a mosquito they would have an area of almost twenty-eight and a half square yards and an expanse of more than twenty-five feet." The chapter on Animal Flight is excellent; also the chapter entitled "Living Lamps," in which amongst other things we are introduced to luminous birds and to an Indian bird which decorates the mud around its nest with fire-flies.

The whole book is altogether entertaining, the only drawback being the arrangement of the figures, which are scattered about without any reference to the text. Thus we have a large portion on insects illustrated by pictures of fishes and a chapter on "Intermediate Foods of the Sea" by pictures of insects, and so on throughout the entire work. Moreover, for the descriptions of the figures we are obliged to consult a list at the beginning of the book instead of having the explanations under the figures themselves.

*Animals of Land and Sea* is, all the same, an extremely useful and interesting work and one that will please all lovers of nature by its variety and charm.

MARIE V. LEBOUR.

**A Study of the Oceans.** By JAMES JOHNSTONE, D.Sc. [Pp. viii + 215, with 41 figures.] (London: Messrs. Edward Arnold & Co., 1926. Price 10s. 6d. net.)

DR. JOHNSTONE follows up his *Introduction to Oceanography*, published in 1923, by the present historical study of the oceans, which is geography in its widest sense. Certainly a thorough understanding of the oceans should include the knowledge of how and when they were discovered as well as their probable origins. The author possesses the spirit of the true oceano-

grapher and regards his subject from all points—historical, physical, and biological, each being allotted a proper amount of attention.

The present work begins with the geological history of the oceans, introducing the reader briefly in simple and non-technical terms to the more important hypotheses of the origin of the earth and its oceans. Then follow chapters on the Classical Geography of the Ocean, the Crossing of the Ocean, the Circumpolar Regions, and finally, special chapters on the Atlantic, Pacific, and Indian Ocean.

The gradual growth of the map forms an important item, and we have the most interesting account of the maps of the ancients, beginning with Anaximander of Miletus (610–547 B.C.), whilst a map of the Atlantic is figured which is attributed to Leonardo da Vinci. On reading these pages one realises how much and not how little was known of the world in olden times.

There is a curious error, which must be a slip of the pen, on page 123, where it is stated that Captain Scott's last voyage was in the *Discovery*. That the *Terra Nova* carried him on his last expedition should be known to all, and in a book such as this, which is presumably intended for students, it is unfortunate that the names of his first and last ships in voyages of extreme national importance should be confused.

*A Study of the Oceans* is an exceedingly readable book and will be enjoyed by all those who love the sea.

MARIE V. LEBOUR.

**The Theory of the Gene.** By T. H. MORGAN, Professor of Zoology in Columbia University. [Pp. xvi + 343, with 156 illustrations.] (New Haven, Yale University Press, 1926. Price 18s. net.)

IN spite of its title, which suggests a much more detailed and interesting inquiry, this book is really a general textbook of genetics, as understood in view of the advanced work with *Drosophila*. In a full theory of the gene we might expect an assembly of the evidence relating to what the gene really is; to how the genes co-operate with each other, and with the cytoplasm to control and determine the course of development; to the cause of dominance and how it is brought about, and why are mutations so frequently recessive, or if dominant so frequently lethal; to what a mutation really is, as indicated by the different mutations which occur at the same locus, by mutation rates, by the stages of development at which they occur, and by the evidence for environmental control of their frequency. Admittedly no full theory of the gene could be written in the present state of knowledge; nevertheless, if the researches of the Columbia school are as fruitful in the future as they have been in the past, we may hope that Prof. Morgan will yet set himself to the larger task, for which the present book only clears the ground.

It is not always sufficiently understood that the great advances of modern genetics rest primarily upon the statistical evidence afforded by the relative frequency with which particular types occur in particular matings. The cytological evidence affords striking confirmation as far as it goes, but, without the precise tests of actual breeding, all would be speculative. In chapter ii, on Particulate Theories of Heredity, Prof. Morgan touches on Spencer's "physiological units," Darwin's gemmules, and Weismann's ids. He sums up:

"These and other earlier speculations have to-day mainly an historical interest. They do not represent the main path along which the modern theory of the gene has developed, which rests its claims to recognition on the method by which it is derived and on its ability to predict exact numerical results of a specific kind."

It seems a pity that space should be occupied, as in chapter vi in beating the dead horse of the Presence and Absence the ~~Cytoplasm~~. An opportunity seems to have been lost in devoting chapter xviii, on the stability of the gene, to a criticism of quite inconclusive Lamarckian experiments.

Chapters which strike the reviewer as being of particular value are ii, quoted above, and vii, on genes in related species, while chapters viii to xii on tetraploids and other phenomena involving abnormal chromosome number supply a very useful popular exposition.

R. A. FISHER.

**The Cell in Development and Heredity.** By E. B. Wilson. [Pp. xxxvii + 1232, with 529 illustrations.] (New York: The Macmillan Company, 1925. Price 36s. net.)

THE appearance of a third edition of this important work is an event of great interest to a wide circle of workers on topics relating to cells, heredity, and many other biological problems. For Wilson's *Cell* has held a unique position among cytological works in the English language. A number of works have appeared in recent years dealing with special aspects of cytology, but no single book covers such a vast and comprehensive field as does this of Professor Wilson.

The new edition is a monumental achievement, running into 1,232 pages divided into fourteen chapters. Some conception of the amount of labour involved in the production of this edition can be gained when it is recalled that the last edition appeared in 1900, the year of the rediscovery of Mendel's work. Since that time the output of cytological work has been enormous, not only in regard to the chromosomes but also in the comparatively neglected sphere of the cytoplasm. What distinguishes Prof. Wilson's book from all others is the inclusion of lower organisms and of the chief results of cytological investigations in plants, providing the widest possible basis for the discussion of the general biological problems treated at the end of the book—sex, growth, development, and heredity.

J. H. W.

**The Early Embryology of the Chick.** By BRADLEY M. PATTEN. [2nd edition, pp. viii+177, with 63 figures.] (Philadelphia: P. Blakiston's Son & Co.)

THE second edition of this book has been somewhat enlarged by the provision of a coloured frontispiece and a few more figures in the text. The simple description and numerous excellent illustrations make the book a useful one for serious beginners in embryology. One feature, however, might be criticised as detracting somewhat from its suitability for beginners—namely, the inclusion of Patterson's account of the origin of the endoderm in the pigeon and the inference from this to the chick, with which this book is concerned. For the process of turning in of the hinder margin of the blastoderm described by Patterson has not been confirmed for other birds, and, outside America, has not met with such wide acceptance as the author suggests. It has the further disadvantage that, far from rendering comparison with lower forms easier, it commits its adherents to the concrescence theory to account for the origin of the primitive streak. It would have been better, in a book for beginners, to have described the facts as far as they are known in the chick, leaving theoretical and speculative matters to a later stage.

J. H. W.

**The Dinoflagellates of Northern Seas.** By MARIE V. LEBOUR, D.Sc., F.Z.S. [Pp. vii + 250, with 35 plates and 53 figures in the text.] (Published by the Marine Biological Association of the United Kingdom, Plymouth, 1925. Price, 12s. 6d. net.)

DINOFLAGELLATES are aquatic protozoa, living freely in fresh, brackish, or marine water, or in sand and mud. They are enormously abundant, and nearly ubiquitous in their distribution, though they prefer warmer waters and the warm seasons of cold seas. Originally they were probably holophytic in their mode of nutrition, though they have become specialised to the extent that many species are holozoic, and some appear to be saprophytes. It is because of their ability to synthesise carbohydrate from carbon dioxide and water that the Dinoflagellates are so very important in all problems of marine metabolism. As producers of organic substance from inorganic materials they come next in importance, in the sea, to the Diatoms, but regarded as a source of food-materials for the multitude of marine animals they probably come first, because of the higher nutritive value of their bodily substance. Every general investigation into the conditions of life in the sea must, at all stages, consider the rôle of these important organisms.

Before the publication of Dr. Lebour's book there was no general up-to-date survey of the Dinoflagellates. Zoologists had to rely mostly on the article in the *Nordisches Plankton*, and, for many years, this has been defective because of the rapid growth of knowledge of the group. It is with sincere gratitude to the author, and to the Royal Society and the Development Commissioners (who have assisted in the cost of publication), that all marine biologists will receive this book, and no marine station, zoological laboratory, or private worker can possibly work on the group, or even on problems of general marine biology, without its assistance.

The work is very well done. Every species has been described and figured, and a large number of the illustrations are original, occupying the thirty-five plates. All these figures are most clearly drawn, and will be easily consulted. Necessarily the work is highly technical, but there is no reason why beginners in marine biology should not "get up" the group of Dinoflagellates from Dr. Lebour's monograph. We particularly welcome works of this kind, done by specialists with first-hand knowledge of their subject. Such another volume dealing with the Marine Diatoms in the same way would be invaluable. May we hope that the Marine Biological Association will undertake it?

J. J.

**A Monograph on the Tetracophyllidea, with Notes on Related Cestodes.** By DR. T. SOUTHWELL. [Pp. 368, with 244 figures in the text.] Liverpool: at the University Press, 1925. Price 20s. net.)

THE phase in the history of zoology (that is, systematic zoology) has come, when, in sheer desperation, the various groups must be monographed. It would be quite the biggest advance that could be made were each University Department, Museum, or Biological Station to set itself the task of writing up the whole state of our knowledge with regard to the systematics of one or more groups. So great are the numbers of papers and so dreadful is the difficulty of obtaining journals, books, inaugural dissertations, etc., that confronts workers, that systematic work is often badly done.

Such monographs as zoologists have long wanted are represented by Dr. Southwell's book, and that on the Dinoflagellates by Dr. Marie Lebour, which has been reviewed above. But in the case of the group of Cestode worms called Tetracophyllidea the difficulties have been unusually great, how

great no one who has not attempted to identify these animals can realise. The classification was in a state of utter chaos ; many species in the literature were obviously mis-identified ; the synonymy was loaded with names that denoted nothing real ; the animals themselves are variable to an extent that leads to confusion ; the difficulty of obtaining type-specimens, and even good museum specimens, is enormous, for the Cestodes are most difficult to preserve properly,—when all this is considered we cannot be too thankful to Dr. Southwell.

Then the group is rather an important one. It figured largely, for instance, in the investigations into the origin of the Orient Pearls, and there is always the possibility of its importance in parasitic disease. Tetraphyllid Cestodes are, so far as we know, almost exclusively confined to fish hosts, and it is not known that any human tapeworm parasite has a part of its life-history in a marine fish—still, we are not yet sure. The reviewer heard, not so long ago, of the identification, by a medical officer of the Ministry of Health, of a larval *Tetrarhynchus* inhabiting the flesh of the Halibut as a *Bothriocephalus*, and the consequence of such a mistake might have been commercially important. So long as the possibility of confusion like this exists there is room for clearly written and exhaustive monographs of parasitic worms. Such a monograph is the work now under notice. It is well done and well illustrated and the synonymies, lists of literature, and discussions are very excellent. We congratulate the Liverpool School of Tropical Medicine on the publication of the book—it is not the least meritorious of their ventures—and we thank those friends of Dr. Southwell who helped to finance the preparation of the monograph.

J. J.

**Animal Life in the Sea.** By R. J. DANIEL, M.Sc. [Pp. 119, with 56 illustrations.] (Liverpool: at the University Press, 1925. Price 5s. 6d. net.)

THE lighter side of natural history is not without its exponents, and yet the result of ventures of this kind has not always been satisfactory. The "Marvel" books usually make appeal to readers who are not *much* interested in the solid parts of biology, and really all natural history is "marvellous." There is increasing opportunity for writers to deal sensibly with biological results, and yet to arrest the attention of readers without what may be called "bilge."

Thus we have heard, for centuries, about mermaids, krakens, sea-serpents, and the like, and people who read about these creatures are mostly of two kinds: those who believe all they read, and those who believe nothing that they read. The truth is, of course, that seamen and fishermen never invented the stories of the sea-serpent; for instance. They really saw things that were very extraordinary (and when a fisherman sees things that *he* regards as very unusual, it is always safe for the biologist to conclude that there is material for investigation in the reports).

Mr. Daniel writes clearly and simply and with both knowledge and acuteness of judgment about the legendary animals of literature, attempting to trace these to the real things. He also writes about marine life in its more quaint and interesting and bizarre aspects, and he does this in a most agreeable manner. The book is one which is really the kind of thing that might be taken as a textbook, or reading-book, by form-masters in schools, and illustrated and amplified as time and opportunity permit. But for the general reader also—the person who is interested in biology and in the sea—the book will be welcome; to all such readers we can cordially recommend it

J. J.

**A Naturalist in East Africa.** By G. D. HALE CARPENTER, D.M. (Oxon.), M.B.E. [Pp. 187, with 31 plates and 3 maps.] (Oxford: At the Clarendon Press, 1925. Price 15s. net.)

DR. CARPENTER'S latest book is, as one would expect from such a keen and experienced field naturalist, packed with observations on the bionomics of the different insects he came across in his wanderings while on active service in East Africa during the Great War. While he occasionally touches on the flora, and the mammal and avifauna, he deals mainly with entomology, which is obviously his first love.

He is most interesting when describing mimicry in certain species of lepidoptera and when discussing the aposematic and procrystic coloration of various species.

No one who has travelled in Africa for many months could have failed to notice one or more of the numerous species of fossorial wasps conveying to their burrows or nests caterpillars rendered inert by being stung. The author has much to tell us of these interesting insects.

The reviewer himself has often watched a common coastal species which builds on the rafters of the dwellings a many-chambered nest of mud. Each compartment as soon as completed is left with a small aperture through which a living but paralysed caterpillar is eventually pushed and after the ova has been deposited the apertures are sealed up.

The author tells us that the ubiquitous Painted Lady is not the only butterfly to be found both in Europe and Africa. The delight at seeing some of the common insects of our own country-side, not to mention certain birds such as the homely swallow, which also breeds in equatorial Africa, is fortunately not an uncommon experience to the home-sick exile in Africa. Among the interesting notes on mammals the author tells us that otters are common around certain lakes, in which there are no fish, so one is left to presume that their chief food is frogs. The author is mistaken when he says that the nearest relative of the rabbit-like hyrax, or dassie, is the elephant. It is the rhinoceros. It was owing to the anatomical structure of the hyrax, chiefly with respect to its molar teeth and the hind feet, which have only three toes each, being similar to that of the rhinoceros that Cuvier placed these quaint little creatures in a genus by themselves next to the rhinoceroses. At the present day, however, systematists are more in favour of placing them in a family entirely distinct, having no immediate relatives among mammals, living or extinct.

The small solitary buck, seen by the author at Ndala, was probably a Duiker and not a Steinbok, as he suggests.

Again, the small "jackal" met with at Itigi and described as "a dark, smoke-coloured animal," sounds like that delightful little creature, the African long-eared fox (*Otocyon megalotis*). The final chapter on the relative edibility of insects (tested on monkeys) has already been published in greater detail in the *Transactions of the Entomological Society of London*, but has been wisely incorporated in its present form in this book, where it will have a wider public.

The illustrations, which are all good, are from the author's own photographs, while at the end of the book are several photographic plates depicting mimicry and seasonal changes in certain butterflies and lycid beetles and also a plate showing a moth and a snake as faunal links between Africa and South America. There is a useful little map at the end which shows the author's itinerary, and completes a most interesting book which contains in its 182 pages much valuable information to all those interested in natural history.

R. E. DRAKE-BROCKMAN.

**The Nematode Parasites of Vertebrates.** By WARRINGTON YORKE, M.D., Professor of Parasitology, University of Liverpool, and P. A. MAPLESTONE, M.D., D.S.O. With a Foreword by C. W. STILES, United States Public Health Service. [Pp. xi + 536, with 307 illustrations.] (London: J. & A. Churchill, 1926. Price 36s. net.)

IN the above-mentioned book Professor Yorke and Dr. Maplestone have rendered an indispensable service to all who are interested in the study of Nematodes, or in the many diseases caused by the presence of Roundworms in the vertebrate body. These creatures play a very considerable part in human, animal, and plant diseases. They are responsible for the devastating hookworm disease, which has spread throughout the tropics and many other human and animal troubles in other and more temperate parts of the world. They are a perpetual trouble to some genera of Bees as well as to other insects. Nematodes are a very difficult group of animals to study, partly because their thick cuticle prevents the penetration of staining reagents and to study their anatomy the best method is to make them as transparent as possible. They show comparatively few external anatomical features, and these are not by any means easy to observe.

*The Nematode Parasites of Vertebrates* is by no means an easy book to review because, in the main, it consists of technical accounts of some hundreds of genera of Roundworms, arranged under their separate families. The names of the species of each genus follow the account of the genus; but the names only, otherwise the work would have been of colossal proportions, but there is a complete index of the very numerous species extending over forty pages of small print. A separate index comprises the generic names and synonyms, and although the authors do not claim that their list of previous references to Nematode "literature" is complete, it includes just under seven hundred headings.

The work is prefixed by a thoughtful preface by the well-known Professor of Zoology in the United States Public Health Service, C. W. Stiles, who ends up with a characteristic letter to a future unborn helminthologist:

"MY DEAR UNBORN COLLEAGUE:

"When you consult this book you will find in it some views with which you will not agree. This will be the inevitable result of an advance in knowledge after the publication of this volume. And, as you differ with taxonomic views expressed here, so will your successors modify your views as their knowledge increases. These changes of view from generation to generation are inevitable unless science stagnates.

"This work by York and Maplestone was written in order to save your time and to make you more efficient in your professional life. It has cost them many hours and days of patient labour. It has not been prepared in order to gain a reputation. It is a labour of love on their part in order to help you. See that you do for the generation that follows you what Yorke and Maplestone have done for you."

A. E. S.

**Guide to the Collections of British Lepidoptera in the National Museum of Wales.** [Pp. 31. With 2 col. plates.] (Cardiff: The Museum and the Press Board of the University of Wales, 1925. Price 6d.)

THIS little handbook is a commendable attempt to save the time of the student and increase his interest in the collections. The Museum contains two large and valuable collections of British Lepidoptera, one made by the late H. W. Vivian, comprising over 20,000 specimens, the other that of Mr. A. F. Griffith, which is even larger. These collections have, of course, been duly arranged and classified, but since comprehensive catalogues cannot be issued broadcast,

the handbook has been designed to call attention to the unusual varieties or specimens of historic interest. The facts concerning them are concisely set forth and an exact reference to their positions in the cabinets is given, so that they can be found instantly.

The value of the guide is enhanced by two excellent coloured plates of the more remarkable specimens, and a short monograph on some peculiarities of the Welsh Lepidoptera.

H. M.

**The Respiratory Function of the Blood. Part I. Lessons from High Altitudes.** By PROF. J. BARCROFT. [Pp. viii + 206, with illustrations.] (Cambridge: at the University Press, 1925. Price 12s. 6d. net.)

To the many who know and continue to enjoy Prof. Barcroft's *Respiratory Function of the Blood*, the obligation of the reviewer is easily performed. It is sufficient to pass on the news that this happy volume is in process to revision. The author, deferring to the rapid advance of knowledge, has broken up the subject into separate parts corresponding to the sections of the original volume. The first part—*Lessons from High Altitudes*—is now in our hands.

If there be students of biology who are not yet friends with Prof. Barcroft's pen, we are content to persuade these to but glance at the preface to the first edition. They will scarcely leave the further pages uncut. To read Barcroft is to adventure with a rare companion. With him we live amongst the Cholos of the Andean Pampa whilst we, almost unconsciously, learn the physiological peculiarities by which these little people have become adapted to high altitudes. Mountain sickness loses its terrors whilst it gains our interest. Cyanosis becomes more than a study in purple. And so, passing easily between the laboratory and the Cerro de Pasco, we follow the effects of reduced oxygen tension upon the blood, muscular activity, the heart, the circulation, and the mind. Readers will appreciate particularly the author's discussion of Haldane's theory of the secretion of oxygen.

Four appendices complete the volume. The first, entitled, "The Physiological Difficulties in the Ascent of Mount Everest," is written by the medical officer to the last expedition, Major R. W. G. Hingston, I.M.S. This is the great adventure. The remaining appendices are of academic interest, and present valuable data bearing on the physico-chemical interpretation of the respiratory function. That by Dr. C. Murray is a notable study.

It is not given to many to capture in words the full romance of science. When we find an author with this precious gift we do not pause to measure out our gratitude to him.

R. K. C.

## MEDICINE

**The Conquest of Disease.** By DAVID MASTERS. [Pp. xvi + 314, with 68 illustrations.] (London: John Lane, 1925. Price 8s. 6d. net.)

A WORTHY history of medical discovery might already be commenced, although we hope the tale is by no means yet complete. It would be more important than the histories of Tacitus and Gibbon and would require an historian of ability equal to theirs. His knowledge of the subject would have to be great, his acumen penetrating, and his judgment supreme. This book appears to have been indited by a layman; and, if so, we can only judge that his hand has been guided on matters of scientific history by unknown professional friends. We commend his part in the work, but not that of his advisers. Those portions of the story with which I have been acquainted since the times when the advances were actually made are

full of serious inaccuracies evidently due to spurious prompting. Names of men who have done great work (as in connection with plague and cholera) are omitted, while various small men are often puffed in a manner which begets laughter in those who know the facts. It is impossible to review the book without hurting the feelings of the latter as much as the book itself may hurt the feelings of the former. The work may stimulate the interest of the numerous good people who subscribe to medical charities, but it is of no value as a serious contribution towards the history of a great and important subject.

R. R.

**The Fats.** By J. B. LEATHES, M.A., M.B., F.R.S., and H. S. RAPER, C.B.E., M.B., Ch.B., D.Sc. [Pp. vii + 242.] (London: Longmans, Green & Co., 1925. Price 12s. 6d. net.)

THE first edition of *The Fats* by Prof. Leathes was published fifteen years ago, and the welcome it received at its first appearance is shown by the fact that, like his earlier book upon Metabolism, it has long been out of print. This second edition is, therefore, very welcome and the more so, that it is the joint work of Professors Leathes and Raper, the two physiologists best qualified to prepare such a monograph. The book is full of valuable information, and written with clearness and distinction. It is divided into two parts, which will appeal differently to readers according as to whether their interests are mainly chemical or biological. But, as the authors state in their preface, those who approach this subject from the chemical or physiological side are likely to have their interest in, and their power of tackling the many important problems associated with, the fats greatly increased by knowledge from the other standpoint.

The first part deals with the chemistry of the fats, methods of extraction and estimation of fat in animal tissues, and of determination of their characters and properties. The second part is concerned with the physiology of the fats, and gives an extremely interesting account of their rôle in the living organism. It deals with the digestion and absorption of fat, its transference from the alimentary canal to the blood, from the blood into the organs, and its utilisation in the cells, with special reference to fat, as a source of energy, either in the production of heat or in the performance of external work.

The last chapter of the book passes under review the part played by fats in the life of the cell. It takes, first, its most commonly recognised use as a reserve fund of energy, for which purpose it is extremely suitable, since, being stored, not in solution, but as fat itself large stores of energy can be put by in a small volume with great economy of weight. All other substances available as sources of energy are present in solution, and therefore require the carriage in the body of many times their weight of water. Fats can be used in mechanical ways too, as for example the wax in which the bee stores concentrated solutions of highly hygroscopic sugars, or the waxes and fats secreted on the superficial parts of plants to protect them from excessive loss of water; they appear also in certain bacteria, as in the peculiar fatty substances in which the tubercle bacillus is encased, and which are regarded as conferring upon the organism much of its vitality and special powers of resistance.

An interesting discussion follows as to the undoubted, but, as yet, not clearly understood part played by the fats in the actual structure of the cell, and here many suggestive questions are discussed showing the opinion of the authors that in the understanding of the rôle of fats in the structure and functions of the cell lies one essential step to the solution of these problems. To put it in their own words with which they end this valuable, interesting,

and scholarly book, "behind the capricious semipermeability of the cells lie the mysteries of physiological absorption and secretion, of excitation in muscles, and in nerves, and of the division of cells and their nuclei."

W. C. CULLIS.

**An Introduction to Sexual Physiology. For Biological, Medical, and Agricultural Students.** By F. H. A. MARSHALL, F.R.S. [Pp. xii + 167, with 72 illustrations.] (London: Longmans, Green & Co., 1925. Price 7s. 6d. net.)

AN admirable book written by the man best able to deal with the subject. Prof. Marshall's book *The Physiology of Reproduction*, now in its second edition, has long been recognised as one of the most satisfactory and comprehensive works on the subject, and this smaller book gives in a shorter form all the more important facts dealt with in the larger one, and in a manner that makes it suitable for those with very little biological knowledge. It is extremely satisfactory to realise that here is a book that supplies a need, long felt, for an authoritative and at the same time clear and accurate statement as to the more important sexual and reproductive processes in the higher animals and man. Such a book is often asked for by those who desire, and rightly desire, accurate and scientific information as to this most important of functions. It gives an adequate account of the structure of the reproductive organs in the higher animals, of the mammalian sexual cycle, of pregnancy, parturition, the puerperium and lactation. It contains a discussion of the functions of the organs of reproduction considered as internal secreting glands, and of the questions of heredity and sex. In the last chapter the author deals with fertility from many aspects, ending with a reference to the birth-rate in man, and the problem of the human population, of which he says that its "biological interest is only surpassed by its supreme social importance."

The book with its many excellent illustrations and its extremely interesting text gives a much-needed account of these most important facts in the best possible manner. It will undoubtedly be of the greatest interest to any reader fortunate to come across it, and it is to be hoped that it will be widely read.

W. C. CULLIS.

**Food and the Family.** By V. H. MOTTRAM, M.A. [Pp. xiii + 240.] (London: Nesbit & Co., 1925. Price 5s. net.)

HERE is a most entertaining and at the same time scientifically correct statement of some of the more important facts about foods, their nature, their function, and preparation. From the introduction, in which the author descants upon the interest taken in food from the dons at the High Tables of the Ancient Universities to the anguished traveller deprived of his favourite food, whether it be the Briton robbed of his breakfast bacon or the American of his iced drink, to the final chapters, the whole is spiced with humour, and flavoured with philosophic digressions which render very palatable the good wholesome fare provided. The author discourses, in a manner which makes them easily understood, of problems of nutrition and growth, of calories, of amino acids and the biological value of proteins and of deficiency diseases and vitamins. There are dissertations on food fads, on the dangers of modern food-stuffs, and a consideration of special foods. The author in this section, and indeed throughout the book, seems a little hard upon that excellent food material, the egg! Granted that it is an expensive form of food, and that for the money spent upon an egg more food and more calories could be obtained in the form of herrings, bacon, beef-steak, or of milk, yet there are occasions on which some of these forms of food are not very suitable. In addition the

egg provides a very high quality protein, as the author himself points out—and the quality that perhaps gives it its greatest value in the ordinary dietaries—it is a good source of vitamins A and B. He rightly stresses the value of milk as a food.

At the end he gives some very practical suggestions as to the way to determine suitable dietaries for use under varying conditions, and to calculate the values of the various food-stuffs as regards their calorie value and their content of the three cardinal foods, proteins, fats, and carbohydrates. Finally there are two appendices, the first giving the calorie value, the protein content, and the cost of most of the ordinary foods, and the second giving in tabloid form, as it were, eight dogmatic statements to help housewives, in a hurry, in the selection of an adequate family dietary.

The well-fed individual can stand up so much better than the poorly fed one to adverse circumstances, and the health of a nation depends so largely upon its feeding, that it is to be regretted that more instruction is not given in what constitutes good food. Lack of this knowledge is probably, at least as often as lack of means, the cause of malnutrition. Here in this book is given in a very entertaining manner this most important information, and its modest price makes it possible for many to possess for themselves a book well worth possessing.

W. C. CULLIS.

**Food Values, What They Are and How to Calculate Them.** By MARGARET McKILLOP, M.A., M.B.E. Third Edition. [Pp. xii + 152.] (London: George Routledge & Sons. Price 3s. 6d. net.)

THE sub-title of this book explains very concisely its aims and objects, which it may be said at once are very satisfactorily achieved. Mrs. McKillop served during the war in the Ministry of Food, that is, in a department and at a time when it was a vital national necessity to consider foods and food values much more closely than had been the case hitherto. On the decisions of experts as to the needs of individuals, foods were provided, and it is doubtful whether the nation as a whole realises the debt it owes to the scientists and administrators, who were ultimately responsible for the provision of its foods and who accomplished their difficult task so admirably. As a result of her experience at that time, and of the newer knowledge, that came with the necessity for the close study of foods, the author has largely rewritten the book for this its third edition.

Here are set out very clearly the requirements for the average individual man, woman, and child, and an abundance of tables showing the composition of the more common food-stuffs, cooked and uncooked. The author gives most useful examples of adequate meals for individuals, and of weekly food budgets for an average family, showing exactly how these can be calculated from a knowledge of the individual requirements, and from the given tables of composition. These practical examples are of great value, and will help any who study them to the knowledge, most important for the provider of a household, as to whether an adequate diet is being given, and the greatest value obtained for the money spent. It is a book full of common sense, useful information, and helpful advice.

W. C. CULLIS.

## ENGINEERING

**Principles of Machine Design.** By C. A. NORMAN. [Pp. vii + 710, with 585 illustrations and 106 tables.] (New York: The Macmillan Company, 1925. Price 28s. net.)

PROBABLY no one branch of engineering science overlaps so many others as does the subject of machine design, based as it is fundamentally on the

strength and elasticity of materials and guided by the principles underlying the theory of mechanics. The subject, further modified to suit workshop practice, the cost of production, and experience of wear and tear, has then to be applied to all forms of machinery, with the result that it has a large scope. Such a wide variety of examples is available for illustration that it is very difficult for a writer of a text of limited proportions to choose suitable examples without labouring one part at the expense of another. The difficulty of choice is added to, as he is naturally biased by his own practical experience, which tends to make him pick most of his examples from marine engineering, from locomotive practice, or from motor-car manufacture.

There are probably three classes of men for whom such a work might be written, the junior student, the senior student, who has a certain amount of practical experience, and the fully qualified designer.

The inclusion of many diagrams giving general views of details of actual machine parts may be useful to a teacher in explaining what may be made and how it is used, but for a junior student these diagrams, being without dimensions, can convey little idea of proportions. Such diagrams, however, should assist an experienced designer in selecting a detail of design best suited to his particular purpose. For the senior student there are many fully worked examples to show the application of the formulæ and principles discussed.

The fact that so many books have been written on this subject makes one expect perhaps too much in a new text, so that one is likely to be over-critical.

It is at once apparent from the many references quoted that great care has been taken to keep the work in line with up-to-date practice.

As befits a text intended for college students, over-specialisation has been avoided. The only chapter which might be regarded as over-specialised is that on high-speed discs, which is rather limited, to deal with such a large subject.

Within the limits of the text the author has shown great care in the choice of the subjects of his chapters, also in his worked examples.

For the senior student the work can be thoroughly recommended.

B. L.-E.

**The Principles of Sound-signalling.** By MORRIS D. HART, M.Sc., D.I.C., and W. WHATELEY SMITH, M.A., M.Sc. [Pp. iv + 139, with 12 figures.] (London: Constable & Co, 1925. Price 12s. 6d. net.)

THE problem of signalling by sound under ordinary atmospheric conditions is one of extreme complication. That part of it which concerns the reception and detection of sounds received great impetus during the recent war in connection with the location of hostile artillery, and those who were engaged in "sound ranging" will remember some of the baffling irregularities of sound-propagation and the many *ad hoc* modifications of sound-receivers made with the object of attaining greater reception efficiency. By such this book will be read with interest. The writers have made a notable attempt to invest the problems of sound-signalling with rigid definition, so that they can be put to quantitative test.

The various "efficiencies" and "figures of merit" of a sound-signalling system appear perhaps a little arbitrary, and their development is too lengthy in a work of this kind; but some such conceptions are certainly necessary.

The idea of a figure of merit of a detecting device is advantageous, but doubt might be excused as to whether the difficulty of definition (p. 15, l. 3 *et seq.*) be not submerged in the quantity  $P_R$ —acoustical power.

In those sections of the book dealing with the transmission of sound the

authors have described experiments on acoustical degradation. A little anxiety is experienced as to the complete suitability of the hot-wire microphone in work of this kind—an anxiety evidently felt by the writers (p. 53, and, in a more general form, Appendix III). Quite a lot of work remains to be done on this microphone, and the only papers since the original one of Tucker and Paris have been those of Hippel (*Ann. der Phys.*, B. 75, p. 521, 1924) and a small paper by the writer of this review (*Phil. Mag.*, May 1923). It is very pleasant to read of the experiments of Tucker and Paris on the sounds emitted by the siren of the Tongue Light: humidity is shown to be a very important factor in sound transmission—a fact which throws light on the variation of audibility in gun sound-ranging. Concluding chapters on types of sound source contain useful technical information. It seems a pity that the timely warning of the "Strombos" was at the expense of a low over-all efficiency; anyway, its effectiveness was indubitable.

The book is perhaps a little too long for the matter contained. The whole of chapter iii is a reprint from a Royal Society paper by one of the authors and a shortening of the initial chapters would be advantageous. It is, however, a welcome volume, and should be in the hands of all post-graduate students seeking a research problem in sound, as well as of those interested in the technical and academic aspects of acoustics.

R. C. RICHARDS.

**A History of Engineering.** By A. P. M. FLEMING and H. J. BROCKLEHURST. [Pp. viii + 312.] (London: A. & C. Black. Price 12s. 6d. net.)

A WORK on this subject is, in our opinion, long overdue. A properly proportioned record of the achievements in this most vital branch of applied science is an essential to the sum-total of our historical records. As the authors pertinently remark in their preface, in every age engineering in some form has been one of the fundamental means by which civilisation has advanced. Nevertheless, we must stress the fact that engineering achievement is fundamentally a record of applied science, and from this point of view the work under notice is disappointing. It is written rather from the angle of the student of industry as distinct from the student of applied science. From this angle the authors have done their work well, but the fact remains that a proper account of the development of engineering science (without which a history of engineering can, in our view, be little more than the husk) has yet to be written.

To take a few elementary examples, the enunciation by Hooke, in 1678, of his famous law that strain is proportional to stress within the elastic limit was the starting point of a new scientific phase in engineering; Euler published his formula for the strength of columns and struts in 1744, and opened up a new era in the design of structures; the writings of Besson and Ramelli in the sixteenth century gave a much-needed impetus to the design of mechanisms; the labours of Joule, Kelvin, Sadi Carnot, and a host of others vitally affected the whole outlook in the theory of engine efficiency. Yet none of these are referred to in the work that lies before us. We do not wish to imply that the book is without scientific references. The point is that vital and fundamental references are absent. Nevertheless, we are bound to confess that the authors have turned out a useful and readable book. Two further points call for remark. The story here told is that of engineering achievement in England. References to work outside our own country are scanty. Secondly, the clarity of explanation would have been considerably strengthened by the inclusion of occasional diagrams—a feature we strongly urge upon the authors when a new edition is called for.

I. B. H.

**MISCELLANEOUS**

**Science and Scientists in the Nineteenth Century.** By the REV. R. H. MURRAY, Litt.D., with an introduction by SIR OLIVER LODGE, F.R.S., D.Sc. [Pp. xvii + 408, with bibliography and index.] (London: The Sheldon Press. Price 12s. 6d. net.)

It is both a pleasure and a privilege to read so scholarly a work as has been written by Dr. Murray in this historical survey of the scientists of the nineteenth century. The work, however, shows much more than literary scholarship, it shows also a high level of scientific study, a true understanding of human nature, and a real sense of humour, which last is, after all, the expression of a sense of proportion. The bringing of these qualities to an historical survey of scientific achievement has resulted, as might be expected, in a masterpiece, which ought to be studied by every novice to science, especially on its medical side.

Dr. Murray states in his introduction that one of his main purposes in writing this book has been to prove that there are just as many preconceived notions in science as there are in theology. Unfortunately there is at hand only too much evidence of the truth of his contention. It is not only seen that workers in the field of scientific research can lay no claim to be possessed of a broader outlook and more strictly logical habit than those in any other field of study, it is even found that they have been less ready to give fair consideration to and to adopt new ideas than workers in other fields. In an introduction Sir Oliver Lodge states that we have seen the supporters of new doctrines, the detectors of unwelcome facts coming forward apologetically, humbly presenting their credentials only to be snuffed out or else browbeaten and ridiculed by the High Priests of Science. The suggestion that there may be a branch of inquiry which even now has to run the gauntlet of fierce denial and unbending hostility, and yet may be accepted by posterity as a matter of course, is humiliating. It would not be fitting in this place to refer to any possible existing causes for humiliation, but Dr. Murray very plainly shows how over and over again scientific workers have lowered their prestige in this way. How, for instance, Jenner had to face the most scurrilous attacks from members of his own profession. The author cites, too, the absolutely childish objections brought forward by the profession itself against the use of chloroform, discovered by Dr. Simpson of Edinburgh. One would have expected that after what Dr. Simpson had himself undergone he would have exhibited some fellow feeling towards Lister, who perhaps of all our great medical discoverers suffered most keenly at the hands of his seniors, of whom Simpson was almost the most prejudiced. Possibly it was his Quaker upbringing that enabled Lister in his later years to see his own work in its proper place and proportion. Lyell was another of the few great scientists who were able to keep an open mind as to the true value of their own special theories. His renunciation, comparatively late in life, of the doctrine of special creation and adoption in its place of evolution was regarded by his scientific contemporaries as "a signal example of heroism." Surely it was no more than should be expected of any man imbued with a true scientific spirit.

In the experiments and researches that led eventually to the enunciation of the principle of the conservation of energy, both Helmholtz and Joule, approaching the problem from widely different points of view, had to meet most violent opposition. Even a man of so keen an intellect as William Thomson, later to become Lord Kelvin, declared in 1848, in spite of the experiments and determinations of Joule, that the conversion of heat into mechanical effect was probably impossible, certainly undiscovered.

Perhaps the most interesting chapters in a book, no page of which is dull, are those dealing with the growth of the idea of evolution. It is here

that Dr. Murray's special scholarship is most valuable, for it enables him to give an excellent résumé of the contest between the idea of evolution and that of special creation, tracing it forward from the very earliest times.

Dr. Murray shows how St. Augustine, on the most meagre knowledge of animal life and growth, completely rejected the doctrine of special creation, and at least paved the way to a theory of evolution—of progressive change of one kind or another. The author shows, too, that whatever stimulus was given to the study of evolutionary ideas was derived much more from the philosophers than from the scientific workers.

It is perhaps inevitable that the actual experimenters should so often be unable to see the view for the trees around them. Realising how enormous is the mass of morphological and other detail in biological studies and research we are not therefore surprised that the evolutionary theory also found its greatest opponents in the ranks of the biologists themselves. Even the Church, following the lead of St. Augustine, did not offer anything like so formidable an opposition.

A very vivid picture is given of the mental outlook and services to science of Darwin, Wallace, and all the other great biologists and shows to what extent their powers of absorbing the ideas of others were limited.

The story of Pasteur and of the beginnings of bacteriology is well told by Dr. Murray. Just as Jenner—even in time of war—had received a measure of recognition from Napoleon that was denied to him in his own country, so Pasteur found acknowledgment first in Austria and Italy of work undertaken originally for the special benefit of his own country. Thanks to the broad outlook and generous spirit of Lister the value of Pasteur's work was more quickly appreciated in England than in France.

The book witnesses to a remarkably wide range of study on the part of the author, not only in the chapters dealing with the great questions to which reference has already been made, but also in a very valuable chapter on those whom he pleases to call forgotten scientists. Some of them certainly were men who made very valuable contributions to science that were either entirely rejected or scarcely appreciated by contemporary workers. For instance, as far back as 1845, J. J. Waterston, a Scotchman, had written a paper enunciating the conception that the temperature of a gas is to be measured by the *vis viva* (kinetic energy) of the colliding molecules. Lord Rayleigh, who pre-eminently among scientists has made a cult of looking up the sources of all inspirations and theories, discovered the abstract of Waterston's paper in the *Proceedings*, and also the fact that one of the referees of the Royal Society reported that Waterston's paper was nothing but nonsense, unfit even for reading before the Society. In a final critical chapter the author deals generally and with fairness and skill with the limitations of scientists and touches upon many aspects of the subject; the position of mathematical thought and expression in relation to science; the influence of a sense of the artistic; intuition, and many other ways of approach to human thought and effort.

If only every future worker in the realm of science will absorb, as part of himself and of his daily habit of thought, the underlying principle of this book, learning its lesson from the past, much bitterness and heart-burning will be avoided and the lamp of progress will burn more brightly.

W. C. B.

**The Mind and its Place in Nature.** By C. D. BROAD, M.A., Litt.D. [Pp. x + 674.] (London: Kegan Paul, Trench, Trübner & Co., 1925. Price 16s. net.)

IN his *Scientific Thought* (1923) Dr. Broad gave us the first instalment of his Critical Philosophy, *The Mind and its Place in Nature* constitutes the second

instalment. By Critical Philosophy Dr. Broad means the critical investigation of the fundamental concepts and beliefs of common sense and of science. *Scientific Thought* was devoted to the examination of the concepts of the physico-mathematical sciences, the present volume deals with the most important concepts of biology and psychology.

The scheme of the book is as follows. The Introduction deals with some problems of method, and lays bare certain ambiguities in the terms Pluralism and Monism. Section A examines the usual views on matter and mind, the rival theories of Mechanism and Vitalism, and the Relation of Mind and Body. Section B discusses our alleged knowledge of Matter, of our own Minds, and of other Minds, also the connected problems of Memory and Introspection. Section C is mainly concerned with the common features of living organisms and minds, and with various problems relating to the so-called Unconscious. Section D disposes of the arguments usually adduced in support of the belief in the Human Survival of Bodily Death. Lastly, Section E considers the Unity of the Mind, and the Status and Prospects of Mind in Nature.

The account of Mechanism and Vitalism, as rival explanations of the behaviour of organisms, brings out a number of distinctions which are often overlooked. With the aid of these distinctions Dr. Broad differentiates certain varieties of these rival theories, and so helps to remove some of the confusion in which the issue is usually involved. According to Dr. Broad there are two kinds of Mechanism, namely, Pure Mechanism and Biological Mechanism. There are also two kinds of Vitalism, namely, Substantial Vitalism and Emergent Vitalism. Pure Mechanism posits a single kind of stuff, mere change of position, a single law according to which particles influence each other by pairs, and a single principle of composition, according to which the behaviour of any aggregate of particles (or of aggregates of aggregates) follows as a resultant from the mutual influences of the constituent particles taken by pairs. But such pure mechanism is unworkable even in the realm of physics and of chemistry. According to Biological Mechanism the behaviour of living beings might be deduced from an adequate knowledge of the physical and chemical laws which the material components would obey in isolation, but these components need not be of the same ultimate kind of stuff, and the laws of chemical composition may not be merely mechanical. Biological Mechanism has certain advantages, but needs a supplementary Deistic theory. For it rests on the analogy of self-regulating machines, and such machines do not originate without somebody's interference. Again, Substantial Vitalism explains the characteristic behaviour of living bodies by supposing these to be endowed with a special component (usually called an *Entelechy*) which is not present in inorganic or in dead bodies. Since, however, there is no other evidence in support of the hypothesis of such a special component, this theory cannot be regarded favourably, although it is not impossible. Dr. Broad himself favours the theory of Emergent Vitalism, which seems to be essentially like Prof. Lloyd Morgan's Emergent Evolution. According to the theory of Emergent Vitalism there are certain wholes (composed of certain constituents in a certain relation) whose characteristic properties as wholes cannot be deduced from the properties of the components (either in isolation or in other kinds of wholes). To understand such wholes we must study the wholes, and not merely their parts, for the wholes are not merely the resultants of the parts, but show the emergence of something more. Such emergence is already observable in the inorganic world—the knowledge of the properties of the chemical elements as such does not enable one to predict the properties of compounds. But it is most striking in the case of organisms. There appears to be a general tendency for complexes of one order to combine with each other under suitable conditions to form complexes of a higher order. At

each stage there arise things with new characteristics, and at each stage there are possibilities of further developments. As compared with Biological Mechanism, the Theory of Emergent Vitalism is simpler, as it rests within the scheme of Naturalism, and can dispense with the aid of a Deistic supplement. On its positive side (that is, apart from its rejection of rival theories), the theory of Emergent Vitalism seems to amount to little more than a confession of ignorance of the way in which the new properties emerge. Still, it is something to realise that not everything can be accounted for as a merely mechanical resultant.

The relation of Mind and Body, according to Dr. Broad, is most intimate. Mind acts on body in volition, and body acts on mind in sensation. If the relation is not described as one of interaction, it is mainly because that word does not express sufficiently the intimate character of the relation. In any case the mind is not a mere epi-phenomenon of the body, so as to be existentially dependent upon it alone. Dr. Broad has, however, an unusual view of the nature of mind. According to him mind is a compound substance whose constituents are the organism and what he describes as a "psychic factor." The psychic factor itself is not a mind, but may combine with a series of organisms to form a successive series of minds. The psychic factor may exist independently of an organism, and may combine temporarily with a medium so as to reproduce the characteristics of some dead person whose body it had once animated. This view, of course, excludes the survival of the *whole* mind of a human being. Dr. Broad bases his conclusions to some extent on what he regards as well-attested psychical phenomena. He fully realises that certain men of science will scorn him for it. But he is not awed by the pontifical pomposities of certain conceited men of science. Dr. Broad administers a timely rebuke to the type depicted in the familiar rhyme:

"You know, I'm Prof. at Impy College,  
And what I know not is not knowledge!"

Dr. Broad can be caustic on occasion, as is shown by his comments on Behaviourism, on Psycho-Analysis, and on the revival of the faculty-psychology in Prof. McDougall's psychology of instincts. Indeed, the incidental criticisms and asides are among the most valuable parts of Dr. Broad's new book.

Skipping many interesting chapters, we may turn to Dr. Broad's views on the status and prospects of mind in nature. These views are not very definite, nor are they particularly rosy. Perpetual mental progress, on the evidence available, seems to be neither inevitable nor impossible. The possibility, Dr. Broad thinks, depends on our getting an adequate knowledge and control of life before the combination of ignorance on these subjects with knowledge of physics and chemistry wrecks civilisation. "Which of the runners in this very interesting race will win, it is impossible to foretell. But physics and death have a long start over psychology and life."

Dr. Broad's new book is a monument of patience in analysis and caution in argument. It is not easy reading, but it is worth taking pains with it. The biologist, the psychologist, the methodologist, the philosopher, and many others should profit greatly from a close study of this book.

A. WOLF.

**Colour Blindness.** By MARY COLLINS, Ph.D., Lecturer in Applied Psychology, Edinburgh University. With an Introduction by DR. JAMES DREVER, George Combe Department of Psychology, Edinburgh University. [Pp. xxxi + 237.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1925. Price 12s. 6d. net.)

THIS book, which belongs to the *International Library of Psychology, Philosophy, and Scientific Method*, describes an investigation of the colour blindness of

ten students, each of whom was examined by the following twelve tests: Stilling's pseudo-isochromatic tables, a modified form of Holmgren's wool test, colour naming, colour equations, Rayleigh equation (by means of the colour disc), analysis of spectrum (made with a coloured print of the spectrum since no spectrometer was available), Bradley paper test, contrast experiments, the Nagel card test, the Edridge-Green lantern, Prof. Roaf's painting test, colour preference. The results are described at great length and "the tentative finding is that the Hering theory, the Young-Helmholtz theory, and the Ladd Franklin theory all fit the facts of red-green blindness when the defect takes an extreme form, but they seem to fail to take account of cases which suffer from this inability in a less marked degree"—a conclusion which will be generally accepted. The author has a high opinion of the Edridge-Green lantern test. She prefixes the description of her own experiments with an historical account of the subject and of the principal theories; the problem is approached in an unbiased manner, a virtue somewhat rare in colour vision work.

Within the limits she has set herself the author accomplishes her task in a very thorough manner. The difficulty arises about these limits. Colour vision is a subject in which physicists, physiologists, psychologists, and statisticians are alike interested. Each science has, of course, a perfect right to its own standpoint, and the literature of the subject is so great that it is a serious task to master more than the one line of approach. All the same the confusion prevalent in the field at present is largely due to workers confining themselves to the one line of approach. The book is written wholly from the psychological standpoint, and reveals considerable ignorance of the properties of the colour triangle. It is the colour triangle, and not the spectrum, as stated in the book, by which physicists approach the subject, and a knowledge of its properties is a great help both in marshalling facts and deducing conclusions.

R. A. H.

**Metallurgy and its Influence on Modern Progress, with a Survey on Education and Research.** By SIR ROBERT A. HADFIELD, Bt., D.Sc., D.Met., F.R.S., F.I.C., M.Inst.C.E. [Pp. xvi and 388, with 71 plates.] (London: Chapman & Hall, 1925. Price 25s. net.)

IN this most interesting book the subject of metallurgy is presented from a fresh and wide standpoint. It looks backwards and it looks forwards, yet it is very much in the present. A span of seven centuries, from the thirteenth, is covered. It shows that metallurgy is something more than a skilled art and something more than a science; it is human. Its achievements and its progress mirror the strength and weakness of humanity. It might well have been called the *Romance of Metallurgy* and as a romance it is of absorbing interest and constant inspiration.

As the Iron Age passed into that of Steel with the coming of Bessemer and open-hearth processes so is the latter, with the advent of special steels, passing into the present Age of Alloy Steels. The progress of Engineering is naturally dependent on the materials available and alloy steels enable the engineer to undertake work on a vaster scale than before. He has at his disposal material with a tensile strength of over 100 tons per square inch. He has metals which are resistant, to an increasing degree, to wear, corrosion, and high temperatures. This is the outcome of the tireless research of many workers over several years with the expenditure of much money. Metallurgy is a record of progress, and research is the foundation of metallurgical progress. Sir Robert Hadfield gives an interesting account of the early experiments in 1882 which resulted in the production of manganese and silicon steels and shows how by patient work various difficulties were overcome and these alloys made available for practical application.

The book is divided into five sections and the first part, which is historical, traces the main events and lines of progress from the time of Roger Bacon in the thirteenth century, through the mechanical age, to the present time. Part II considers ferrous metallurgy and is chiefly devoted to the properties and uses of alloy steels. There are chapters on the antiquity of iron, the influence of carbon on simple and alloy steels, the use and importance of alloy steels and their application, manganese and silicon steels, heat treatment, and the micro-structure of steel. The important subject of fuel economy is dealt with in Part III, where the characteristics of various fuels and methods of controlling the combustion as regards temperature and efficiency are considered. Also Prof. Groumc-Grjmaillo's method of applying the laws of hydro-dynamics to the problem of furnace design, which is the outcome of over twenty years of experience, is described. In this section most useful matter is condensed into a little less than fifty pages. In Part IV the author discusses the use, methods, and facilities of education and research, and devotes a chapter to the scientific societies. Part V consists of a single chapter on "The Future," in which it is considered that the advance of metallurgical science, although dependent on effort and renewed effort of the individual, will be best effected by international co-operation.

There are five appendices: (1) Translation of the poem "Ferrum." (2) List of early workers in scientific metallurgy. (3) Classified list of papers by Sir Robert Hadfield. (4) List of famous scientists illustrating the ramifications of international co-operation. (5) Extracts on methods of research from the *Art of Scientific Discovery*, by Dr. G. Gore, F.R.S.

Naturally, Sir Robert has drawn information from his many papers and addresses, and from the list of these in the appendix it is easy to obtain any fuller particulars that may be required.

The book is profusely illustrated and most readable. It should appeal to everybody who is interested in the application of science to industry and in the material progress of the country, and particularly to those young men to whom is committed the future of engineering and metallurgy.

E. COURTMAN.

**The Maori as He Was.** By ELSDON BEST. [Pp. xv + 280, with 130 figures.] (Wellington: N.Z. Dominion Museum, 1924.)

MR. ELSDON BEST probably knows more about Maori life than anyone else at the present day, or indeed than anyone else is ever likely to know, as the old customs and beliefs are fast disappearing before the march of modern civilisation. He has devoted a vast amount of time, at much personal sacrifice, to the study of this branch of the Polynesian race, and it is pleasant at last to have the results of his investigations.

*The Maori as He Was* gives a succinct account of the culture of this interesting people, and forms a valuable introduction to the subject for those who are prevented from reading the author's larger work. The book gives chapters on traditional history, on mythology and folk-lore, on religious ideals and social customs, and on arts and crafts, all of which are well illustrated both verbally and pictorially.

The question of the origin of the Polynesian race in general will always be one of interest. Elsdon Best accepts the theory of S. Percy Smith that these people came from India, largely on the grounds that their traditions state that they sailed towards the rising sun, and that they carried with them a grain, which by its description and name seems undoubtedly to have been rice. The art of cultivating this grain was subsequently lost, but since Rivers has shown that useful arts *can* disappear, e.g. even that of boat-building on an island, this forms no insuperable obstacle to the theory. The

Maori branch of the race probably inhabited Tahiti before sailing for New Zealand.

The main value of the book, however, lies in its intimate knowledge of Maori life and mentality. There is special attention paid to the interpretation of myths and legends, and to the stage reached by Maori religion, both subjects which are somewhat neglected in other works, and these accounts bear the authentic ring of first-hand investigation extended over a long period of years, during which the author patiently explored that "library" of Maori lore—the memories of the old people, for in default of a method of writing, all traditions had to be handed down orally.

It may be of interest to add, that although the old culture of the Maori is vanishing, the people themselves, in spite of their small numbers and their presence among a numerically superior white population, are not disappearing, but that census returns show a steady, though small, increase.

M. SHACKLETON.

**Technical Education: Its Development and Aims.** By C. T. MILLIS, M.I.Mech.E. [Pp. 183.] (London: Edward Arnold & Co. Price 6s. net.)

As Principal of the Borough Polytechnic Institute for a period of thirty years from 1892-1922, Mr. Millis has been in a position that enables him to write authoritatively on matters of Technical Education.

His book is an attempt to trace the growth of Technical Education in this country, to indicate its true nature, and to show how best Technical Institutes may be organised to carry out their true functions, relative to the industrial occupations of the day. At all points Mr. Millis shows the need to keep the work of these institutes in close touch with industrial needs and demands, as, for instance, in the framing of their curricula and in the training of their teachers. Among many other problems reference is made to the difficulty of avoiding "narrowness" in the Monotechnic institutes on the one hand, and on the other hand to the tendency to give too liberal, or rather, too academic a training in the wider polytechnic institutes, especially if they are regarded as local colleges. The author fears that in recent years there has been a growing habit of encouraging the more purely scientific and theoretical side of education to the neglect of the more practical and handicraft side of the trades and industries. It is not within the scope of Mr. Millis's work to trace this unfortunate tendency to its sources in the elementary and secondary schools, but this will need to be done if we are to realise the true bearing of his concluding paragraph, in which he points out that without manufactures and industries, the need for the clerk and professional worker would cease. The author truly adds that it is an evil day for a nation when it loses respect for success in industry. The book is a valuable addition to educational literature, not only for the teacher, but more especially for the administrator.

W. C. B.

**Sahara.** By ANGUS BUCHANAN, M.C., F.R.S.G.S. [Pp. 301, with 84 illustrations from photographs, and a map.] (London: John Murray, Albemarle Street, W. 1926. Price 21s. net.)

THIS book is unique in that it is dedicated to a camel, Feri N'Gashi, who later tells its own tale in one chapter.

Camels are not lovable animals, but, like the elephant and his mahout, so the riding camel and its master soon get to know each other and become inseparable, as horses and dogs will with their masters.

In his former book, *Out of the World North of Nigeria*, the author entered the fringe of Sahara and explored Aïr, one of the rare Saharan oases, or, moun-

tainous regions. The present book describes a more ambitious journey across the great desert from south to north, which took fourteen months, during which the author and Mr. Glover, his cinema operator, actually travelled 3,556 miles from Kano in Nigeria to Touggourt in Algeria, the termini of the two railways.

On both trips the author, who is an experienced field naturalist, was able to accomplish his object, thanks to Lord Rothschild and the Trustees of the Natural History Museum, who received all the scientific collections.

The collection of mammals, although small, as one would expect in desert country, proved to be of particular value, and is a welcome addition to the Natural Collection. Out of the forty-two species collected from Sahara, there were no less than ten new species, comprising a cat, ratel, dormouse, gerbil, two rats, spinous mouse, porcupine, hare, and dassie. Besides these there were ten new sub-species, giving a grand total of no less than twenty mammals new to science out of a collection of forty-two. Besides the Saharan forms twenty-two mammals were collected from the Western Sudan on the fringe of Sahara, and these produced six new species, comprising a skunk, gerbil, giant rat, and three new mice, together with a gerbil described as a new sub-species. It must be a long time since the Natural History Museum received a small collection of mammals so prolific in new forms.

Of the 207 different species and sub-species of birds collected for Lord Rothschild, about fifteen were found to be new sub-species. The author very naturally compares the trackless sandy waste over which he passed to the wide expanse of the ocean, and reminds us of the difficulty in keeping to the beaten track when there is little or nothing to guide one except the stars by night.

This is the story of a fine piece of work, both in exploration and field collecting, and the author is to be congratulated on his great exploit. The book will appeal to all those who are interested in hearing of the few remaining unknown regions of the earth and their remarkable denizens both human and animal.

The illustrations, which are numerous, are from photographs and are as good as one could wish.

R. E. DRAKE-BROCKMAN.

**American Journal of Physical Anthropology.** Edited by ALEŠ HRDLIČKA. Vol. IX, No. I. January-March 1926. (Annual Subscription \$6.25. Published at Geneva, N.Y., U.S.A., by W. F. Humphrey.)

THE longest and most important paper in the first part of vol. ix of the *American Journal of Physical Anthropology* is an account by R. G. Harris of the so-called White Indians of Panama. The first sensational newspaper reports of the discovery of a number of natives in that region with light skin, hair, and eyes was followed last year by the authoritative statement, made at the Toronto meeting of the British Association, and also by American anthropologists, that three of the eight individuals taken to the States to be examined were partial albinos. Mr. Harris undertook the examination of the Sun Blas Indians in their native habitat and the results of his researches, conducted from a genetic point of view, are now published. The very high percentage (0.7) of partial albinos found is supposed due to in-breeding, although the marriage of two "whites" is forbidden by tribal custom. The few anthropometric measurements taken prove nothing, but they suggest that the abnormal subjects are not differentiated from the normal local type by any of the usually considered characters other than the integumentary colours. Pedigrees of sixteen families and several good photographs were obtained. The main points, the ophthalmoscopic examination of the eyes, the questions of photophobia, of nystagmus and of retinal pigment on which

the grade of albinism depends seems to have been entirely overlooked. In another paper Dr. Hrdlička gives measurements of sixty-six natives of Panama, of the eight taken to Washington and of six Maya (Yucatan) skulls. He concludes, too, that the "whites" are true Indians. By applying performance tests, requiring little, if any, knowledge of language, Miss Grace Allen found that the mental ages of the five adult and normally pigmented Indians were all under ten years, and two of the partial albino children (aged 12 and 14-16 years respectively) had reached the age of 9.5 years in mental development. Dr. Schultz gives an anthropometric study of thirty-seven Nicaraguan men which is a valuable record, but the somewhat detailed statistical analysis of this population is practically valueless, as the total number of individuals is so small. The last original paper is by M. J. Herskovits on the correlation of length and breadth of head in American negroes (1,211 boys and 63 men). In dealing with the comparative material the writer appears to be quite ignorant of the fact that means, standard deviations, and coefficients of correlation all have probable errors. There are short notices of a large number of recently published books dealing with physical anthropology. The majority are reviewed by Dr. Hrdlička, and he finds occasion to express his opinion on several controverted points. He supports the somewhat unorthodox contention of Sir Arthur Keith that the Chancelade skeleton is of European rather than Eskimo type. All the original articles in this journal are followed by concise summaries. It is symptomatic of the growing realisation of the importance of measurement in dealing with questions of physical anthropology that, of the five papers in this journal, four give anthropometric data and the other gives psychological measurements. We may hope that the need for adequate statistical analysis of such material will be more fully realised later on.

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**Linking Science with Industry.** Edited by HENRY C. METCALF. [Pp. 206, with chart.] (London: Baillière, Tindall & Cox. Price 17s. 6d. net.)

THIS American book is another example of the increased attention, almost amounting to a vogue, now being paid to the subject of the relations between science and industrial management. It is virtually a record of a symposium held by the "Bureau of Personnel Administration," and contributed by leading biologists, physiologists, psychologists, historians, business administrators, personnel directors, labour leaders, and philosophers. Its purpose is to discuss the contributions of the humanistic sciences to the art of business management. The evolution of a new philosophy in business, and in a new outlook on the relations between employing organisations and the employed, is here explored. This is a book that will, therefore, be of special interest to the student of industrial psychology and of the social sciences. It should be pointed out that the series of essays comprised in this book are primarily based on conditions in America, though of course the fundamentals are common to all countries and all industrial communities of to-day.

I. B. H.

**The Biology of Population Growth.** By RAYMOND PEARL, Director of the Institute of Biological Research, The John Hopkins University. [Pp. xiv + 260.] (New York: Alfred A. Knopf, 1925. Price \$3.50 net.)

How do things grow? Is there any general form of curve which will represent the growth of individuals in size and populations in number? This is the problem that Prof. Pearl puts before us to start with. The potential

or "natural" rate of increase of any living population, whether of plants or of animals, is enormous; but in practice this is always held in check in a greater or less degree by the conditions of environment. The history of the world as a whole has probably been marked by long periods of stability, when the innate forces of growth have been balanced by the restrictive forces of environment. But if for some reason or other these latter forces are removed by a change in conditions, perhaps by an increase in the food supply—or in the case of man by the discovery of new forms of industry and new methods of food production, or by the chance of expansion into new territory—a cycle of growth will commence. At first the population will grow at an increasing rate tending towards the natural geometric rate of increase, but sooner or later the possible limits of expansion will be approached and a damping force will set in, gradually reducing the rate of growth until a stable state is again reached. If we measure the size of population vertically and the time horizontally, in simplest form, such a curve of growth will lie between two horizontal asymptotes which represent the stable periods; it will curve upwards from the first, reach a single point of inflexion, and bend back into line with the second asymptote. For a curve of general adaptability we need therefore some mathematical function which will satisfy these requirements. If  $y$  represents the size of population and  $x$  the position in the cycle, then in most general form

$$y = a + \frac{b}{1 + e^{f(x)}},$$

known as the logistic curve, satisfies these conditions.

The author gives us two examples of population growth which can be closely represented by this curve; firstly for a population of the fruit-fly, *Drosophila melanogaster*, and secondly for the case of the Arab population of Algeria. The populations of *Drosophila* were bred in stoppered bottles; starting from a few flies there was at first a rapid increase which was gradually damped down as the numbers mounted up to the total which could exist on the limited food supply.

The case of Algeria is especially interesting. Prof. Pearl gives reasons for believing that the figures for the Arab population have not been seriously influenced during the last half-century either by ideas of birth control or by increasing longevity which might have arisen from improvements in hygiene, sanitation, etc. The growth cycle, he suggests, was initiated by the introduction of agricultural improvements, irrigation, etc., by the French in the 70's and 80's after their complete pacification of the country, while the check in growth, the damping effect, has come into play because the improvements were limited, and the maximum advantage to be gained from them has almost been reached. And so between 1880 and 1920 we find almost a complete growth cycle following the logistic curve, representing an increase in population from about three up to five millions.

The curve has also been fitted to the population figures of a great many of the countries of the world. The fits are excellent, but we cannot perhaps help feeling that Prof. Pearl has laid too much stress on the universal significance of this particular form of curve. From its form it is naturally adapted to fit a series of points lying on a line which is either convex or concave, or contains a single point of inflexion, and it seems possible that nearly all the short stretches of points representing the census counts of 100 to 150 years could be fitted equally well by a simple cubic. The logistic is an extremely suggestive form of curve, but our census records are not yet long enough to make it certain that there is anything fundamentally significant in the way which it fits the data.

The growth curve of an individual has also much the same form as that of a population; initially an increasing rate of growth with  $\frac{d^2y}{dx^2}$  positive,

then a point of inflexion, and finally a decreasing rate with  $d^2y/dx^2$  negative. A very close fit to the logistic is shown, as an example, by the average change with age in the weights of a population of rats.

The second half of the book contains an inquiry into some of the factors which determine the rate of increase in human populations, and the possible factors which put a brake on this rate. Starting from the results of some experiments with *Drosophila*, and also with the fertility of hens, Prof. Pearl suggests that the density of population in itself may have a restrictive influence on fertility. It was found that hens in 100-fowl pens not only laid fewer eggs than those with more space per fowl, but than hens in 50-fowl pens who had exactly the same space per fowl. That is to say, there may be not only a physical but what may be termed a psychological factor of density influencing fertility. An analysis of the relation of density to fertility in 132 American towns leads to rather inconclusive results on this point, and further investigation is required. Finally there is a chapter on the differential birth-rate and another containing data, which is probably unique, of the sex activity according to age, duration of marriage, size of family and social class of a sample of 257 men, in which the interrelation of these various factors is discussed. There are some who view with alarm an inevitable prospect of an overpopulated world struggling for the necessities of life. But Prof. Pearl concludes his suggestive survey by stating his firm belief in the adaptive power of the human species to environmental forces, particularly to that of population pressure, and suggests that there is no more reason to be alarmed about the future of world growth than about the ordered progress of a baby into manhood.

E. S. P.

## BOOKS RECEIVED

*(Publishers are requested to notify prices)*

- The Theory of Functions of a Real Variable and the Theory of Fourier's Series. By E. W. Hobson, Sc.D., LL.D., F.R.S., Sadleirian Professor of Pure Mathematics, and Fellow of Christ's College, in the University of Cambridge. Second Edition, revised throughout and enlarged. Volume II. Cambridge: at the University Press, 1926. (Pp. x + 780.) Price 50s. net.
- Traité de Mécanique Rationnelle. Par Paul Appell. Cours de Mécanique de la Faculté des Sciences. Tome Cinquième. Éléments de Calcul Tensoriel. Applications Géométriques et Mécaniques avec la Collaboration de René Thiry, Professeur à l'Université de Strassbourg. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 198.) Price 40 frs.
- An Introduction to Mechanics. Part I. Statics. By J. P. Clatworthy, B.Sc., Lecturer in Mathematics, University, Reading. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 226, with 81 diagrams.) Price 8s. 6d. net.
- Probabilités Géométriques. Par R. Delthel, Professeur à la Faculté des Sciences de Toulouse. Tome II. Les Applications de la Théorie des Probabilités aux Sciences Mathématiques et aux Sciences Physiques. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 123.) Price 22 frs.
- Publications of the Yerkes Observatory. Volume IV, Part IV. Zone -450 of Kapteyn's Selected Areas: Parallaxes and Proper Motions of 1,041 Stars. By Oliver Justin Lee. Chicago: The University of Chicago Press. (Pp. x + 67.)
- The Earth and the Stars. By C. G. Abbot, D.Sc., Assistant Secretary, Smithsonian Institution; Director, Smithsonian Astrophysical Observatory. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. xi + 264, with 46 figures.) Price 15s. net.
- The Fundamental Concepts of Physics in the Light of Modern Discovery. Three Lectures at the Carnegie Institute of Technology, Pittsburgh. January 1925. By Paul R. Heyl, Ph.D., Physicist, Bureau of Standards, Washington. Baltimore: The Williams and Wilkins Company, 1926. London: Baillière, Tindall & Cox, 8 Henrietta St., W.C.2. (Pp. xii + 112.) Price 9s. net.
- The Quantum Theory of the Atom. By George Birtwhistle. Fellow of Pembroke College, Cambridge. Cambridge: at the University Press, 1926. (Pp. xi + 236.) Price 15s. net.
- Recueil des Travaux et Compte Rendu des Séances. Publié sous la Direction du Bureau Central de la Commission. The National Physical Laboratory, Teddington, England. Commission Internationale de l'Éclairage en succession à la Commission Internationale de Photométrie. Sixième Session. Genève—Juillet, 1924. Cambridge: at the University Press, 1926. (Pp. 432.) Price 15s. net.

La Théorie de la Relativité. Par M. von Laue. Traduction faite d'après la quatrième édition allemande, revue et augmentée par l'auteur. Par Gustave Létang. Tome II. La Relativité Générale et la Théorie de la Gravitation d'Einstein. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 300.) Price 65 frs.

Les Bases Physico-Chimiques de la Régénération. Par Jacques Loeb. Paris: Gauthier-Villars et Cie, 1926. (Pp. ix + 172, with 115 illustrations.) Price 30 frs.

This is a French translation by H. Mouton of the original American edition which was recently reviewed in our columns. The translation appears to have been carried out carefully and with due regard for the author's meaning, but the price charged for the book strikes one as being regrettably high.—P. H.

Practical Physiological Chemistry. By Sidney W. Cole, M.A., Trinity College, Cambridge. University Lecturer in Medical Chemistry, Cambridge, Hon. Director of Pathological Chemistry, Charing Cross Hospital Medical School. Seventh Edition. Cambridge: W. Heffer & Sons, 1926. (Pp. xii + 478, with 60 figures.) Price 16s. net.

Qualitative Inorganic Analysis. By D. R. Snellgrove, Ph.S., M.Sc., A.I.C., and J. L. White, D.Sc., Lecturer on Inorganic Chemistry, Battersea Polytechnic. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 281, with 14 diagrams.) Price 7s. 6d. net.

A Dictionary of Applied Chemistry. By Sir Edward Thorpe, C.B., LL.D., F.R.S., Emeritus Professor of General Chemistry and Director of the Chemical Laboratories of the Imperial College of Science and Technology, South Kensington, London. Assisted by Eminent Contributors. Vol. VI, revised and enlarged Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. viii + 791.) Price £3 net.

Recent Advances in Physical and Inorganic Chemistry. By Alfred W. Stewart, D.Sc., Professor of Chemistry in the Queen's University of Belfast. Fifth Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. x + 312, with 5 plates.) Price 18s. net.

Physico-Chemical Methods. By Joseph Reilly, D.Sc., F.Inst.P., F.I.C., William Norman Rae, M.A., F.I.C., and Thomas Sherlock Sheeler, Ph.D., B.Sc., F.I.C. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 735.) Price 30s. net.

Outlines of Inorganic Chemistry. A Textbook for Schools. Part I. Non-Metals and some Common Metals. For Matriculation Students. By J. Morris, M.A., Senior Chemistry Master, Collegiate School, Liverpool. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvi + 201, with 23 figures.) Price 7s. 6d. net.

Lectures on Certain Aspects of Biochemistry. By H. H. Dale, M.D., F.R.S., J. C. Drummond, D.Sc., F.I.C., L. J. Henderson, A.M., M.D., A. V. Hill, Sc.D., F.R.S. London: University of London Press, 17 Warwick Square, E.C.4, 1926. (Pp. viii + 313.) Price 12s. 6d. net.

The States of Aggregation. The Changes in the State of Matter in their Dependence upon Pressure and Temperature. By Gustav Tamman, Director of the Institute for Physical Chemistry at the University of Göttingen. Authorised Translation from the Second German Edition, by Robert Franklin Mehl, Ph.D., National Research Fellow. London: Constable & Co., 1926. (Pp. xi + 297.) Price 24s. net.

- Chemistry in Modern Life. By Svante August Arrhenius, Ph.D., M.D., D.Sc., LL.D., Director of the Nobel Institute, the Swedish Academy of Sciences, President of the Swedish American Foundation. Translated from the Swedish and revised by Clifford Shattuck Leonard, Ph.D. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. vii + 286.) Price 15s. net.
- A Textbook of Organic Chemistry. By Dr. Julius Schmidt, Professor of Chemistry in the Technische Hochschule, Stuttgart. English Edition by H. Gordon Rule, Ph.D. (Munich), D.Sc., (Edm.). London: Gurney & Jackson, 33 Paternoster Row, E.C.4; Edinburgh: Tweeddale Court, 1926. (Pp. xxiv + 798.) Price 25s. net.
- The Vredefort Mountain Land in the Southern Transvaal and the Northern Orange Free State. By A. L. Hall, M.A., Sc.D., Assistant-Director Geological Survey of the Union of South Africa, and Dr. G. A. P. Molengraaff, Professor of Geology at the Technische Hoogeschool in Delft, Shaler Memorial Series. *Verhandelingen der Koninklijke Akademie van Wetenschappen te Amsterdam (Tweede Sectie) Deel XXIV, No. 3.* Amsterdam: *Litgave van de Koninklijke Akademie van Wetenschappen*, 1925. (Pp. xiv + 183, with 39 plates and a Geological Map.
- An Introduction to Earth History. By Hervey Woodburn Shimer, Professor of Palæontology in the Massachusetts Institute of Technology. London: Gunn & Company. (Pp. viii + 411, with 141 figures.) Price 12s. 6d. net.
- The Petrology of the Igneous Rocks. By F. H. Hatch, O.B.E., Ph.D. Eighth Edition, revised with the Assistance of A. K. Wells, D.Sc., Lecturer on Petrology, University of London, King's College. London: George Allen & Unwin, 40 Museum Street, W.C.1; New York: The Macmillan Company. (Pp. xxiv + 566, with 144 figures.) Price 15s. net.
- Catalogue of the Machæridia (Turrilepas and its Allies) in the Department of Geology. By Thomas Henry Withers, F.G.S., Assistant in the Department. London: British Museum (Natural History). Printed by Order of the Trustees, 1926. (Pp. xv + 99, with 8 plates.) Price 7s. 6d. net.
- Life of Plants. By Sir Frederick Keeble, Professor of Botany in the University of Oxford. Oxford: at the Clarendon Press, 1926. (Pp. xii + 256, with 51 figures.) Price 5s. net.
- Soil and Civilisation. A Modern Concept of the Soil and the Historical Development of Agriculture. By Milton Whitney, Chief, Bureau of Soils, U.S. Department of Agriculture. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. x + 278, with 32 illustrations.) Price 15s. net.
- The Nervous Mechanism of Plants. By Sir Jagadis Chunder Bose, M.A., D.Sc., LL.D., F.R.S., C.S.I., C.I.E., Director, Bose Research Institute, Calcutta. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. xix + 224.) Price 16s. net.
- Evolution. By J. Graham Kerr, F.R.S., Regius Professor of Zoology in the University of Glasgow. London: Macmillan & Co., St. Martin's Street, 1926. (Pp. x + 276, with 53 figures and 2 plates.) Price 12s. net.
- Life and Evolution. An Introduction to General Biology. By S. J. Holmes, Ph.D., Professor of Zoology in the University of California. New York: Harcourt, Brace & Company. (Pp. v + 449, with 227 figures.)

- Evolution, Genetics, and Eugenics. By Horatio Hackett Newmann, Professor of Zoology in the University of Chicago. Chicago, Illinois: The University Press. (Pp. xx + 639, with 92 figures.)
- Biological Memory. By Eugenio Rignano, Professor in the University of Milan and Editor of "Scientia." Translated, with an Introduction, by E. W. MacBride, D.Sc., LL.D., F.R.S., Professor of Zoology, Imperial College of Science and Technology. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1926. (Pp. vi + 253.) Price 10s. 6d. net.
- Problems of Bird Migration. By A. Landsborough Thomson, O.B.E., M.A., D.Sc. London: H. F. & G. Witherby, 326 High Holborn, W.C.2, 1926. (Pp. xv + 350.) Price 18s. net.
- The Origin of Birds. By Gerhard Heilmann. London: H. F. & G. Witherby, 326 High Holborn, W.C., 1926. (Pp. v + 208, with two plates in Colour and 140 photographs and text-figures from drawings by the Author.) Price 20s. net.
- Heredity. By A. Franklin Shull, Professor of Zoology in the University of Michigan. London: McGraw-Hill Publishing Co., 6 Bouverie St., E.C.4, 1926. (Pp. xi + 287, with 111 figures.) Price 15s. net.
- The Comparative Anatomy, Histology, and Development of the Pituitary Body. By G. R. de Beer, M.A., B.Sc., F.L.S., Fellow of Merton College; Jenkinson Memorial Lecturer in Comparative and Experimental Embryology in the University of Oxford. Biological Monographs and Manuals, edited by F. A. E. Crew and D. Ward Cutler. Edinburgh and London: Oliver & Boyd, Tweeddale Court, and 33 Paternoster Row, E.C., 1926. (Pp. xix + 108, with 11 plates and 118 figures.) Price 12s. 6d. net.
- British Birds. Written and Illustrated by Archibald Thorburn, F.Z.S., in Four Volumes. Vol. III. Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. x + 167, with 192 plates.) Price 16s. net.
- Race and History. An Ethnological Introduction to History. By Eugène Pittarel, Professor of Anthropology at the University of Geneva. London: Kegan Paul, Trench, Trübner & Co.; New York: Alfred A. Knopf, 1926. (Pp. xxiii + 505.) Price 21s. net.
- Emotion and Insanity. By S. Thalbitzer, Chief of the Medical Staff in the Copenhagen Asylum St. Hans Hospital. With a Preface by Prof. Harald Høfding. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1926. (Pp. x + 128.) Price 7s. 6d. net.
- Comparative Physiology. By Lancelot T. Hogben, M.A., D.Sc., Assistant Professor in Zoology, McGill University. London: Sidgwick & Jackson, Ltd., 1926. (Pp. xiii + 219, with 21 figures.) Price 7s. 6d. net.
- Muscular Activity. By Archibald Vivian Hill, M.A., Sc.D., F.R.S., Professor of Physiology, University College, London. London: Baillière Tindall & Cox, 8 Henrietta St., Covent Garden, W.C.2. (Pp. iv + 115, with 47 figures.) Price 12s. 6d. net.
- Constitution Orthobiontique des Êtres Vivants. I. Théorie Orthobiontique. Par Charles Janet. Beauvais: H. Dumontier, 1925. (Pp. 84.)
- The Endocrine Organs. An Introduction to the Study of Internal Secretion. By Sir E. Sharpey-Schafer, LL.D., D.Sc., M.D., F.R.S., Professor of Physiology in Edinburgh University. Second Edition. Part II. The Pituitary, The Pineal, The Alimentary Canal; The Pancreas and the Sex Glands. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. xxii + 418, with 203 figures.) Price 20s. net.

- Human Physiology. By John Thornton, M.A. Third Edition. Completely revised by William A. M. Smart. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. vii + 463, with 281 illustrations.) Price 10s. 6d. net.
- The Wonders of the Human Body. A Health Reader for Schools. By Margaret A. Shuttleworth. London: University of London Press, 17 Warwick Square, E.C.4, 1926. (Pp. xi + 195, with 38 figures.) Price 2s. 6d. net.
- The Importance of Diet in Relation to Health. The People's League of Health Lectures. London: George Routledge & Sons, 68 Carter Lane, E.C., 1926. (Pp. xii + 130.) Price 3s. 6d. net.
- The Secretion of Urine. By Arthur R. Cushing, M.A., M.D., LL.D., F.R.S. Second Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. xii + 288.) Price 16s. net.
- Civil Aviation. A Report by the Joint Committee on Civil Aviation of the U.S. Department of Commerce and the American Engineering Council. London: McGraw-Hill Publishing Co., Bouverie St., E.C.4, 1926. (Pp. xvii + 189, with 31 figures.)
- Telephone Communication Systems. By Royce Gerald Kloeffer, Professor in Electrical Engineering, Kansas State Agricultural College. New York: The Macmillan Company, 1925. (Pp. vii + 284.) Price 17s. net.
- Photography. A Practical Manual of Photographic Surveying Methods. By Arthur Lovat Higgins, M.Sc., A.R.C.S., A.M.Inst.C.E., Lecturer in Civil Engineering, East London College (University of London). Cambridge: at the University Press, 1926. (Pp. xv + 139.) Price 6s. net.
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- The Mystery of Mind.** By Leonard T. Troland, S.B., A.M. Ph.D., Assistant Professor of Psychology, Harvard University. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. xi + 253.) Price 15s. net.
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- The Philosophical Presuppositions of Mathematical Logic. By Harold R. Smart, M.A., Ph.D., Assistant Professor in Philosophy, Cornell University. New York: Longmans, Green & Co., 1925. (Pp. 98.) Price \$1 net.
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- Scott's Polar Journey and the Weather, being the Halley Lecture delivered on 17 May, 1923. By G. C. Simpson, F.R.S. Oxford: at the Clarendon Press, 1926. (Pp. 31.) Price 2s. 6d. net.
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## CORRIGENDA

(For Article on Pure Mathematics in this Number)

- Page 190, line 4. For  $y = tn$ , write  $y = tx$ .  
" " " 28. For  $(O,o)$ , write  $(o,o)$ .  
" 192, " 3. For  $y'^2$ , write  $y'^2$ .  
" " " 9. Ditto.  
" " " 31. For  $\frac{1}{k}$ , write  $\frac{1}{k'}$ .  
" 193, " 10. Insert *of* after triangle.  
" " " 34. For  $h(u) - x$ , write  $h(u)\bar{x}$ .  
" 195, " 20. For *Darboren*, write *Darboux*.  
" " " 32. Ditto.  
" " " 43. For *Bampiani*, write *Bompiani*.



# SCIENCE PROGRESS

## RECENT ADVANCES IN SCIENCE

**PURE MATHEMATICS.** By F. PURYER WHITE, St. John's College, Cambridge.

*Algebraic Geometry.*—If an algebraic surface  $F$  be projected from a point  $O$ , which may either be external to  $F$  or may belong to it with a certain multiplicity, upon a plane, we get a representation of the surface upon a multiple plane of a certain order  $n$ , with a branch curve  $C$ .  $C$  is the image of the curve of contact of the cone of vertex  $O$  circumscribed to  $F$ , apart from the cone which projects the double curve (supposed not cuspidal) of  $F$ . The order  $m$  of  $C$  is connected with the genus  $\pi$  of sections of  $F$  by planes through  $O$  and the number of intersections of  $F$  with straight lines through  $O$ , the order  $n$  of the multiple plane, by the formula

$$m = 2n + 2\pi - 2.$$

But  $C$  is not a general curve of order  $m$ ; even if  $F$  has general moduli (with a double curve and triple points), and for a projectively general surface  $F$  of the proper order,  $C$  possesses a certain number of nodes and cusps; the nodes arise, in general, from the bitangents of  $F$  through  $O$ , and the cusps from the straight lines through  $O$  which have three-point contact with  $F$ . The question now arises: when can a plane curve  $C$  be taken as the branch curve of a multiple plane? F. Enriques (*Annali di mat.* (4) (1924) 185-98) has now completed the investigation of this problem, which began to occupy his attention as far back as 1912. Taking an arbitrary plane curve  $C$ , of even order, we attempt to construct  $F$ ; we first construct a plane algebraic curve  $K$ , represented upon an  $n$ -ple straight line  $a$ , of which the branch points are given as the intersections of  $a$  with  $C$ . Then, varying  $a$  in a pencil in the plane of  $C$ , we consider the surface described by  $K$ . If  $K$  is rationally determined by the parameter of the line  $a$  in its pencil, the locus will be the surface  $F$ , but, in general,  $K$  is not so rationally determined, and the set of branch points will determine a finite number of algebraic curves, *i.e.* of

$n$ -ple lines, birationally distinct, which are permuted when  $a$ , varying in its pencil, returns to its initial position. We thus have first to examine how these birationally distinct  $n$ -ple lines are permuted as  $a$  varies in the pencil  $y = tn$ ; this is done by reference to the corresponding Riemann surfaces and the complex  $t$ -plane, in which we consider the elementary closed paths corresponding to simple tangents to  $C$  belonging to the pencil and to the lines passing through its nodes and cusps. Secondly, the elementary conditions of invariance of the  $n$ -ple line are found and, finally, it is proved that these conditions are also sufficient to determine rationally a curve  $K$ , which describes the surface  $F$  required. Enriques also obtains the following important corollary: If one curve  $C$  of a continuous system of plane curves is a branch curve for an  $n$ -ple plane, the same is true for the other curves of the system.

A real algebraic curve  $f(x, y) = 0$ , in which the coefficients are real and which has an infinite number of real points, has a certain number ( $\geq 1$ ) of branches or circuits, so that the function  $y = y(x)$  defined by the equation is *reducible* in the real field into several different functions. O. Chisini (*Annali di mat.* (4) 1 (1924) 147-73) considers the analytic representation of such functions in their entirety, *i.e.* given a real branch of an algebraic curve to construct a pair of real analytic functions  $x = x(t)$ ,  $y = y(t)$ , of a parameter  $t$ , which are regular (save for possible poles) for all real values of  $t$  and which represent the entire branch. A simple example is given by the elliptic cubic  $y^2 = x(x^2 - 1)$ , composed of an oval  $\Gamma$  and an odd branch  $\Gamma_1$ ; a straight line from the point  $(0, 0)$  of the oval meets this again in one further point  $P$ . Thus the co-ordinates of  $P$  are single-valued functions of the parameter of the straight line, in fact  $x = \frac{1}{2}(t^2 - \sqrt{t^2 + 4})$ ,  $y = \frac{1}{2}t(t^2 - \sqrt{t^2 + 4})$ , where the positive square root is taken. Moreover, by the substitution  $\tau = (e^{\lambda t} - 1)/(e^{\lambda t} + 1)$ , where  $\lambda = \pi/2a$ , the strip of the complex  $t$ -plane, of breadth  $2a$ , with the real axis as centre line, is represented upon the circle  $(\tau) \leq 1$ , and thus the real functions  $x$  and  $y$  of  $t$ , which are single valued on the real axis of the  $t$ -plane and therefore remain regular within such a strip for  $a$  sufficiently small, are expressed as analytic, though no longer algebraic, functions of  $\tau$ , which multiplied perhaps by a product of the type  $(\tau - \alpha)(\tau - \beta) \dots$  become regular within the circle  $(\tau) \leq 1$  and may be developed as power series in  $\tau$  convergent within the unit circle. The problem is thus solved in this particular case. Another example in which the solution is easy is when the curve, of genus  $p$ , has the maximum number  $p + 1$  of branches. In this case the linear series  $g_{p+1}^1$  constructed from a set of  $p + 1$  points taken one on each of the branches is cut out on  $f$  by a pencil of curves, each

of which meets each of the branches in a single point, and the method proceeds as before. Chisini now proves that such a representation is possible in general and he gives a method by means of which the representation can be effectively obtained.

The method of "small variations," which consists in deducing from a curve of given order, made up of several curves of lower order, an irreducible curve of the same order, has proved extremely fruitful for the construction of curves satisfying given conditions. Essentially it goes back as far as Plucker, who used it for the generation of a curve of order  $n$  by means of  $n$  straight lines, but it was Klein who first employed it systematically for the investigation of the real singularities of curves and surfaces. We may mention in particular his construction, from the two ellipses  $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$ ,  $\frac{x^2}{b^2} + \frac{y^2}{a^2} = 1$ , of a

quartic curve  $\left(\frac{x^2}{a^2} + \frac{y^2}{b^2} - 1\right) \left(\frac{x^2}{b^2} + \frac{y^2}{a^2} - 1\right) + \epsilon = 0$ , which for  $\epsilon$  a sufficiently small positive number consists of four ovals and has 28 real bitangents. And there is his well-known paper on cubic surfaces (*Gesammelte Mathematische Abhandlungen*, t. 2, pp. 11-62), in which he deduces the shapes of various types of cubic surface from the well-known surface with four nodes. The method has been applied by Hilbert and later writers to the investigation of the various types of plane curves of a given order with the maximum number of circuits, and a good deal is now known on this subject (see L. Brusotti, *Rend. Lombardo*, t. 43, 47-9). For space curves, however, much less has been done; it has been known, since the fundamental memoirs on space curves by Halphen and Noether, that a space curve of given order which has the maximum number of circuits must lie on a quadric, but the only types investigated until recently were those constructed by Hilbert (*Math. Ann.*, 38, 1891, 115-38) to prove the actual existence of such curves. Hilbert made use of an auxiliary conic to construct irreducible curves of order  $n$ , without singularities and having the maximum number of circuits, consisting only of even circuits if  $n$  is even, and of one odd circuit in addition if  $n$  is odd; he also used an auxiliary quartic consisting of two odd circuits to construct space curves with several odd circuits. M. Piazzolla-Beloch (*Annali di mat.* (4) 2 (1925), 203-16) now carries this work a good deal further, obtaining new types of curves on the quadric and examining the mutual relations of the circuits.

*Projective Differential Geometry.*—Just as in algebraic geometry the projective point of view has come into prominence as compared with the more elementary, metrical treatment, so in differential geometry, within the last fifty years, there

has been a notable development, which has not yet reached the textbooks, in a similar direction. If we consider, for example, the formula  $\rho = (1 + y'^2)^{3/2}/y''$  for the radius of curvature of a plane curve  $y = f(x)$ , we may inquire the meaning of the vanishing of numerator or denominator. The equation  $y'' = 0$  means that the point in question is an inflexion on the curve, clearly a projective property, *i.e.* one which is preserved if the plane be subjected to a collineation. The equation  $1 + y'^2 = 0$ , on the other hand, means that the tangent passes through one of the circular points at infinity; this is not a projective property of the curve itself, but may be regarded as a projective property of the system formed by the curve and the circular points. We may thus systematically investigate first all combinations of  $y, y', y'' \dots$  which, put equal to zero, have a projective meaning for the curve itself, and secondly, all such combinations which have a projective meaning for the system consisting of the curve and the circular points; and then it is clear that all formulæ of metrical, Euclidean, differential geometry must be obtainable from expressions of these two kinds. If we substitute for the projective group any other continuous group of transformations the theory will be included in the general theory of differential invariants as developed by Lie. The special theory of projective differential variants (and thus what is now known as projective differential geometry) was begun by Halphen in 1878 (*Œuvres*, t. 2, pp. 197-253), who formed all the rational integral functions of the independent variable  $x$ , the dependent variable  $y$  and its first  $n$  derivatives, which for any collineation are only multiplied by a factor, up to the value  $n = 7$ . The simplest differential invariant is  $U = y_4$ . (We shall put  $y_k = \frac{1}{k} \frac{d^k y}{dx^k}$ ). Set equal to zero it means, as we have seen, that the point is an inflexion on the curve; the equation  $U = 0$  is the differential equation of the straight line. This last remark suggests the next simplest invariant,  $V = y_2 y_5 - 3y_3 y_4 + 2y_3^2$ ; the equation  $V = 0$  being the differential equation of the conic. At a point of the curve  $V = 0$  if six consecutive points lie on a conic, *i.e.* if the point be a *sextactic point*. The next invariant to be considered is

$$\Delta = \begin{vmatrix} y_3 & y_4 & y_5 & y_6 & y_7 \\ y_3 & y_4 & y_4 & y_5 & y_6 \\ -y_2^2 & 0 & y_3^2 & 2y_2 y_4 & 2y_3 y_5 + y_4^2 \\ 0 & y_3^2 & 2y_2 y_3 & 2y_2 y_4 + y_3^2 & 2y_2 y_5 + 2y_3 y_4 \\ 0 & 0 & y_3^2 & 3y_2 y_3 & 3y_3^2 + 3y_2 y_4 \end{vmatrix}$$

of the seventh order.  $\Delta = 0$  is the differential equation of

one of the important class of curves known as W-curves, first examined by Klein and Lie, which are invariant for a continuous one-parameter group of linear transformation. Such curves can be put into the form  $x^a y^b z^c = \text{const.}$ , where  $a + b + c = 0$ ; the cross ratio of the four points consisting of the point of contact of a tangent and its intersections with the three sides of the triangle of reference is constant along the curve, hence the name W-curves, from "Wurf," von Standt's word for cross-ratio. The special case given by  $\Delta = 0$  is a curve which by suitable choice of the triangle reference becomes logarithmic spiral cutting the radii vectores at an angle of  $\frac{1}{2}\pi$ . A fourth invariant can now be formed from the foregoing; namely,  $H = \frac{256\Delta^3 - 27V^3}{U^4}$ , in which the numerator is divi-

sible by the denominator. The differential equation  $H = 0$  again gives a special class of W-curve, cubic curves with a cusp. The expression  $\Delta^3/V^3$  is the lowest absolute invariant, and the differential equation of the seventh order  $\Delta^3 - kV^3 = 0$  gives a W-curve for which the cross-ratio  $\lambda$  is determined by  $k$ .

Following on the work of Halphen comes Wilczynski, who developed the theory using homogeneous co-ordinates which naturally give the advantage of greater symmetry. A curve  $C$ ,  $x_i = x_i(u)$  ( $i = 1, 2, 3$ ), determines a linear differential equation of the third order  $x''' + 3p_1 x'' + 3p_2 x' + p_3 x = 0$ , the  $p$ 's being functions of  $x$ , of which  $x_1, x_2, x_3$  form a fundamental system of integrals, i.e. linearly independent. Any integral is a linear function of  $x_1, x_2, x_3$  with constant coefficients, and any three such, which are linearly independent, form a fundamental system. This shows that the differential equation is the same for any curve obtained from  $C$  by a collineation and thus the differential equation defines a class of projectively equivalent curves. The equations  $x_i = x_i(u)$  contain, however, arbitrary elements, not belonging to the curve itself. In the first place only the ratios are of importance, and a transformation  $x = h(u) - x$  will not affect the curve  $C$ . Moreover, in place of the parameter  $u$  we may use any other,  $v = v(u)$ . If therefore we form combinations of  $p_1, p_2, p_3$ , which are unaffected by such transformations, they will be expressions which are of significance for the curve itself, a significance not affected by a collineation. Hence the projective invariants of the curve are the invariants of the differential equation. The principle of duality also comes in; it appears that the co-ordinates of a tangent of the curve satisfy a differential equation covariantly connected with the original equation, its lagrange adjoint. On these lines Wilczynski develops the theory systematically, as may be seen in his book *Projective Differential Geometry*, Trubner, 1906. A great part

is played by the osculating conic, the osculating cubic (passing through 9 consecutive points of the curve) and the pencil of 8-point cubics. These latter all have a further point in common, the Halphen point, and include, besides the osculating cubic, another curve of special importance, the *penosculant nodal cubic*, which has a node at the point. Wilczynski also deals in the same way with space curves, using a linear differential equation of the fourth order. He also investigates ruled surfaces; here he begins with two linear differential equations of the second order

$$\begin{aligned} y^{11} + p_{11}y^1 + p_{12}z^1 + q_{11}y + q_{12}z &= 0, \\ z^{11} + p_{21}y^1 + p_{22}z^1 + q_{21}y + q_{22}z &= 0, \end{aligned}$$

forms four pairs of solutions  $y_1, z_1$ , such that the determinant

$$D \begin{vmatrix} y_1^1 & y_2^1 & y_3^1 & y_4^1 \\ z_1^1 & z_2^1 & z_3^1 & z_4^1 \\ y_1 & y_2 & y_3 & y_4 \\ z_1 & z_2 & z_3 & z_4 \end{vmatrix}$$

does not vanish, remarks that if  $\eta_i = \alpha y_i + \beta z_i$ ,  $\xi_i = \gamma y_i + \delta z_i$ , ( $i = 1, 2, 3, 4$ ), the four pairs of solutions  $\eta_i, \xi_i$  equally form a fundamental system, and thus concludes that the equations define, except for collineations, a ruled surface. The condition  $D = 0$  means simply that this surface is not a developable.

A notable improvement on Wilczynski's work has been made recently by C. Sannia, who remarks that this is clearly a case in which the absolute differential calculus of Ricci, with which the Theory of Relativity has made us so familiar, should be used. He therefore develops an absolute differential calculus of one variable (*Atti Torino*, 62 (1922) 293) of which the principle is as follows. By some law to a function  $f$  of  $n$  there corresponds a function  $\bar{f}$  of  $\bar{n}$ ; if for every transformation  $n = n(\bar{n})$  we have  $fdu^n = \bar{f}d\bar{u}^{\bar{n}}$  (for a certain integer  $n$ ), then  $f$  is said to be a *covariant of order  $n$* , and is denoted by  $f_n$ . Thus, if to the coefficient  $a_1$  of a differential  $A = a_1du$  we make correspond that of the transform  $\bar{a}_1d\bar{u}$  of  $A$ ,  $a_1$  is a covariant of order 1, and  $a_1^n$  of order  $n$ . We now introduce, instead of the ordinary derivatives  $f^{(n)}$ , *covariant derivatives*  $f_n$  with respect to a differential  $A$ , defined by  $f_{n+1} = f_n' - nSf_n$ , in which  $S \approx a_1'/a_1$ . These covariant derivatives satisfy the ordinary rules of the differential calculus when applied to rational functions of the covariants.

Using this we get, instead of the ordinary differential equation, an equation

$$f_3 + 3p_1f_2 + 3p_2f_1 + p_3f = 0,$$

which has an invariant character with respect to all transformations of the parameter. It depends, however, on two arbitrary elements, the common factor  $h(u)$  and the differential  $A$ ; normalising these, that is to say, fixing them by intrinsic laws invariant for collineations, we arrive at a *normal* equation, in  $(1,1)$  correspondence with the curve and its collineations. By this means Sannia (*Rend. lincei* 5, 31 (1922) sem. 1, 450-4, 503-6; sem. 2, 17-19, 432-4) is able to define a projective arc, curvature and normal, in complete analogy with the ordinary metrical theory. Still more recently (*Annali di mat.*, (4) 1 (1924) 1-18, 3 (1925) 1-26) Sannia has applied the same method to space curves, obtaining, among other things, projective Frenet-Senet formulæ. He here has to distinguish between curves which belong to a linear complex and those which do not, constructing two different projective metrics in the two cases—also twisted cubics fall outside both categories, being lines of zero length in both metrics.

The projective differential geometry of surface is of late development, although its beginnings are to be found in a paper by Darboren as far back as 1880. Its systematic development, with the use of the asymptotic curves as parameter curves, is due to Wilczynski and his pupils (see *Trans. Amer. Math. Soc.*, vols. 8-10 (1907-9), and, in particular, a paper by Green, *ibid.*, vol. 20 (1919). Later work on the subject is by C. Fubini (*Rend. Palermo*, 43 (1919) 1-46), E. Čech (*Annali di mat.*, (3), 31 (1922) 191-206, 251-78) and E. Bompiani (*Rend. lincei*, 5, 33 (1924) 85-90). An important part in the theory is played by the Lie quadric, which osculates at the point  $O$  of the surface, the ruled surface of asymptotic tangents of one system at the points of the asymptotic curve of the other system which passes through  $O$ ; other important notions are those of the *Darboren tangents*, the three triple lines of the involution of triads of tangents to the curves, with a triple point at  $O$ , determined by the quadrics which have second order contact with the surface at  $O$ , and of the *Segre tangents*, which are their conjugates with respect to the asymptotic tangents. Reference may also be made to an interesting paper by C. Sannia (*Annali di mat.*, 3, 31 (1922) 165-89), who compares the metrical, affine, and projective differential geometries of a surface, showing how each may be made to depend upon two differential forms of the first order, one quadratic and one cubic.

These considerations have recently led E. Bompiani (*Rend. lincei*, 6, 3 (1926) 118-23) to re-examine some work of Segre on a projective invariant of two plane curves having at a point  $O$  a contact of order  $(\mu - 1)$ , where  $\mu > 1$ . Taking  $y = 0$  as the common tangent, the Taylor expansions for the two curves

will be, in non-homogeneous co-ordinates,  $y = ax^u + \dots$ , and  $y = \bar{a}x^u + \dots$ ; then  $a/\bar{a}$  is invariant for collineations and we may give it the following geometrical interpretation. Consider a straight line passing near the point  $O$  and cutting the two curves and their common tangent in  $\bar{P}$ ,  $P$ ,  $T$ . Then if  $M$ , different from  $O$ , is any point of this straight line, the limit of the cross-ratio (PPTM) as the straight line tends to  $O$  is  $a/\bar{a}$ , independently of the limiting position of the line (different from the tangent) and of  $M$ . Segre remarked that for the case of two branches of the same cycle of a curve the ratio  $a/\bar{a}$  is a root of unity; it is precisely this case which Bompiani examines in detail, and by considering the curve and, first, its osculating conic, secondly, its osculating nodal cubic, he obtains two infinitesimal projective invariants of the curve and from them a finite differential invariant. A subsequent paper by A. Terracini (*ibid.*, 584-91) carries the matter further and emphasises its connection with the "projective normal" of Sannia and Fubini.

Another method of attack for the projective differential geometry of surfaces is by means of line geometry, considering the complex of tangents of a surface. This is due in the first place to C. Thomsen (*Math. Zeits.*, **21** (1924), *Jahresbericht D. Math. Verein.*, **34** (1925), *Abhand. Math. Seminar. Hamburg*, **4** (1925) 232-66), and recently, from the proper point of view of a quadric in five dimensions, by E. Bompiani (*Rend. lincei*, **6** (1926) 395-400).

Here we may leave the subject, with the remark that a treatise on the whole subject by C. Fubini and E. Cech is announced as in the press, if indeed it is not already published.

**ASTRONOMY.** By W. M. H. GREAVES, M.A., Royal Observatory, Greenwich.

*Planetary Temperature derived from Water-cell Transmissions.*—In the *Astrophysical Journal* for April 1926, O. H. Menzel, W. W. Coblentz, and C. O. Lampland give an account of the derivation of Planetary Temperatures as derived from an extensive series of radiometric measurements made at the Lowell Observatory, Flagstaff, during 1924.

The principle of the method is as follows: The radiation from a planet is measured with a thermocouple, first through a screen of quartz, glass, or fluorite and secondly through a water-cell. The water-cell transmits radiation which lies between wave-lengths  $0.3 \mu$  and  $1.4 \mu$ , while the screens of quartz, glass, and fluorite are transparent up to  $4 \mu$ ,  $8 \mu$ , and  $12.5 \mu$  respectively. Now the radiation from a planet consists of reflected solar radiation of short wave-length, and planetary

radiation of long wave-length. From the observations the water-cell transmission  $W$  is derived, and this gives the ratio of the planetary radiation to the total radiation. The amount of planetary radiation will depend on the surface temperature of the planet and on its emissivity  $e$  (for a perfect radiator  $e = 1$ ), and the amount of reflected solar radiation will depend on the planet's distance from the sun, and on its albedo  $A$ . Both the planetary and solar radiation are affected by the absorption due to the earth's atmosphere. If, then,  $W$  be observed, we have the material for the calculation of the surface temperature  $T$  of the planet, provided  $e$  and  $A$  and the atmospheric transmissions for the long and short wave-lengths concerned be known. As far as the albedo is concerned, it was assumed that the albedo for the total incident solar radiation is equal to the known visual albedo. The emissivity  $e$  was taken to be unity.

It was necessary to introduce a factor  $x$  into the computation which allowed for any spottedness or inequalities of illumination of the planet and which was, in fact, the ratio of the brightness of the region under consideration to the average brightness of the entire surface. This factor  $x$  is somewhat uncertain in many cases, but fortunately it enters into the final result only as the fourth root.

Observations on Mars were made at various dates on the centre of the disc and on the north, south, east, and west limbs. The measures at the centre range from  $-36^{\circ}\text{C.}$  to  $+22^{\circ}\text{C.}$  The observed temperatures on the east limb were considerably lower than those on the west limb, showing that the Martian afternoon is considerably warmer than the morning. The lowest temperature recorded for the east limb is  $-86^{\circ}\text{C.}$ , which is probably near to or somewhat greater than the night temperature of the planet.

The lowest temperature recorded for the south polar area was  $-108^{\circ}\text{C.}$ ; as the south polar cap melted the temperature rose, the highest being  $+6^{\circ}\text{C.}$  These temperatures were computed, using  $x = 1$ . Since the polar cap is considerably brighter than the rest of the planet, these temperatures are too low. The authors consider that  $x = 3$  is not unreasonable, and using this value it is indicated that the temperature at the edge of the polar cap is  $0^{\circ}\text{C.}$ , thus suggesting that the disappearance of the polar cap is due to the melting of snow and ice.

The temperatures derived for Venus, Jupiter, and Saturn were  $+57^{\circ}\text{C.}$ ,  $-140^{\circ}\text{C.}$ , and  $-145^{\circ}\text{C.}$  respectively. The value for Venus is somewhat in doubt owing to uncertainty in  $x$ . The values for Jupiter and Saturn confirm the theoretical work of Jeffreys, who suggested that the temperatures of these planets are low and are maintained by solar radiation alone, internal heat contributing little or nothing.

The observed temperatures on the moon range from  $+50^{\circ}\text{C}$ . with the sun  $16^{\circ}$  above the lunar horizon in the lunar morning to  $+120^{\circ}\text{C}$ . with the sun  $75^{\circ}$  above the lunar horizon in the lunar afternoon.

*Continuous Hydrogen Absorption in Early-type Stars.*—Simple quantum considerations predict that if an element can emit a series of lines, the lines of this series will crowd together as we pass into the violet, and when we go still further into regions of shorter wave-lengths beyond the series limit, the element should be capable of continuous emission. Conversely the same element will absorb light of the wave-length of each of the series of lines, and as we pass to the violet side of the series limit there should be a region of continuous absorption.

This phenomenon is well known in the case of the Balmer series of hydrogen lines in early-type stellar spectra, and has been the subject of a quantitative spectro-photometric study by Ch'ing-Sung Yü, the details of which are published in *Lick Observatory Bulletin*, number 375. Ch'ing-Sung Yü finds that the continuous absorption begins, not at the series limit of the Balmer lines at 3646, but to the redward side of the limit, that the absorption increases rapidly until the last line of the observed series is reached, and that as we pass still farther into the region of shorter wave-lengths the absorption is practically constant. Furthermore, he finds that the amount of the continuous absorption varies from star to star with the strength of the line absorption.

The observational material consisted of all stars of spectral types B<sub>5</sub> to A<sub>9</sub> brighter than the 4th photographic magnitude and north of Declination  $-18^{\circ}$ . In addition to these, one or more bright stars were shown from each of the other types, beginning at Oe<sub>5</sub> and extending to as far as K<sub>5</sub>. The total number of stars observed was 91.

Spectra of these stars were secured with the two-prism quartz spectroscope attached to the Crossley reflector of the Lick Observatory. Immediately before or after the photographing of the stellar spectra, 6 standard exposures of a lamp, with intensities increasing by the ratio 1:2 were impressed on the plates, the light from the lamp passing through an ultra-violet filter. These standard exposures served to determine the variation of density of photographic deposit with light intensity. The densities of photographic deposit of each spectrum for various wave-lengths and of the six standard exposures were measured in a Hartmann micro-photometer, and the distribution of light intensity in each spectrum thereby deduced. In order to eliminate various effects such as the absorption in the optical train, each spectrum was compared with that of a comparison star which was photographed and

measured in precisely the same way. The star selected was  $\zeta$  Ophiuchi, a star of class B0, in which the continuous absorption which is the subject of the investigation is absent. An effective temperature of  $22,000^\circ$  K. was assumed for this star, the distribution of energy in its spectrum calculated on this basis, and the distribution of energy in the spectra compared with it deduced.

As a by-product of the investigation, the effective temperatures of 69 stars are given; but these are subject to uncertainty.

It was found that to the violet side of the series limit, the absorption was practically constant. Ch'ing-Sung Yü points out that this conflicts with a theoretical result obtained by E. A. Milne, namely, that the intensity of the absorption should be proportional to the cube of the wave-length. It seems doubtful to the writer of the present article whether the range covered by the observations, which extend down to  $3,300\text{ \AA}$ , is sufficient to discriminate between Milne's law and a law of constant absorption.

Ch'ing-Sung Yü's paper contains two beautiful reproductions of the spectra of  $\alpha$  Leonis and  $\beta$  Ursa Majoris. In these the continuous absorption is very apparent.

*The Sun's Velocity with Respect to Faint Stars.*—In *Lick Observatory Bulletin*, number 374, P. van de Kamp gives a determination of the sun's velocity with respect to stars of magnitude 9 to 10. The radial velocities of 105 stars between visual magnitudes 9 and 10 and within  $50^\circ$  of either the apex or antapex of the sun's motion were determined with the light one-prism spectrograph and 6-inch camera attached to the 36-inch Lick Observatory refractor. The dispersion was approximately 6 mm. between  $H_\beta$  and K. Such spectra cannot give good *individual* radial velocities, but provided the velocities obtained be free from systematic error, they should give nearly as accurate a determination of the solar motion as that derived from first-class radial velocities. In fact, great individual accuracy is not needed, for the individual velocities of the stars are subject to dispersion, and in obtaining a solar motion the statistical "probable deviation" of individual stars is about 11 km/sec, so that one does not gain much by cutting down the observational "probable errors" to small amounts. For the short spectra used in the present investigation the probable error of a single radial velocity derived from a good plate amounts to 14, 8, and 6 kilometres per second respectively for stars of spectral types A to F, G, and K.

Separate determinations were made from stars of spectral types A and F, from stars of spectral type G, and from stars of spectral type K. In the case of G and K type stars spectra

were also obtained of various bright stars for which high-class radial velocities were available. These served for the determination of systematic errors in the determinations. A systematic correction was found which varied with wave-length. No sensible systematic correction to the one-prism velocities depending on zenith distance was found.

The results obtained for the sun's velocity are as follows :

From stars of spectral types A and F	. . .	$17.0 \pm 3.4$ km/sec.
From stars of spectral type G	. . .	$16.9 \pm 3.9$ km/sec.
From stars of spectral type K	. . .	$20.3 \pm 4.4$ km/sec.
From all stars	. . .	$18 \pm 2.2$ km/sec.

These values are believed to be affected by no systematic errors larger than 1 km/sec.

The mean visual magnitude of the stars used was 9.2. The above value of the solar motion relative to such stars does not support the work of Seares, who from a comparison of the secular parallaxes of stars of different apparent magnitudes with their annual parallaxes as derived from the luminosity and density functions, has found that the sun's velocity changes with the brightness (both absolute and apparent) of the reference stars. According to Seares, the solar velocity with respect to 10th magnitude stars amounts to 25 kilometres per second.

**PHYSICS.** By L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

*The Measurement of X-ray Intensities.*—The direct measurement of the intensity of a beam of X-rays is accompanied with such serious difficulties that, in general, indirect methods of measurement are employed. The latter are divided into two main groups which depend respectively on the ionising and the photographic action of the rays. It is therefore clearly necessary that we should be able readily to compare the results obtained by an ionisation method with those obtained by a photographic method. For this we require to know, firstly, the law of blackening of a photographic plate by X-rays of various wave-lengths, and, secondly, the relation between the blackening and the ionisation produced by X-rays of a given wave-length.

A study of the blackening of the photographic plate was undertaken by Glocker and Traub (*Phys. Zeit.*, 22, p. 345, 1921). They caused a beam of X-rays from a Coolidge tube to fall on a secondary radiator, the primary radiation being so adjusted that the characteristic radiation was excited, and the latter was allowed to fall upon a photographic plate, placed about 4 cm. away from the radiator. The blackening of the plate

was measured by means of a Marten photometer. By employing radiators of various metals, plates were exposed to characteristic radiations of different frequencies and were then developed in groups of three to six. The law of blackening was found by plotting the values obtained with the Marten photometer against the logarithm of the time of exposure to the X-rays, the condition of the tube being maintained constant. In this way a set of curves corresponding to the characteristic radiations from elements such as strontium, silver, antimony, and barium were obtained, and the individual curves were found to be accurately parallel to one another. This meant that the law of blackening was the same in each case. In particular, the independence of the blackening curve of the wave-length was very clearly shown in the case of the radiations from silver and selenium, where it chanced that with corresponding modes of operation of the tube, equal blackening of the two plates was found for a certain time of exposure. The result was that for all other equal times of exposure the blackening was equal in the two cases. No trace of any change in the nature of the blackening curves was found when the incident radiation approximated in frequency to those of the characteristic radiations of bromine and silver. This extraordinary simplicity of the law of blackening, found for X-rays of wave-lengths 0.4 to 1.1 Å, is of considerable practical importance, for it means that the intensities of mixed beams of fixed composition can be determined photographically, *e.g.* two lines which for a given exposure produce equal blackening, will always produce equal blackening when the exposures are prolonged or shortened. Further, the above result contradicts the widespread impression that very hard X-rays produce less blackening than soft X-rays for equal times of exposure. The authors state that they considered the refinement of their experimental arrangements, to permit the use of strictly monochromatic radiation, were likely to lead to no further results of importance, and such refinements were therefore not made.

The relation between the blackening of a photographic plate and the ionisation produced by X-rays of given wave-lengths was investigated by Berthold and Glocker (*Ann. der. Phys.*, **76**, p. 409, 1925, and *Z. für Phys.*, **31**, p. 259, 1925), who devised an arrangement by which homogeneous radiation simultaneously produced effects in an ionisation chamber, and on a photographic plate. The necessary radiation was produced by using anticathodes of different materials and causing the excitation of the corresponding K radiations. The latter traversed suitable filters which were so chosen that the edges of their absorption bands were situated between

the  $\alpha$  and  $\beta$  radiations from the anticathode, so that the  $\beta$  and  $\gamma$  lines, together with the scattered radiation, were so reduced in comparison to the  $\alpha$  radiation that they could almost be left out of account. The anticathodes possessed three faces, on each of which was mounted a different material, in order that by rotation of the anticathode during an experiment, records could be obtained with characteristic radiations from different metals without altering the condition of the tube. Silver was mounted on one face of all the anticathodes used, in order to provide a standard for comparison. The filtered radiation passed through a lead slit to an ionisation chamber. The lead slit was kept open for such a period of time that the electrometer moved a definite number of divisions, and the blackening simultaneously produced on a photographic plate, placed immediately above the ionisation chamber, was determined by means of a Marten photometer. (A comprehensive discussion of photographic spectralphotometry is given by Dorgelo, *Phys. Zeit.*, **26**, p. 756, 1925. See also Fabry, the fifth Hurter and Driffield Memorial Lecture, *Proc. Roy. Photo. Soc.*, April 20, 1926.) The elements used in the anticathodes were selected from a range of iron to thorium; they were mounted either in the form of thin sheets, or mixed with water-glass and placed on the rough surface of the anticathode, or mixed with water-glass and graphite and embedded in a small pit in the surface of the anticathode. Now, according to Wooton's rule (*Phys. Rev.*, **13**, p. 71, 1919) the intensity of the characteristic radiation from a given element is proportional to  $V^2 - V_0^2$ , where  $V$  is the applied potential and  $V_0$  is the excitation potential for the characteristic radiation to be investigated. To find the range of  $V$  within which the tube could be worked without the excitation of an undue amount of general radiation, the intensity of ionisation produced by the filtered radiation was plotted against  $V^2$ . If this radiation were purely characteristic a straight line would be obtained, but on the passage of general radiation a departure from the straight line would occur. In this way, it was found in the case of platinum that the radiation was homogeneous when the tube was worked with  $V$  equal to 1.2 to 1.5  $V_0$ . In addition, the homogeneity of the radiation was tested by examination with a Seeman spectrograph.

We have seen that the blackening of a photographic plate corresponding to a constant ionisation effect was measured for a series of different characteristic radiations, of which silver was taken as a standard. Therefore the ratio of the amount of blackening obtained with any characteristic radiation to that obtained with the characteristic radiation of silver was the ratio of the blackening to the ionising effect for the former

radiation. The values of this ratio obtained with the various anticathodes are given below :

Fe	Cu	Se	Rb	Zn	Mo	Ag	Sn	Ce	Pt	Th
·51	·50	·49	·48	1·03	1·02	1·0	1·0	2·4	1·95	1·6

These values show at once the influence of the bromine and silver absorption edges, for regions of wave-length just below which the plate is much more sensitive than the ionisation chamber. It is thus clear that no direct comparison is possible between the intensity measurements by one method, and those by the other, unless a calibration curve, obtainable from the numbers above, is available. The above results permitted the determination of the variation of the ratio of the photographic to the ionisation effect with frequency, (*a*) when equal amounts of energy were incident on the plate and on the chamber, and (*b*) when equal amounts of energy were absorbed by the plate and by the chamber. The result in case (*b*) was capable of expression in the form  $\text{Blackening/Ionisation} = m (c - \gamma)$ , where  $m$  and  $c$  are constants and  $\gamma$  is the frequency of the incident radiation. These experimental results were checked by measurements made with an ionisation chamber containing air under pressure.

The next series of measurements which must occupy our attention are those made to determine the relation between the energy and the ionising powers of beams of X-rays of different frequencies. We know that the number of pairs of ions,  $n$ , produced by the absorption of an amount of energy,  $E$ , from a beam of X-rays of constant frequency is given by the equation  $E = n \epsilon$ . The question therefore arises whether the factor  $\epsilon$  depends on the frequency. Experiments by Boos (*Z. für Phys.*, **10**, p. 1, 1922) indicated that  $\epsilon$  varied with the frequency. In order to check this result Griebe and Kriegesmann (*Z. für Phys.*, **28**, p. 91, 1924) measured the energy of the beam of X-rays by a thermometric method over a range of wave-lengths from 0·15 to 0·5 Å. A lead plate was placed in a Dewar flask and absorbed most of the radiation incident upon it. This flask was connected by a capillary tube to a similar flask containing an identical piece of lead which could be heated electrically. When a drop of liquid was placed in the capillary tube to serve as indicator, the apparatus formed a differential air thermometer, and the square of the heating current was a measure of the energy of the X-ray beam. Corrections were applied for the energy absorbed by the glass walls, etc. Identical radiation fell upon an ionisation chamber, so that the ratio of the energy to the ionisation could be determined. The frequency of the radiation was varied by

working the tube at different potentials and employing copper filters of suitable thicknesses. For five wave-lengths, within the region mentioned, the ratio of energy to ionisation was expressed in arbitrary units and the results showed a rapid increase of this ratio with decrease in wave-length, in fact, according to an exponential law. Kriegesmann (*Z. für Phys.*, **32**, p. 552, 1925) has drawn attention to the difficulty of calculating the energy required for the formation of a pair of ions, using X-rays of the given frequencies, owing to the inexact knowledge of the absorption coefficient of air for these frequencies.

More recently, Kulenkampff (*Ann. der Phys.*, **79**, p. 97, 1926) has investigated the relation between the energy and the ionising power over a range of wave-lengths from 0.3 to 2.3 Å, and he found no variation of  $\epsilon$  with wave-length, and Grebe has since informed him that over the range 0.5 to 1.5 Å he, too, has found no variation of  $\epsilon$ ; but it must be recorded that there is a big discrepancy between the results of Kulenkampff and Grebe for the numerical value of  $\epsilon$ . Kulenkampff used filtered K radiation from various anticathodes, and measured the energy of an incident beam by the heat generated when the rays were absorbed in a series of small silver plates, which were attached to a sensitive iron-bismuth thermopile. The ionisation produced by an identical beam was measured by means of an ionisation chamber designed according to the recommendation of Becker and Holthusen (*Fortschr. a. d. Geb. d. Röntgenstr.*, **26**, p. 211, 1919, *Ann. der Phys.*, **54**, p. 625, 1921. Further see O. Berg (*Wiss. Veröffentl. a. d. Siemens Konz.*, **3**, p. 162, 1924). Since ionisation chambers thus designed have been so generally used in the experiments described in this article, it will not be out of place to summarise their essential features quite briefly. Firstly, the volume of the chamber must be great enough to prevent even the swiftest photoelectron from reaching the walls of the chamber; i.e. all the energy of ionisation must be communicated to the air inside the chamber. Secondly, secondary radiation from the walls or windows must play no part. Thirdly, the volume of ionised gas must be exactly defined, and, finally, with the maximum amount of ionisation saturation must definitely be attained.

Kulenkampff's measurements were carried out in the following manner. The sensitivities of the thermopile and electrometer systems were determined at the start and finish of each set of readings; that of the thermopile was found by illuminating it with light from a Hefner lamp, the total radiation of which has been carefully studied by Gerlach (*P. Zeit.* **14**, p. 577, 1913). The ionisation produced by the

passage of X-rays through the chamber for three seconds was then measured; the thermopile was next placed in position and the heat generated during an exposure to the rays of two minutes' duration was found. Many readings were taken to overcome the variations produced by slight changes in the voltage applied to the tube. The filtered radiation was examined with an ionisation spectrometer. For each anticathode the spectrum of the unfiltered radiation and the absorption coefficient of the filter were found, so that the spectrum of the filtered radiation could be deduced. The fractions of the incident radiation actually absorbed by the thermopile and the ionisation chamber were also calculated. The results for five different wave-lengths showed that  $\epsilon$  was constant within the limits of experimental error. The numerical value of  $\epsilon$  was found to be 35 volts per pair of ions. Kulenkampff considers that this value is correct to within 15 per cent. It will be remembered that the very early experiments of Rutherford and McClung, carried out with mixed radiation, gave a value of  $\epsilon$  equal to 87 volts per pair of ions. It must be recorded, however, that whilst Kulenkampff found  $\epsilon = 35$ , Grebe found  $\epsilon = 21$ , and even allowing for the 15 per cent. error suggested by Kulenkampff, the discrepancy is far outside experimental limits. Kircher and Schmitz (*Zeit. für Phys.*, vol. 36, p. 484, 1926), using the experimental arrangements devised by Grebe, found that  $\epsilon$  was equal to 20.4 volts per pair of ions and was very approximately constant from 0.5 to 1.5 Å. They state that, although our imperfect knowledge of the absorption coefficient of air does not permit definite statements for regions of short wave-length, yet there appears to be a considerable increase in the amount of energy required to produce a pair of ions in these regions.

Finally, mention must be made of a series of measurements carried out by Bouwers (*Z. für Phys.*, 14, p. 174, 1923; *Physica*, 3, p. 113, 1923). He showed, in agreement with Glocker and Traub, that the law of blackening of a photographic plate was independent of the wave-length. Moreover, he evolved a simple method of calculating the absorption due to the film, by using five films instead of one in the photographic cassette, so that these were all blackened to different extents. Bouwers measured the energy of the filtered radiation from five anticathodes, Pt, Wo, Sb, Ag and Mo, by means of a vacuum bolometer, the absorption of the bolometer for each wave-length being calculated. The individual radiations were also allowed to fall on photographic plates for equal times of exposure and the blackening was measured in the usual way. Thus, in each case the ratio of the blackening to the energy could be found. The values of this ratio for the K radiations from the above

metals were 1.0, 1.0, 1.4, 1.95, and 2.6 respectively (the wave-lengths varied from 0.2 to 0.9 Å), *i.e.* the ratio was apparently constant for the shorter wave-lengths.

Kulenkampff's work enables us to compare the work of Bouwers and of Berthold and Glocker. The results of Berthold and Glocker shows a small increase in the value of the ratio of blackening to ionisation with increase in wave-length over the region investigated by Kulenkampff, who has pointed out that this increase occurs owing to the fact that they have assumed the absorption coefficient of air to be proportional to the third power of the wave-length in this region, and that the increase disappears if the absorption coefficient is calculated as in Kulenkampff's paper. The constancy of this ratio was established by Bouwers for the shorter waves in the region under consideration.

The work described in this article is undoubtedly of great interest to all engaged in the practical measurements of X-ray intensities. Whilst an examination of the experimental material and results gives the impression that it is now possible to compare the results obtained by different methods with a certain amount of confidence, and whilst the constancy of  $\epsilon$  appears well established over the range of wave-lengths in everyday use, yet the discrepancy in the values of  $\epsilon$  obtained by different observers is a serious matter, and one which deserves the attention of skilled research workers who have the necessary apparatus at their disposal.

*Single Crystals of Metals.*—In the May Lecture, 1926, to the Institute of Metals, Prof. Carpenter dealt with the preparation and properties of single crystals of metals. It is now well known that such single crystals may be prepared in two different ways. Firstly, the liquid metal may be so manipulated that on solidification it forms a single crystal. Secondly, a piece of metal consisting of a very large number of crystals formed in the ordinary course of solidification may be converted into a single crystal by suitable after-treatment with heat and tension. The latter method is the one which has been so extensively and successfully employed by Carpenter and Elam (*Proc. Roy. Soc.*, vol. 100, p. 329, 1921) in their study of single crystals of aluminium. Their work is so well known that only a brief description of their procedure is necessary here. The crystals in the parallel portions of a suitable flat test piece of aluminium were converted into a single crystal in the following manner. A preliminary heat treatment caused complete softening of the metal and produced new equiaxed crystals of approximately uniform size. The crystals were then strained in order to bring them into such a state of activity that further heat treatment caused them to grow,

so that, finally, a single crystal was formed by the absorption of a very large number of small crystals. The time required for the production of such a single crystal was about 100 hours, and on an average one experiment in every four was effective.

Edwards and Pfeil (*Journ. Iron Steel Inst.*, vol. 109, p. 129, 1924) have employed the method of Carpenter and Elam to produce single crystals of iron. They used test pieces 8 to 12 in. long, 1 to 2 in. wide, and  $\frac{1}{8}$  in. thick, from which all traces of carbon were previously removed by heating the iron in an atmosphere of hydrogen. The specimens were then strained much more than in the case of aluminium, and annealed for several days. It was found that after this treatment a large number of small crystals still remained on the surface, and these had to be cut away.

Single crystals, more or less accidentally produced from the melt, have long been known, but Czochralski (*Zeit. Phys. Chem.*, vol. 92, 219, 1918) appears to have been the first to prepare such crystals experimentally. He withdrew a rod of metal from the melt at a definite rate, and the metal thus solidified in the form of a long thin thread, which was found to consist of a single crystal. Gompertz (*Zeit. für Phys.*, vol. 8, p. 184, 1922) followed the same procedure, but used a silica rod, which he dipped into the melt and then withdrew at a constant rate in an atmosphere of neutral gas. Crystals of tin, lead, and antimony were thus successfully made.

More recently, however, Bridgman (*Proc. Amer. Acad. Sci.*, 60, p. 305, 1925) has used the method of slow solidification from the melt to produce large single crystals of tungsten, antimony, bismuth, tellurium, cadmium, zinc, and tin, and his method has been used by Miss Elam to produce large single crystals of copper, silver, and gold, which were exhibited at the May lecture. Bridgman caused the metal under investigation to melt in an antechamber attached to a cylindrical mould of glass or quartz which was placed horizontally in a furnace. The melting was carried out *in vacuo*. After molten material had been shaken to remove occluded gases, the mould and furnace were rotated into a vertical position so that the melt passed through a narrow capillary tube into the main chamber of the mould, whilst the impurities were retained in the antechamber. Some of the molten material then passed through another capillary tube, at the other end of the mould, into a small tube. When this tube and the mould were filled, the mould was slowly lowered through the bottom of the furnace into a pipe containing air or oil. The result was that solidification started at the lower end of the tube and proceeded gradually along its axis, keeping pace with the lowering of the

mould. When only one nucleus was formed at the lower end, and the rate of lowering of the mould was sufficiently slow, the whole metal usually crystallised as one grain, and the final casting, of course, exhibited the cylindrical shape of the mould and not the geometrical form characteristic of the crystalline system of the metal. If more than one nucleus formed in the tube, then the capillary between the tube and the main chamber of the mould usually acted as a filter and allowed only one grain to extend into the main chamber, so that, provided precautions were taken to prevent the formation of fresh nuclei, a single crystal was formed there. It will be observed that this method at present permits no control of the orientation of the casting, although it was found that the most general method of growth was with the principal cleavage plane parallel to the axis of the mould. For example, no castings of antimony were found to be orientated in any other way.

Experiments on the properties of single crystals are of great importance. The properties of single crystals of aluminium were extensively examined by Carpenter and Elam. Bridgman (*loc. cit.*) determined with the above-mentioned metals the linear compressibilities, the elastic constants which give the extensions under simple tensions in the principal directions, the thermal expansions, the specific resistances, and the temperature and the initial pressure coefficients of specific resistance. The results showed that the plane of cleavage, or the plane of easiest slip in case the crystal did not cleave, corresponds to some fundamental property of crystal structure which has important bearings on the other physical properties of the crystal. Thus, the linear compressibility was always greatest across the cleavage plane—across which the atoms are more loosely bound—and actually, in the case of tellurium the linear compressibility in the cleavage plane was negative. Similarly, the thermal expansion was also greatest across the plane of cleavage, and again, tellurium gave a negative thermal expansion in the cleavage plane itself. Analogous effects were found in the case of the other properties. Unfortunately our present knowledge does not permit us to give a theoretical treatment of these interesting results. The great importance of single crystals of iron can scarcely be exaggerated, and Honda and his collaborators (*Nature*, May 29, 1926) have prepared crystals of iron by the method of Edwards and Pfeil, and have investigated the magnetisation curve and the magnetostriction effect. It is noteworthy that the hysteresis loss of a single crystal of iron is about one-tenth that of ordinary sheet-iron containing silicon.

*A New Absolute Manometer.*—E. Brüche (*Ann. der Phys.*,

79, p. 695, 1926) has recently described an extremely neat apparatus for the precise measurement of very low pressures, which possesses the great advantage that it is not attacked by gases. Formerly, only two absolute manometers which fulfilled this condition were known, namely, the Knudsen absolute manometer and the quartz fibre manometer. The Knudsen manometer (*Ann. der Phys.*, **32**, p. 809, 1910) possesses the disadvantage that a pressure determination is a lengthy procedure. The quartz fibre manometer, designed by Haber and Kerschbaum (*Zeit. für Elektrochem.*, **20**, p. 296, 1914) is based on the work of Langmuir (*J.A.C.S.*, **35**, p. 105, 1917) and consists of a thin quartz fibre which is suspended vertically from one end and allowed to oscillate in the gas whose pressure is required. The damping of the oscillations is taken as a measure of the pressure of the gas. This manometer possesses many advantages, but it is slow in action, is relatively insensitive at the lowest pressures owing to the internal friction of the quartz, and is liable to be strongly affected by external vibrations. It must be noted that the action of the instrument is also dependent on the molecular weight of the gas, and this disadvantage cannot be avoided. Brüche, however, has replaced the fibre by a thin vane of quartz, and has paid great attention to the elimination of the other defects. The vane is suspended vertically by means of two thin fibres of quartz, so that the system may swing freely with the plane of the vane perpendicular to the direction of motion, but will not be subject to external disturbances. A small piece of iron wire, encased in a light quartz tube, is attached to the back of the vane, together with a light pointer. The system is deflected magnetically until the vane rests against a stop, and is then allowed to oscillate, and the time taken for the amplitude of the oscillation to decrease to one-half of its initial value is recorded. Brüche investigated the behaviour of such an instrument over the region  $5 \times 10^{-4}$  to  $2 \times 10^{-2}$  mm., but it may be used in regions well outside these limits. It is very quick in action, and the time required for a pressure determination is about one minute. The general equation of this manometer contains three constants, two of which, namely, the molecular weight and the coefficient of viscosity, vary from gas to gas, but in many cases a simplified formula may be used and pressure determinations may be made with the aid of a simple graph. The sensitivity in the region investigated increases with decreasing pressure, and the mean error is of the order of 2 per cent.

*Glaser's Experiments.*—Since we have so frequently had occasion to deal with the experiments of Glaser on a new phenomenon exhibited by diamagnetic gases, it is necessary

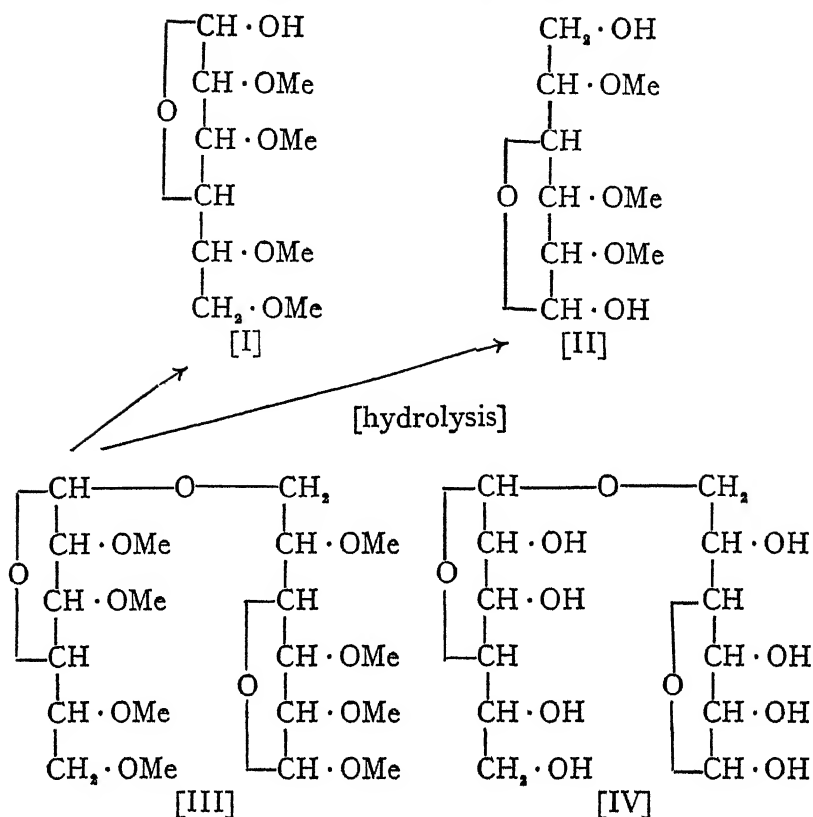
to direct attention to a short paper by E. Zehrer (*Zeit. für Phys.*, **37**, p. 155, 1926), who states that in his experiments he did not find evidence of this phenomenon. Zehrer placed a horizontal tube containing gas in magnetic field. One-half of this tube was kept at room temperature, and the other half was electrically heated up to 250° C. Owing to the different values of the susceptibility at different temperatures there existed a difference of pressure between the two ends of the tube. This difference of pressure is theoretically proportional to the total pressure of the gas. Measurements on the gases argon and carbon dioxide made with a sensitive manometer showed complete agreement between experiment and theory, and the method was used to determine the susceptibilities of the above gases.

**ORGANIC CHEMISTRY.** By J. N. E. DAY, B.Sc., A.I.C., University College, London.

THE structure assigned to maltose by Haworth and Leitch (*J.C.S.*, 1919, **115**, 809) has recently been modified, the position given to the oxygen bridge in the glucose molecule, and the point of attachment of the two glucose residues in the maltose molecule, having both been altered. In view of the importance of a knowledge of the structure of maltose in the chemistry of the sugars, a summary of this work is given.

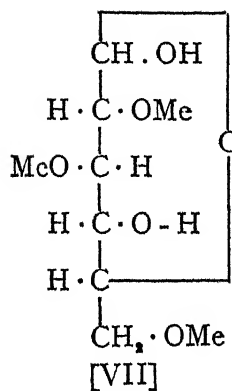
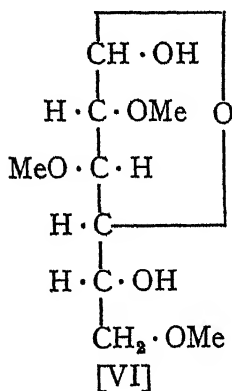
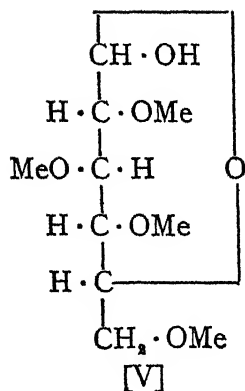
The structure of maltose was determined by Haworth and Leitch (*loc. cit.*) in the following way: the maltose on complete methylation gave hepta-methyl methylmaltoside, and this latter compound gave, on hydrolysis, a tetramethyl glucose and a trimethyl glucose. These were separated by distillation, when the tetramethyl compound crystallised and was identified as the crystalline 2 : 3 : 5 : 6 tetramethyl glucose [I], (butylene oxide form). The trimethyl compound did not crystallise and the structure was determined by analysis both of the compound and of the product of oxidation. It was believed that evidence was obtained that the terminal  $-\text{CH}_2\cdot\text{OH}$  group was free, and therefore the structure [II] was given to this compound, which was believed to be 2 : 3 : 5 trimethyl glucose (butylene oxide form). Thus the two glucose residues are connected through the terminal carbon atoms. The structure of the heptamethyl methylmaltoside will, therefore, be given by [III], and that of maltose by [IV].

The structural formula of glucose has been revised by Charlton, Haworth, and Peat (*J.C.S.*, 1926, 89). The butylene oxide formula had been applied to glucose on the assumption that the five-membered ring would be the least strained of any of the possible ring forms. This assumes an analogy between

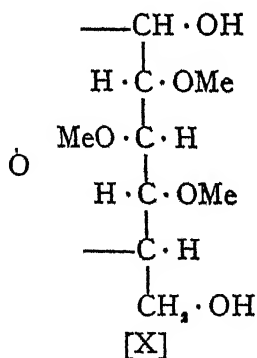
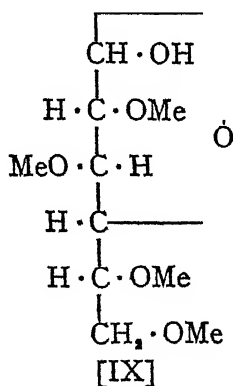
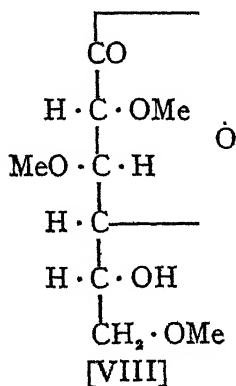


this ring and cyclo-pentane, which is the least strained polymethylene ring according to the Baeyer strain theory. In the above paper, Haworth and his co-workers give, in a series of curves, the results of the study, by polarimetric methods, of the rate of hydrolysis of the normal and  $\gamma$  forms of the lactones from tetramethyl glucose, tetramethyl galactose, trimethyl arabinose, and trimethyl xylose. They found that these curves belonged to two distinct types. It was found that the curve for the lactone from normal tetramethyl glucose belonged to the same type as the normal forms of the other three sugars. These three have been shown to have the amylenoxide structure, and therefore it is concluded that normal tetramethyl glucose is represented by the amylenoxide form [V].

Confirmatory evidence is given from a study of 2:3:6 trimethyl glucose—this must now be represented by [VII] in place of [VI]. This on further methylation is converted into normal tetramethyl glucose [V]. On oxidation, however, there

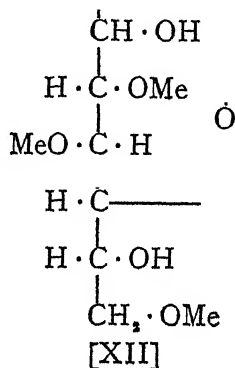
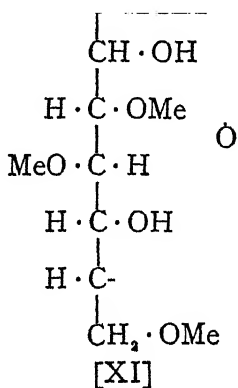


are two positions available for the attachment of the lactone ring. According to Hudson's rule, this lactone will be represented by the  $\gamma$  form [VIII]. It has now been shown that, on complete methylation, this lactone gives the tetramethyl gluconolactone identical with that derived from the labile form of tetramethyl glucose [IX]. This then is further evidence that while the normal form of glucose contains an amylenic ring, the labile form possesses the butylenic ring.

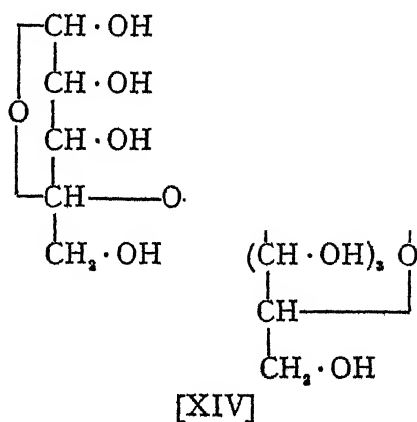
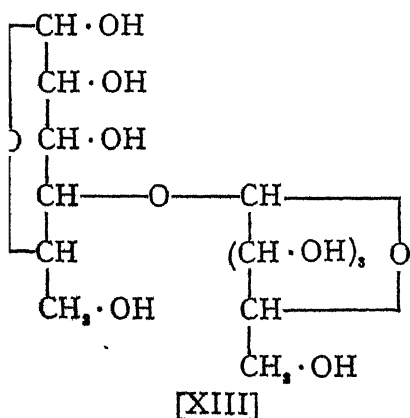


Irvine and Black (*J.C.S.* 1926, 862) have again raised the question of the constitution of maltose. These authors have now obtained from maltose, by methylation followed by hydrolysis, crystalline tetramethyl glucose, and a crystalline trimethyl glucose, which they have shown to be 2 : 3 : 6 trimethyl glucose [XI], and not the liquid form described by Haworth and Leitch as 2 : 3 : 5, or 2 : 3 : 4 [X] if the amylenic oxide structure is adopted. There is one point of difficulty in the fact that there are now more disaccharides, which give the 2 : 3 : 6 compound on hydrolysis, than can be accounted for on stereo-

chemical grounds. The probable explanation of this is the fact that 2 : 3 : 6 trimethyl glucose can react as a  $\gamma$  sugar. Therefore, the production of 2 : 3 : 6 trimethyl glucose does not necessarily distinguish between the positions 4 and 5 as the connecting link of two glucose residues. Any disaccharide which contained the  $\gamma$  form of the glucose would, after methylation and hydrolysis, give the  $\gamma$  form of the trimethyl glucose [XII], which would change to the stable form [XI].



It is, therefore, now suggested that the structure of maltose is either [XIII] or [XIV].



Cooper, Haworth, and Peat have published a paper (*J.C.S.*, 1926, 876) stating that 2 : 3 : 6 trimethyl glucose is obtained from maltose, and correcting the earlier work of Haworth and Leitch. These authors give to maltose a constitution similar to that given by Irvine and Black.

**GEOLOGY.** By G. W. TYRRELL, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

*Stratigraphy: Regional and Tectonic.*—A very large number of important stratigraphical memoirs have been produced during the year. We may notice first the extremely useful reference work published by the United States Geological Survey, and written by Miss M. Grace Wilmarth, on "The Geologic Time Classification of the U.S. Geological Survey compared with Other Classifications, accompanied by the Original Definitions of Era, Period, and Epoch Terms" (*Bull.* 769, 1925, pp. 138). The original definitions and references to the era, period, and epoch terms in common use are most valuable.

Welsh stratigraphy is, as usual, very much to the fore in the *Quarterly Journal of the Geological Society*. Dr. Ethel Woods and Miss M. C. Crosfield describe the Silurian rocks of the central part of the Clywdian range in Denbighshire. The Lower Ludlow rocks are greatly compressed and twisted, producing a series of anticlines and synclines crossing the area in mainly N.W. to S.E. directions (*Q.J.G.S.*, 81, pt. 2, 1925, pp. 170-94).

Prof. O. T. Jones has published the first part of his work on the geology of the Llandovery District, which deals with the southern part of that area (*Ibid.*, pt. 3, pp. 344-88). At the same time G. Andrew has described the Llandovery rocks of Garth (Breconshire) (*Ibid.*, pp. 389-406); and the two authors in a further paper have jointly explained the relations of the rocks in the two above-mentioned areas (*Ibid.*, pp. 407-16). It is only possible here to quote the final conclusion, that the faunal and lithological contrasts between the shelly facies of Llandovery and Garth, and the graptolitic facies of the Rhayader district, prove that, during the Llandovery epoch, differential movements were in progress along lines trending roughly parallel to the present strike. The new investigations show that during the same period other movements occurred along axes lying transversely to that strike.

The interesting igneous massif of Rhobell Fawr (Merioneth) has been comprehensively described and mapped by Dr. A. K. Wells (*Ibid.*, 81, pt. 4, 1925, pp. 463-538). The succession ranges from the Lingula Flags to the Bala Mudstones. The region differs from others bordering the Harlech Dome in the occurrence of an igneous cycle above the Tremadoc, but below what is, in adjacent areas, the local base of the Ordovician. The basal wreck of a great subaerial volcano of this age constitutes Rhobell Fawr itself. The lavas are andesitic, and a diorite-porphyry hypabyssal phase is injected into the underlying rocks. The main volcanic group (Lower Ordovician) is

largely pyroclastic, but contains pillow-form lavas of basalt and spilite. These volcanic rocks are separated from an Upper Acid Volcanic Series by a mass of unfossiliferous slates. Basic sills, doleritic to spilitic, are intruded into various horizons between the Ffestiniog Beds and the Upper Acid Group. The paper is full of excellent stratigraphical, structural, and petrographic detail, to which it is impossible to do justice in a short notice.

Another famous Welsh mountain area, Cader Idris, is dealt with in a valuable paper by Prof. A. H. Cox (*Q.J.G.S.*, 81, pt. 4, 1925, pp. 539-94). Cader Idris itself shows a sequence of Ordovician rocks from Arenig to Early Bala, in which there are four different groups of volcanic rocks, separated by considerable thicknesses of slate. Each of these groups contains intercalated bands of slate and bedded ash, proving a submarine locus for the volcanoes. The lowest volcanic rocks of the mountain are rhyolites of Arenig age. The second group consists of a thick series of pillow-lavas, ash-beds, and basic intrusions, apparently replacing the Upper Llanvirn, and a great part of the Llandeilo Series. The third volcanic group also consists of pillow-lavas, which pass laterally into agglomerate and ash; and the fourth group is built of 500-1,500 feet of rhyolites and acid andesites, with the corresponding tuffs. This series is probably of early Bala age. The main structures of the district have a N.E. to S.W. trend, but there is also a minor system of N. to S. folds which have a marked effect on the outcrops of the strata.

In an important article on the tectonics of the Southern Midlands, Dr. R. H. Rastall (*Geol. Mag.*, 62, 1925, pp. 193-222) endeavours to show that certain structural features of the Mesozoic rocks of the southern Midlands of England can be attributed to the recurrence of activity along old-established fold-lines of earlier date. It will only be possible to give here his main conclusion: "The general structure of the Midlands is due to the superposition of a fan-like virgation of the Pennine axis on a pre-existing series of folds with N.W. to S.E. (Charnoid) trend, the whole being limited on the west by the outer margin of the Caledonian fold system, and on the south by the outer margin of the Armorican system, while on the east the relations are unknown." Mr. Beeby Thompson (*Ibid.*, 410-15) offers some criticisms of, and additions to, Dr. Rastall's theory, in so far as it deals with evidence derived from Northamptonshire.

The principal thesis of Dr. R. L. Sherlock's most interesting paper on the Correlation of the British Permo-Triassic Rocks, Part 1, North England, Scotland, and Ireland (*Proc. Geol. Assoc.*, 37, 1926, pp. 1-72), is that the terms Bunter, Keuper, and Permian, as used in this country, are really lithological

in their application, and do not represent actual time divisions. The Permo-Triassic rests with a great unconformity on older rocks, and has a very uneven base. Hence the usual subdivision of the formation, starting from the base, leads to fallacious results, although certain lithological types do tend to occur in regular sequences. It is contended that subdivision from above downward gives a truer time scheme, with the Rhætic forming the uppermost datum-plane, and certain beds of salt, gypsum, and anhydrite, other datum-planes at lower horizons. It is shown that "Bunter" strata in one area are the time equivalents of "Permian" in another. The rather drastic conclusion is drawn from the available evidence that no true Permian system exists in the area dealt with. Other topics treated are the modes of deposition of the strata, and the geography of Permo-Triassic times within the British region.

The Liassic rocks of the Radstock district of Somersetshire are described by J. W. Tutcher and Dr. A. E. Trueman (*Q.J.G.S.*, **81**, pt. 4, 1925, pp. 595-666). The condensed development of certain Liassic zones, and the numerous non-sequences arising from penecontemporaneous erosion, are due to the proximity of the mobile Mendip axis of earth movement. The non-sequences are marked in several cases by beds of phosphatised nodules with derived fossils.

Numerous valuable papers on English local geology continue to be published in the *Proceedings of the Geologists' Association*. It is only possible here to list the most important of these: B. Smith and others, on the geology of the Whitehaven district (*Proc. Geol. Assoc.*, **36**, 1925, pp. 32-75); L. Richardson, on the excursion to the Cirencester district (*Ibid.*, pp. 80-98); C. T. Trechmann, on the Permian formation of Durham (*Ibid.*, pp. 135-45); J. J. Hartley, on the succession and structure of the Borrowdale volcanic series in the area between Grasmere, Windermere, and Conistone (*Ibid.*, pp. 203-26); H. Dewey and others, on the geology of the Canterbury district (*Ibid.*, pp. 257-90); A. J. Bull and H. B. Milner, on the geology of the Eastbourne-Hastings coast-line (*Ibid.*, pp. 291-320); Prof. W. W. Watts and others, on the geology of South Shropshire (*Ibid.*, pp. 322-405); G. S. Sweeting, on the geology of the country around Crowhurst, Sussex (*Ibid.*, pp. 406-18).

Mr. E. B. Bailey continues his studies of Highland stratigraphy and structure in a paper entitled "Perthshire Tectonics: Loch Tummel, Blair Atholl, and Glen Shee" (*Trans. Roy. Soc. Edinburgh*, **53**, pt. 3, 1925, pp. 671-98). He states that many of the structural features of the district may be summarised as follows: The Perthshire Quartzite Series is arranged in three major recumbent limbs; Cairnwell limb (top), Tummel limb

(middle), and Ben-y-Gloe limb (bottom). The two upper limbs are separated by a broken core of Ben Eagach and Ben Lawers schists; and the two lower limbs by another broken core consisting of Blair Atholl Series. The paper is illustrated by a beautiful coloured geological map.

In pursuance of their studies on the geology of the Outer Hebrides, Prof. T. J. Jehu and Mr. R. M. Craig have now described North Uist and Benbecula (*Trans. Roy. Soc. Edin.*, 54, pt. 2, 1926, pp. 467-89). The latter island, and the eastern half of North Uist, present an unusual type of scenery, an intricate interpenetration of land only a few feet above sea-level by ramifying water channels. This is probably a "drowning" effect, as these islands are undergoing subsidence. As in South Uist and Barra the bed rocks are Archæan gneisses of various types, intersected on the eastern side by a N. to S. zone of crushed gneisses, mylonites, and flinty-crush phenomena.

The new *Handbook to the Geology of Ireland* (London: T. Murby & Co., 1924, pp. 82) forms the most comprehensive yet concise account of Irish geology extant. It is based on Prof. Cole's contributions to that part of the *Handbuch der Regionalen Geologie* which dealt with Great Britain. Mr. T. Hallissy is the joint author and editor. (For a review see SCIENCE PROGRESS, October 1925, p. 356.)

Prof. C. F. Kolderup has now described another of the scattered Devonian areas of Western Norway (Haasteinens Devonfelt, *Bergens Mus. Aarb.*, 1923-4, *Naturvid. Raekke*, 11, 32 pp.). The Haasteinen field has an area of 37 square kms., and consists chiefly of breccias, conglomerates, and subordinate sandstones, which fill up a valley-shaped basin in a floor composed of Cambro-Silurian schists and a Caledonian intrusion of mangerite-syenite. The region is mainly bounded by faults which make it difficult to recognise the original limits and extent of the formation. The paper is notable, as was its predecessor on the Kvamshesten region, for excellent chemical analyses of sandstones of Old Red Sandstone type.

The most important point in Dr. M. A. Peacock's study of the geology of the island of Videy, near Reykjavik, Iceland (*Trans. Roy. Soc. Edin.*, 54, pt. 2, 1926, pp. 441-65) is the new subglacial extrusion hypothesis which he has put forward to explain the Palagonite Series of that island. He concludes that this series, a downward succession of palagonite-breccia, with irregular intercalations of palagonite-rock, globular basalts, and fine-grained shattered basalts, was formed in Early Glacial times by a series of localised intrusions under the ice-sheet which then covered the country. In Videy the palagonite series was extensively faulted, and a composite

sill of basaltic composition was injected into it. Then followed a period of erosion, after which Videy was overrun in Late Glacial times by a massive sheet of markedly undersaturated, grey basalt, immense flows of which cover the adjacent mainland. A further advance of the ice-sheet in a north-westerly direction, which caused the glaciation of the grey basalt, was the final event ; and during this advance Videy was sufficiently depressed to separate it from the mainland.

Prof. O. Høltedahl has described in detail some remarkable homologies in geological structure between Spitsbergen and Great Britain, and between Europe and North America (*Norske Vidensk.-Akad. Oslo*, 1, *Math.-Nat. Kl.*, 1925, No. 4, 20 pp.). The resemblance between Spitsbergen and Great Britain has been noted before, and it is an indication of the similar geological history of the two regions, especially of the areas which form parts of the great Caledonian geosyncline.

In a further paper on the geology of North Greenland, Lauge Koch (*Amer. Journ. Sci.*, 9, 1925, pp. 271-85) gives the results of his later journeys, and defines more exactly than before the areas covered by four great structural elements ; the Archæozoic Gneiss plain ; the plain of undeformed Palæozoic sediments ; the area of the Caledonides Mountains ; and a down-faulted area at Cape York consisting of Archæozoic Gneiss and Algonkian sediments. The last-named unit is here described for the first time. The period of the faulting is apparently Mesozoic.

Prof. O. T. Jones contributes a valuable paper on the Ordovician-Silurian boundary in Britain and North America (*Journ. Geol.*, 33, 1925, pp. 371-88), in which he explains that difficulties experienced in correlating strata near this boundary within the two regions arise partly from imperfect description of the British Llandovery fauna. British Ordovician and Silurian rocks occur in three lithological facies : pelitic, psammitic, and calcitic. The better-known American Ordovician and Silurian rocks mainly belong to the calcitic facies. A comparison is made between the genera of brachiopods and trilobites recorded from the British and American rocks near the Ordovician-Silurian boundary. As a result the various local formations in Anticosti are correlated with their respective English equivalents ; the Richmond of the interior states is unmistakably Bala ; and the Clinton (probably also the Medina) is Upper Llandovery. Evidence that Lower Llandovery rocks occur on the American continent is wanting.

Prof. Jones has also studied the famous section of Silurian rocks near Arisaig, Nova Scotia (*Amer. Journ. Sci.*, 11, 1926, pp. 119-25), in which he was fortunate enough to discover a graptolite fauna permitting a very precise correlation of

these strata with the basal part of the Upper Llandovery of Britain.

Five important memoirs dealing with correlation problems in the Pre-Cambrian of the Canadian Shield have recently appeared. The term Coutchiching was proposed by Lawson for a series of mica schists which he believed to underlie the Keewatin lavas of the Rainy Lake region in Northern Minnesota and the adjacent parts of Ontario. In a new study of the Coutchiching problem (*Bull. Amer. Geol. Soc.*, **36**, 1925, pp. 351-64) Prof. F. F. Grout proposes the abolition of the term on the ground that the Knife Lake mica schists, which are considered for good reasons to be of Huronian age, can be traced across an intervening granite into the type area of the 'original Coutchiching. In the discussion of this paper Prof. A. C. Lawson reiterated his view that a great schist formation, to which the term Coutchiching could properly be applied, lies below the Keewatin, although he may have included some areas of younger rocks within the original limits of the formation.

While Prof. Grout wishes to abolish the Coutchiching formation as a sedimentary series underlying the "oldest known rock," the Keewatin lavas, G. W. Bain (*Journ. Geol.*, **33**, 1925, pp. 728-43) believes that he has found pre-Keewatin sediments in the Upper Harricana basin of Quebec. The Keewatin here consists of pillow-lavas, rhyolites, and tuffs, and the basal flow overlies and contains pebbles of an older sedimentary series of carbonaceous shale, cross-bedded sandstone, silicated limestone, quartzite, and greywacke-gneiss, reaching from 4,000 to 6,000 feet in thickness. These sediments have been intruded by two granites, the earlier of which is cut by the dykes which acted as feeders to the Keewatin lavas. The second granite cuts an outlier of the Cobalt Series. The ancient sediments were folded, intruded by granite, and eroded, before the Keewatin volcanism broke out on the eroded surface.

A new memoir by W. H. Collins on the North Shore of Lake Huron (*Canada, Geol. Surv., Mem.* **143**, 1925, 160 pp.) deals with the resurvey of the original Huronian area of Logan and Murray, and with the correlation of its rocks with those of the well-known Sudbury district. The rock formations are grouped into four main structural elements, each quite distinct in age and characters: 1, a pre-Huronian basement; 2, a thick succession of Huronian sediments and associated igneous rocks resting with profound angular unconformity upon the preceding rocks; 3, Palæozoic sediments, resting with a great unconformity upon the Huronian; 4, unconsolidated Pleistocene glacial and lacustrine beds. The pre-Huronian consists of a mainly volcanic schist-complex with a sedimentary series (Sudbury), involved in great batholiths of granite-gneiss. The

whole was eroded deeply before the succeeding Huronian was deposited; and the latter formation, while little disturbed in some areas, was, in others, intensely folded and faulted during a period of diastrophism in late Pre-Cambrian times, within which also the huge Killarnean granite batholiths were intruded. A new point in this study is the relegation of the Sudbury formation (with some uncertainty) to the pre-Huronian.

T. T. Quirke (*Amer. Journ. Sci.*, **11**, 1926, pp. 165-73), in continuing his work on the relations between the Huronian and Grenville formations, brings forward a considerable amount of evidence to show that some of the sedimentary schists and paragneisses in south-eastern Ontario between Killarney and Parry Sound, and commonly called the Grenville Series, are really of Huronian age; and that much or all of the associated granite is younger than the Laurentian of the International Committee. This granite cuts the Cobalt Series, and the quartz-dolerite dykes of the Keweenawian, and is in turn cut by the later olivine-dolerite dykes. The correlation of these so-called Grenville rocks with the Huronian lessens the gap between the western and eastern pre-Cambrian sediments in Ontario, and future work may eliminate the gap altogether.

On the other hand, M. E. Wilson, in a paper on the Grenville Pre-Cambrian subprovince (*Journ. Geol.*, **33**, 1925, pp. 389-407), criticises the correlation of Huronian and Grenville as premature. The bedrock formations of the Grenville subprovince consist of three groups: 1, a group of marine sediments with local volcanic rocks, the Grenville-Hastings Series; 2, a related series of igneous intrusives, peridotite, gabbro, diorite, shonkinite, etc., the Buckingham Series; 3, batholithic masses of syenite and granite. Wilson compares this Grenville subprovince with that of Temiskaming, in which the Huronian is well developed, and shows that, with the exception of group 3, the formations of the two regions are almost entirely different, and for this reason cannot be directly and definitely correlated.

T. A. du Rietz (*Geol. Fören. Stockholms Förh.* **47**, 1925, pp. 250-60) has studied the deformation of the Pre-Cambrian peneplain of North America in the same way as G. de Geer for that of Fennoscandia (see SCIENCE PROGRESS, April 1925, p. 569). His results, which are too detailed to be sketched here, are summarised in a contoured map.

Papers by W. N. Benson on the Structural Features of the Margin of Australasia (*Trans. and Proc. N.Z. Inst.*, **55**, 1924, pp. 99-137), and by W. H. Bryan on Earth Movements in Queensland (*Proc. Roy. Soc. Queensland*, **37**, No. 1, 1925, pp. 1-82) are notable contributions to the study of tectonic and stratigraphical problems in the Australasian continent. Mr. E. R. Stanley has given a useful summary of the geology

of Papua in a publication by the Australian Government (Melbourne: Government Printer, 1925, 56 pp.). Prof. Grabau's monumental Stratigraphy of China, of which Part 1, dealing with Palæozoic and Older Rocks, has appeared (Pekin: Geol. Surv. China, 1923-4, 528 pp.), can be only mentioned in this place.

J. A. Stansfield contributes an excellent short general summary of the geology of Madagascar (*Amer. Journ. Sci.*, **10**, 1925, pp. 1-11), based on a journey through part of the island, and on the detailed work of Lacroix, Lemoine, and other French workers.

In his interesting Presidential Address to the Geological Section of the South African Association for the Advancement of Science, Johannesburg, 1924 (*South African Journ. Sci.*, **21**, 1924, pp. 52-78), Dr. A. L. du Toit deals with the contribution of South Africa to the principles of geology. Among the topics treated are mineralogy and petrology, the kimberlite of the diamond pipes, the alkaline rocks, magmatic ores, the Bushveldt igneous complex, metamorphism, ancient glaciations, Gondwanaland, and the Taylor-Wegener hypothesis of continental disruption, as applied to South African stratigraphical and tectonic problems.

The great memoir by Dr. A. L. Hall and Prof. G. A. Molengraaff entitled, "The Vredefort Mountain Land in the Southern Transvaal and the Northern Orange Free State" (*Verh. d. Kon. Akad. v. Wetens. Amsterdam* (2nd Ser.), **24**, No. 3, 1925, pp. 1-183) is equally remarkable both from the petrological and tectonic sides. It describes in the most minute detail the large Vredefort granite boss of Archæan age, which is surrounded by a ring six miles wide of overturned and metamorphosed rocks belonging to the Witwatersrand System. Epidioritised basic intrusions occur both within the granite border and in the circumferential sediments. There are also several small bosses of alkali-granite, and dykes of nepheline-syenite, cutting the higher portion of the same sedimentary series. The Vredefort granite and the enclosing sediments up to and including the Ventersdorp System are cut irregularly by innumerable veins of pseudo-tachylyte, the result of intense compression, trituration, and local fusion. The culmination of this action is believed to have produced a great ring-dyke of enstatite-granophyre, which is therefore regarded as a completely fused pseudo-tachylyte as capable of injection and crystallisation as a true igneous magma.

A zone of maximum thickness  $6\frac{1}{2}$  kms., distributed eccentrically around the granite, exhibits intense metamorphism of both regional and local type. The regional component is the earlier, and is believed to be due to the load of superincumbent

strata, combined with orogenic forces developed during the updoming of the granite. The local component is mainly thermal, and is not due to the Vredefort granite, which is indubitably older than the surrounding sediments, but to an immense concealed intrusion, the only surface manifestations of which are the small bosses and dykes of alkali-granite and nepheline-syenite. The updoming of the central granite and its sedimentary cover is believed to have been due to centripetal pressure. The ensuing relief of load led to the uprise of the younger magma. Analogies are drawn with the Black Hills (Dakota) and the Ries cauldron in Bavaria. In an as yet unpublished paper Mr. E. B. Bailey has called attention to the similarity of the phenomena of the North Arran granite to those attending the updoming and intrusion at Vredefort.

Dr. F. Dixey has given an excellent account of the geology of Sierra Leone (*Q.J.G.S.*, **81**, pt. 2, 1925, pp. 195-222). About half the area of the Protectorate is occupied by a series of granites and granite-gneisses, which contain a group of charnockitic rocks analogous to those of the Ivory Coast. Of the remaining area, half is made up of a group of ancient schists and gneisses, whereas the other half consists of a series of very old sediments termed the Rokell River Series. A great series of basic igneous rocks, including the norite complex which builds up the Sierra Leone peninsula, occur in this region.

#### **PREHISTORIC ARCHÆOLOGY.** By J. REID MOIR, F.G.S., F.R.A.I.

ONE of the most important discoveries relating to ancient man has been recently made public in England in a lay journal, *The Illustrated London News* (October 31, November 7, 14, and 21, 1925). In view, however, of the great interest of the series of articles published by this paper, and the fact that they have been written by Sir Arthur Keith, F.R.S., Prof. D. K. Absolon, and Mr. M. C. Burkitt, it is right that they should be commented upon here.

The place where these discoveries have been made is in the neighbourhood of Predmost, in the old province of Moravia, now included in Czecho-Slovakia, while the discoverer of many of the newly found relics of prehistoric times is Prof. D. K. Absolon, the Curator of the Government Museum in Brunn. For many years past Predmost has been famous for its remains of early man, which occur buried in the loess so prevalent in that region. In places this deposit attains a thickness of 65 feet, as at the foot of a limestone cliff near the village. This cliff faces southward, and is upwards of 100 feet in height, and in the loess at its base have been found abun-

dant remains of Aurignacian Upper Palæolithic Man. Fortunately, the loess at this place is being excavated for brick-making, and it was in these commercial diggings that the recent discoveries were made. The relics have come to light not only at Predmost, but in other adjacent places, and the remarkable character of many of the specimens recovered constitutes nothing less than an archæological revelation. At one site, buried deeply in loess, was found what is described as a family tomb containing twenty human skeletons. The grave was of oval, or boat-like form, measuring 13 feet long by  $7\frac{1}{2}$  feet wide. One side was composed of a palisade of the shoulder-blades of the mammoth, set upright, while the opposite side consisted of numerous lower jaws of the same animal, the whole being covered by a layer of stones 16 inches in thickness, as a protection against disturbance by wolves and hyænas.

Sir Arthur Keith has made an examination of accurate casts of certain of the skulls found—those of a man and of a woman—and concludes that the people represented were large-headed and big-brained, and of true European type. The heads were long, or dolichocephalic, with prominent eyebrow ridges, while the female skull examined had a brain capacity of 1,520 c.c., or more than 200 c.c. above the average Englishwoman of to-day. The tibiæ of this race, which shows certain primitive characters, exhibit a marked side-to-side flattening, or platycnemia. The implements, weapons, and ornaments, made in bone and ivory, are very numerous and perfect, and their presence points to some condition in the loess at this place favourable to the preservation of such objects. From the large number of these relics found, it is clear that their production was a marked feature of the Aurignacian culture of Moravia, as is borne out by the discovery, at one site, of thirteen pieces of mammoth-tusk, representing evidently the raw material of some prehistoric carver of ivory. One of the most remarkable objects found is a large mass of the cancellous portion of a mammoth's thigh-bone, which had been deliberately shaped to represent a human face. There has also been discovered an animal's head carved from reindeer-horn, and several statuettes, of supposedly pregnant women, shaped from metacarpal bones of the mammoth. Other unique finds consist of two-pronged forks made from mammoth ivory, and a spade, or very large spoon, in the same material; necklaces composed of ivory beads, and perforated teeth of bears, lions, Arctic fox, and hyænas, have been found, together with daggers made from the fibulæ of lions, ivory assegais, cylinders and balls, probably used as lassoes, formed from mammoth-tusk, a club made from a

bone of elephant ; spectacle-like buckles, with two perforations, made for fastening clothes, and well-formed needles of reindeer horn.

Several ornamented spear-heads in mammoth ivory have been discovered, also an ivory " powder-box " for toilet purposes, with red colouring matter inside, while several mammoth-ribs, with a perforation at one end, occur, associated with pieces of polished ivory, shaped like a Neolithic axe, which evidently fitted into the perforation, and were used as tomahawks. In one of the excavations a heap composed of twelve skulls of the wolf, broken open for the extraction of the brains, was uncovered, while, at another, no less than fifty molar teeth of the mammoth were recovered. It is estimated that altogether the remains of a thousand mammoths, comprising animals of all ages, have been found in the loess near Predmost.

Prof. Absolon estimates, further, that 40,000 flint implements and flakes have come to light in his researches. These specimens, according to Mr. Burkitt, are of Middle and Upper Aurignacian types, represented by scrapers, gravers, notched blades, saws, and rare laurel-leaf blades. With these were found some forms not hitherto discovered with the Aurignacian, Solutrean, or Magdalenian civilisations in France, such as stone discs, flat on one surface and convex on the other, and a circular stone shaped like a modern quoit, with a central perforation. This specimen, which resembles closely a Neolithic mace-head, and also the bone tomahawks already mentioned, are of great significance as demonstrating that these aggressively Neolithic types of weapons were evidently in use in remote pre-Neolithic times. It is supposed that the Aurignacian man of Predmost had discovered how to boil water in vessels, and that he used the stomachs of newly killed animals for this purpose. This may well be the case, but it is also possible, in view of the fact that he was modelling human and animal figures in clay, that he was acquainted with the use of pottery, and it will be of much interest to see if such evidence comes to light. In any case, the discoveries that have been made in Moravia present Aurignacian man in a much higher state of civilisation than has been before supposed, and show, in a very vivid and intimate manner, the type of people, and their customs, inhabiting Central Europe perhaps 30,000 years ago.

**BOTANY.** By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

*Ecology.*—In the *Journal of Ecology* for February F. M. Haines furnishes details of a soil survey of a Surrey heath that is an

important contribution to our knowledge of this type of vegetation. The area investigated was on the Hythe beds and the soils in general exhibited a high acidity. In the Calluna-Ulex nanus-Erica association the average pH was 3.4 (range 2.3-3.9) at 2 inches and at 9 inches was 3.9 (range 3.1-4.7). By evaluating the ratio of salts to humus in a dry and a wet season and also for the ridges and valleys, the extent of leaching was gauged, and it was found that the valleys in a wet season gain at the expense of the crests. The effect of fires is to reduce the organic content by about 60 per cent. in the younger phases and by 30 per cent. in the older, accompanying which there is a reduction in acidity. Burning also impairs the colloidal properties of the soil, increases its percolability and rate of leaching.

In the same journal Haasen describes a method of determining the water-retaining capacity of soils by the aid of sugar solutions of known concentration whose vapour pressure is balanced against that of the soils tested.

Tansley and Adamson, in an account of the chalk grasslands of Sussex, give useful lists of species, of which the most characteristic are *Phyteuma orbiculare*, *Senecio campestris*, *Aceras anthropophora*, and *Thesium linophyllum*, all practically confined to this community, but of local occurrence. Species generally present and only slightly less characteristic are *Poterium sanguisorba*, *Scabiosa columbaria*, *Asperula cynanchica*, *Bromus erectus*, *Hippocrepis comosa*, *Filipendula hexapetala*, *Anthyllis vulneraria*, and *Brachypodium pinnatum*. Equally characteristic but less general are *Campanula glomerata*, *Ophrys apifera*, and *Orchis pyramidalis*. The entire flora listed comprises 6 chamæphytes (5 per cent.), 91 hemicryptophytes (72 per cent.), 13 geophytes (10 per cent.), and 16 therophytes (13 per cent.) (*Jour. Ecol.*, Feb. 1926).

Experiments carried out by Clum (*Amer. Jour. Bot.*, vol. 13, pp. 194 and 218) indicate that, contrary to the generally accepted notion, the cooling effect of transpiration is comparatively slight and does not amount to more than about 2° to 3° C. The internal temperature of leaves exhibits considerable and abrupt changes, accompanying the changes in illumination, and in one instance a difference of 13.4° C. was recorded in the space of thirty-five seconds. The cooling effect of transpiration can therefore have but little value as a protection against overheating.

An investigation by Barrington Moore on the germination and survival of various coniferous trees suggests that increased water-supply has little effect in ameliorating the adverse influence of deep shade. The low percentage germination on raw humus, whether of Coniferous or Dicotyledonous origin, is

attributed to lack of moisture and the mechanical difficulties of penetration. If the seeds be covered with humus, the germination and survival are improved, but under all conditions growth was much better on mineral soil (*Ecology*, April 1926).

The first part of *Die Pflanzenareale* which has just appeared contains maps showing the distribution of species belonging to the genera *Saxifraga*, *Acer*, *Casuarina*, *Soldanella*, *Pinus*, and *Genista*. The map for *Genista anglica* well exemplifies the Atlantic type of distribution, though exhibiting outlying stations in North Africa and southern Italy. *Pinus pinea* well illustrates the Mediterranean type of distribution, and the indication of the regions where this species is cultivated in the open extends its area into France and South-west Britain, recalling the distribution of more tolerant Mediterranean types.

The two maps showing the distribution of eleven species of *Soldanella* and the three devoted to twenty-four species of *Saxifraga* belonging to the section *Hirculus* both exemplify the peripheral occurrence of restricted species closely related to a wide ranging congener, viz. *Soldanella alpina* in the one case and *Saxifraga hirculus* in the other. The discontinuity of the sections of *Acer*, assuming the reliability of the fossil evidence, can be related to the extensive distribution of the Maple in tertiary times. In contrast to these the genus *Casuarina* exhibits some thirty-four species centred in the Australasian area, whilst *Casuarina equisetifolia* has a much more extended range from the East African coast to the Low Archipelago.

A study by I. Krassovsky of the functions of the two types of roots in cereals seems to indicate that the seminal roots are mainly concerned in the supply of the main stem and upon these depends the yield of grain, whereas the nodal roots supply the tillers and upon them depends the yield of straw. Further, the water absorption of the seminal roots per unit of dry weight is nearly double that of the nodal roots (*Soil Science*, April 1926).

An intensive study of soil reaction in relation to the vegetation in the Sylene National Park, Norway, has been carried out by Christopherson (*Trans. Connecticut Acad.*, Dec. 1925), who finds that each plant association has a definite and relatively narrow range. The most alkaline conditions, viz. pH 7.1, were found in alpine thicket dominated by the calciphile *Salix lanata*, whilst the most acid were encountered in *Betula nana*-*Vaccinium* thicket and dwarf shrub heath with *Betula nana*, etc. The forest communities dominated by *Betula pubescens* exhibited a range from 3.7 to 4.5 where the undergrowth was heathy in character, whilst where the undergrowth

was rich in herbaceous species the range was from pH 5.5 to pH. 6.9. Correlation was observed between the presence of "pan" and high hydrogen-ion concentration.

An intensive study of the humus layer in coniferous woods has just been published by Hesselman (*Meddl. fran Statens Skogsforsoksanstalt*, pp. 169-552, H. 22, No. 5, 1926). The reaction of the undecomposed leaves ranged from pH 3.8 in the case of spruce needles to pH. 7.4 for *Mercurialis perennis*. Characteristic differences in reaction are shown by the foliage of various trees; thus needles of *Pinus* gave pH 4-4.2, *Quercus robur* 4.8-4.9, *Betula* spp. 5-6.1, and *Fagus sylvatica* 5.3-6.6. There is a close negative correlation between the degree of acidity and the amount of calcium present. The latter (as CaO) varied in the plants studied from 5.23 per cent. in *Mercurialis perennis* to 0.16 per cent. in *Deschampsia flexuosa*. The buffer action of the raw humus varies considerably, and five types are recognised, *viz.* (1) highly acid and poor in basic buffer substances, *e.g.* *Calluna*, fir needles; (2) moderately acid and moderately rich in basic buffer substances (most broad-leaved trees); (3) very low acidity and high buffer content (*e.g.* *Corylus avellana*, *Stachys sylvatica*); (4) high acidity and high buffer content (*e.g.* *Quercus*, *Larix*); (5) very low content of buffer substances (*Deschampsia flexuosa*). As might be expected, the nitrate content of the humus decreased as the acidity increased, and whereas ammonification attained its maximum between pH 4.5 and 5, nitrification was at its maximum in soils of pH 6 or with artificial inoculation pH 5.5. Experiments with soils of varying calcium content showed a maximum nitrification when between 2 per cent. and 2.5 per cent. of lime was present. Various factors are shown to modify the effect of reaction on nitrification.

*Morphology.*—The numerous carpels present in the flowers of such Monocotyledons as *Echinodorus* and *Alisma* and the numerous stamens of *Sagittaria* exhibit a meristic variation curve of a markedly periodic character with primary and secondary modes corresponding to multiples of three (Salisbury, *Ann. Bot.*, April 1926). The similarity of these variation curves to those of Ranunculaceous flowers previously investigated (Salisbury, *Ann. Bot.*, 1919 and 1920) affords striking support for the contention that the Ranunculaceous flower is essentially trimerous. The many characteristics common to the Ranales and Helobiales and the peculiar characters exhibited by certain members of both groups warrant the recognition of a closer relationship between them, but the trimerous floral organisation which the Ranales share with the Monocotyledons as a whole and not a few Dicotyledonous families is held to be a primitive character of the angiosperms

in which the meristem is regarded as the multicellular equivalent of a three-sided apical cell.

In the same journal Willis emphasises the uniformity of the conditions under which the members of the Tristichaceæ and Podostemaceæ live, all being aquatics of swiftly running, well-aerated tropical streams. Despite this uniformity of the external conditions, they present an extraordinary diversity of organisation which the author considers must be the result of internal factors and mutations of considerable magnitude. Dr. Willis points out that these groups show "a much greater degree of difference than any other family of flowering plants," and one may question whether this may not be due to a polyphyletic origin, hence the difference of internal factors which the author hypothecates.

*Cytology and Genetics.*—Zirkle, from a study of the chloroplasts in the higher plants, has come to the conclusion that typically this consists of a hollow colloidal protein stroma, in which the pigments are evenly distributed, surrounding a central vacuole containing a watery solution of proteins and sugars and sometimes starch grains. This stroma is traversed by radial pores extending from the vacuole to a cytoplasmic sheath which envelops the chloroplast. There appears to be no evidence for the existence of a limiting osmotic membrane, the swelling of chloroplasts in water being attributed to imbibition (*Amer. Jour. Bot.*, June 1926).

In the same journal Gaines and Aase describe a haploid wheat plant (*Triticum compactum humboldtii*) with twenty-one somatic chromosomes in place of forty-two and which was only distinguishable from the diploid plant at the time of flowering by the form and sterility of the inflorescence. Similar haploid plants have been recorded for *Nicotiana tabacum* and *Datura stramonium*. It appears from breeding experiments carried out by Gairdner (*Jour. Genetics*, April 1926) that the robust horticultural variety of *Campanula persicifolia* known as "Telham Beauty" is self-fertile in contrast to the sterility of the common variety. Telham Beauty is a tetraploid plant with thirty-two somatic chromosomes, whereas the type is a diploid with sixteen.

Further work on the genetics of the cabbage has been carried out by Pease (*Jour. Genetics*, April 1926). The formation of "heart" by the terminal and lateral buds respectively is the outcome of independent Mendelian characters. The absence of two factors are regarded as determining the development of "heart"; and when both of these factors are present, no heart is formed and the kale type results. When one or other alone is present, a slight development of heart is found. One of these two factors is apparently linked with others deter-

mining respectively a narrow leaf form, an entire margin, the development of a petiole, and one of the factors responsible for the "curly" type of foliage. The other factor for "no heart" is associated with the factor for tall habit and the second factor for curly foliage.

From breeding experiments with various species of *Malva* Kristofferson concludes that the section *Fasciculatæ* should be subdivided into two subsections, viz. *Planocentræ*, including our British *M. parviflora* and *M. sylvestris*, and *Conocentræ*, including *Malva crispa* (*Hereditas*, B. VII. H<sub>3</sub>).

In the same journal (H<sub>2</sub>) Muntzing reports on the hybrids produced by crossing *Lamium amplexicaule* and *Lamium hybridum*, the offspring being quite sterile.

*Cryptogamic*.—Pavillard (*Rep. Danish Oceanographical Exped.*, vol. ii, No. 9) records 118 species of marine diatoms from the Mediterranean seas, and these include two new species of *Coscinodiscus* and the very rare *Rhisosolenia firma* from the Tyrrhenian Sea. This latter discovery is of great geographical interest, since heretofore it has only been recorded from the Indian Ocean.

A number of new species of Algæ are recorded from Beloe Lake by Zalessky which include species of *Chroococcus*, *Microcystis* (7), and *Aphanothece* (3) (*Rev. Gen. d. Bot.*, Jan. 1926). In the same number Nicolas, dealing with the biology of certain Bryophytes, emphasises the fact that *Mnium punctatum* is a moss of non-calcareous soils, and the same preference is exhibited by *Targionia hypophylla*. *Fegatella conica* is apparently indifferent as to soil type, but the production of sexual organs would seem to be correlated with the presence in its tissues of the endophytic fungus, thus affording a parallel to the case of the orchid *Gastrodia* and the fungus *Armillaria mellea*. *Fegatella conica* and *Pellia fabbroniana* are both occasionally uninfected, but this condition appears to be the rule rather than the exception in *Lunularia cruciata*, and it would be interesting to know if the shy fruiting of this liverwort is connected with this fact.

Several new and interesting species of Characeæ are described and figured by Groves and Stevens (*Trans. Roy. Soc. S.A.*, vol. xii, pt. 2), viz. three species of *Nitella* and two species of *Chara*. These authors also record *Tolypella glomerata* and *Chara corallina*, both hitherto unknown from South Africa.

**ZOOLOGY.** By R. J. LUDFORD, D Sc., Ph.D., University College, London.

*Protozoa and Radiation*.—An interesting suggestion as to the cause of the mitotic irregularities produced by radiation is made by J. C. Mottram as the outcome of experiments carried

out on the effects of  $\beta$ -radiation on the infusorian *Colpidium colpoda*. Exposure of this organism to radiation is followed by an increase in the hyaline material of the macronucleus and this accumulates between the chromatin and the nuclear membrane. There is also a derangement of the superficial mitochondria, which normally are arranged in longitudinal rows. Mottram suggests that the abnormal mitoses which are the characteristic result of radiation may be due to changes in the hyaline part of the nucleus (nucleoplasm), rather than to any direct action on the chromatin (*Jour. R.M.S.*, 1926, vol. xlv, June 1926).

*Micro-dissection and Micro-injection.*—Considerable contributions to our knowledge of the physical properties of protoplasm have followed the use of the micro-dissection and micro-injection method. In a recent paper R. Chambers and P. Reznikoff have described a series of experiments, carried out with this technique, with the view to determining the action of varying concentrations of sodium, potassium, calcium and magnesium chlorides separately, and in some cases in binary combinations, on the protoplasm of *Amœba proteus*. Observations were made on the effect of immersing, and of tearing amœbæ in the different solutions, and of injecting the solutions directly into the interior of the cell. The general result of these experiments was the demonstration of marked differences between the interior and exterior of the cell in their behaviour towards the saline solutions employed. Particularly striking is the action of calcium chloride on the amœba. Injection of this salt results in a curious "pinching-off" of the portion of the cytoplasm solidified by its action. When torn in the same solution by the micro-dissection needle a progressive coagulation usually converts the entire cell into a gel-like fragment. The toxic effects of sodium and potassium chlorides can be antagonised by calcium chloride, and this antagonism occurs at the surface of the cell ("Microsurgical Studies in Cell Physiology: 1. The action of the chlorides of Na, K, Ca, and Mg on the protoplasm of *Amœba proteus*," by R. Chambers and P. Reznikoff, *Jour. of Gen. Phys.*, 1926, vol. viii, No. 4, pp. 369-401, 2 plates and 19 text-figs.).

*Spermatogenesis.*—C. E. Walker has made a study of the behaviour of the chromosomes during meiosis in a number of mammals. Cytological observations were carried out on the testes of guinea-pigs, rats, mice, rabbits, and a monkey (*Cercopithecus* sp.). Walker interprets the series of changes as occurring as follows: "The daughter chromosomes elongate and divide longitudinally, filling the nucleus with irregularly distributed semivalent threads. During this process of unravelling of the chromosomes into semivalent threads, an

appearance very similar to that seen during synapsis is evident. These threads rejoin laterally in pairs forming univalent filaments. These filaments join laterally in pairs. They again separate, excepting at their ends, which remain joined until the first meiotic division takes place. The longitudinal split of the filament into semivalent threads reappears at the telophase, and is consummated at the second meiotic division " ("The Meiotic Phase in Certain Mammals," by C. E. Walker, *Proc. R. Soc.*, 1926, vol. 99, No. B698, pp. 366-74, with 3 plates).

Very little is known of the rôle of the chromatoid body during spermatogenesis, and practically nothing definitely about its function. Its behaviour during spermatogenesis of the Black-clawed crab (*Lophopanopeus bellus*) has been described recently by N. Fasten. During the growth period of the spermatocyte the chromatoid body appears in the cytoplasm. When the reduction division takes place it passes undivided into one of the daughter cells. There thus arise two kinds of spermatids, one with a chromatoid body, the other without this body. At an early stage in the development of the former, the chromatoid body is extruded, and sperm formation proceeds the same in all the cells. The mature spermatozoa are mostly four-or five-rayed, but three-and six-rayed types also occur. ("Spermatogenesis of the Black-clawed Crab, *Lophopanopeus bellus*" (Stimpson) Rathbun, by N. Fasten, *Biol. Bull.*, 1926, vol. 1, No. 4, 277-292, 3 plates).

*Atypical Sperms and Development.*—An investigation by O. W. Hyman into the process of fertilisation in the mollusc *Fasciolaria tulipa* has shown that while all the ova are at least partially fertilised, yet less than 3 per cent. of them undergo cleavage, and less than 1 per cent. develop into veligers. These results are attributed to the fertilising properties of typical and atypical sperms. The incomplete fertilisation in approximately 97 per cent. of the oval is attributed to the power of the atypical sperms to form a fertilisation membrane, which thus prevents subsequent fertilisation by a typical sperm. The aberrant cleavage of ova which occurs in a few cases is believed to be due to complete fertilisation by atypical sperms ("Natural Partial Fertilisation in *Fasciolaria tulipa*," by O. W. Hyman, *Jour. Morph. and Phys.*, 1925, vol. 41, No. 1, 267-81, 3 plates).

*Growth and Differentiation.*—In a paper on "Energy Production and Transformation in Protoplasm as seen through a Study of the Mechanism of Migration and Growth of Body Cells," M. T. Burrows makes a critical survey of the work that he and others have carried out on growth and migration in tissue cultures. He advances a physico-chemical explanation of

these processes, as well as an interpretation of their relationship to the cellular environment (*Am. Jour. Anat.*, 1926, vol. 37, No. 2, 280-350, with 16 figs.).

T. C. Byerly has investigated the result of suffocating chick embryos. Eggs were incubated for a short period, about twenty-four hours, they were then coated with water-glass and returned to the incubator for seventy-two hours. The most marked effect of suffocation was the formation of large sinuses. Erythroblasts were developed from the cells of the walls of the enlarged blood-vessels. As the result of the accumulation of the products of metabolism the majority of the cells die, but certain of them are able to utilise the dead cells as food and continue to live. Their respiration must be anaerobic ("Studies in Growth: I. Suffocation Effects in the Chick Embryo," by T. C. Byerly, *Anat. Rec.*, 1926, vol. 32, No. 4, 249-70, 15 figs.).

T. S. P. Strangeways and H. B. Fell have compared the development of the undifferentiated embryonic limb-bud of the chick when grafted subcutaneously into a post-embryonic chick, and when cultivated *in vitro*. The grafting experiments were carried out with limb-buds of embryos of 82 hours' incubation. They were inoculated subcutaneously into the under-surface of the wings of the post-embryonic chicks. Such grafts grew and differentiated into cartilage, bone, fibrous tissue, and epidermis, but differentiated muscle was absent. In the tissue culture experiments an improved tube technique was employed. The explants were made from the limb-buds of embryos which had been incubated 72-80 hours. Active cell division was observed, and cartilage, white fibrous tissue and epidermis became differentiated. Two principal differences between the histological composition of the growths and the normal limb were distinguishable. There was no differentiated muscle in either grafts or cultures, and bone was absent from the cultures. Furthermore, it was found that the character of the growth of the culture was dependent upon the degree of fluidity of the medium ("Experimental Studies on the Differentiation of Embryonic Tissues growing *in vivo* and *in vitro*. -1. The Development of the Undifferentiated Limb-bud (a) when Subcutaneously Grafted into the Post-embryonic Chick, and (b) when Cultivated *in vitro*," by T. S. P. Strangeways and H. B. Fell, *Proc. R.S.*, 1926, vol. 99 B, No. 698, 340-65, 9 plates).

*Photoreceptors of the Earthworm.*—Although the response of earthworms to the stimulus of light has been investigated, as well as the relative sensitivity of different regions of their bodies to the same stimulus, yet little work has been done on the nature and structure of the organs responsible for the percep-

tion of light. In a recent paper W. N. Hess has brought forward evidence to show that the *Lichtzellen* of Hesse both on structural and functional grounds are undoubtedly the photoreceptors of *Lumbricus*. These cells occur in the epidermis in regions of the body sensitive to light. Wherever sensitivity to light is greater there are more of these *Lichtzellen*, thus the epidermis of the prostomium contains the greatest number. Each photoreceptor is supplied with large nerves, which give rise to an intra-cellular network of neurofibrillæ—the retinella, that surrounds a transparent hyaline body—the lens. The retinella and lens together constitute the optic organelle. These organelles vary considerably in shape, size, and position in different cells, yet they are always nearly circular in cross-section. The lenses focus light from various directions on to the retinellæ, the fibres of which are the photosensitive structures. The photoreceptors are regarded as specialised epidermal cells; those occurring internal to the epidermis having wandered from the surface along the course of the nerves ("Photoreceptors of *Lumbricus terrestris*, with special reference to their Distribution, Structure, and Function," by W. N. Hess, *Jour. Morph. and Phys.*, 1925, **41**, 1, 63-94, 18 figs.).

*Experiments on Heredity and Length of Life of Rotifers.*—J. E. Finesinger has carried out a series of experiments with the rotifer *Lecane inermis* in an attempt to bring about heritable variations by chemical and other means. *Lecane* reproduces parthenogenetically, and thus assures a pure line hereditarily. In the experiments the effect of certain chemicals ( $\text{FeSO}_4$ ,  $\text{FeCl}_3$ ,  $\text{HCl}$  and  $\text{NaSiO}_2$ ) was investigated, also the action of ethyl alcohol and variations in temperature. The subjection of this rotifer to the various chemical environments, and to higher temperatures for a period of three months (about 25 generations) was ineffective in transmitting diversities beyond the second generation. It is suggested that length of life is influenced by toxic intestinal bacteria as suggested by Metchnikoff. This conclusion is based upon the fact that with organisms kept without food in spring water there would be comparatively less chance for intestinal poisoning from bacteria, hence the longer span of life than in the well-fed organisms kept in malted milk ("Effect of Certain Chemical and Physical Agents on Fecundity and Length of Life, and on their Inheritance in a Rotifer, *Lecane (Distyla) inermis*" (Brice), by J. E. Finesinger, *Jour. Expt. Zool.*, 1926, vol. 44, No. 1, 63-94).

*Lepidocaris rhyniensis*.—In a monograph "On a New Type of Crustacean from the Old Red Sandstone (Rhynie Chert Bed, Aberdeenshire), *Lepidocaris rhyniensis*, gen. et sp. nov." (*Phil. Trans. B.*, 1926), D. J. Scourfield describes

by aid of an excellent series of drawings the morphology of a primitive crustacean which he has reconstructed from its fossil remains. Dr. W. T. Calman points out, in an article in *Nature*, that no other fossil crustacean is known with anything approaching the completeness of the description given by Scourfield of *Lepidocaris* (*Nature*, 1926, vol. 118, No. 2,950, 89). He emphasises the fact that *Lepidocaris* is by no means a primitive crustacean, and is probably off the main line of Anostracan descent.

*The Morphology of the Vertebrate Head.* There have been published recently several important papers on the morphology of the skull. To the *Phil. Trans. of the Royal Society*, 1926, J. R. Norman has contributed an account of the development of the chondrocranium of the Eel (*Anguilla vulgaris*). This paper also contains a comparison of the more important features in the chondrocranial development in Ganoids and Teleosts (*Phil. Trans. R.S.*, 1926, vol. 214, pp. 369-464, 56 figs.). The development of the skull of the Turtle has been described by R. F. Shaner, and this is followed by a short discussion of the fossil reptile skull from the standpoint of embryology (*Anat. Rec.*, 1926, vol. 32, No. 4, 343-68). The anatomy of the head of a foetal African elephant, *Elephas africanus* (*Loxodonta africana*) is the subject of a monograph by N. B. Eales (*Trans. Royal Soc., Ed.*, vol. liv, No. 11, pp. 491-551, 12 plates). In general the upper part of the skull bears a close resemblance to that of a modern elephant, but the lower jaw reveals ancestral characters which are considered confirmatory evidence of the evolution of the modern African elephant from a long-jawed ancestor. G. R. de Beer devotes the second of his "Studies on the Vertebrate Head" to an account of the orbito-temporal region of the skull (*Q.J.M.S.*, 1926, vol. 70, part ii, pp. 263-370, 133 text-figs.). The conclusion arrived at from this study is that "the skulls of all vertebrates are built upon an almost identical plan. The original membranous covering to the brain, pachymeninx, becomes the site of chondrification and gives rise to the primitive cartilaginous cranium of which some features are preserved even in the mammal." A diagram of the schematic skull is given from which it is stated that the conditions in all forms, relations of nerves, and blood-vessels can be derived.

*Hyperthyroidism and the Thymus Gland.*—The effect of thyroid feeding on the thymus and lymphoid tissue of tadpoles has been studied by C. C. Speidel. Desiccated sheep's thyroid extract was placed in the water for the tadpoles to feed upon, and its influence determined by killing and examining the animal after various intervals of time. A general lymphoid hyperplasia was found to ensue. Lymphocytes in the thymus

were stimulated to mitotic activity. The newly formed lymphocytes passed out into the general circulation. The same thing occurred in other lymph glands. Speidel explains this as a preliminary phase of the response on the part of the organism to supply new blood-cells—erythrocytes, granulocytes, and monocytes—which in the frog are derived from lymphocytes, the need for such cells having arisen owing to the new metabolic conditions of hyperthyroidism ("Studies of Hyperthyroidism: II. The significance of changes in the Thymus Glands of Thyroid-treated Tadpoles," by C. C. Speidel, *Am. Jour. Anat.*, 1926, vol. 37, No. 1, 141-58, six figs.).

**ENTOMOLOGY.** By J. DAVIDSON, D.Sc., Rothamsted Experimental Station, Harpenden.

*General Entomology.*—In number 182 of Abderhalden's *Handbuch der biologischen Arbeitsmethoden* (Urban & Schwarzenberg, Berlin, 1925) there are several chapters dealing with methods employed in investigations on insects, the chapter on Orthoptera by F. Zacher occupying 100 pages. It should be noted that a chapter on entomological technique by A. Koch appeared in 1923 in No. 94, pp. 479-534 of this handbook. F. S. Bodenheimer (*Zeitschr. f. angewandte Entomologie*, 12, 91-122) discusses the question of the relationship of climate to insect life, the factors of temperature and humidity especially being particularly important in relation to insect development. A useful manual for the teacher and student dealing with the chief injurious insects of the U.S.A. is written by G. W. Herrick (New York, Henry Holt & Co. 1925, xviii + 458 pp.); the various insects are treated under the plants which they inhabit. Another work on insect pests is by P. Vayssière and J. Mimeur, *Les Insectes nuisables au cotonnier* (Paris, Émile Larose, 1926, ix + 175), which consists of observations on the principal insects of cotton in French African territory.

R. Heymons (*Biol. Zentrbl.*, 46, 51-62) has a paper on the egg-burster, and the mechanism of hatching from the egg in various insects. The author summarises the following types of egg-bursters met with:—an unpaired occipital and frontal, paired epicranial and thoracic, and thoracic abdominal forms, the chief structural character being a saw-like ridge of chitin, which by the appropriate movements of the emerging insect, breaks the hard chorion of the egg; in some dipterous larvæ the egg is burst by the action of the mouth parts of the embryo. h. T. Gimingham (*Trans. Entom. Soc., London*, 1925, 585-90) has a paper on the egg-burster in Aphididæ.

H. Gadeau de Kerville (*Bull. Soc. Entom. France*, 1926, 47-52), describes the results of experiments in decapitation and

grafting the head of adult insects of different orders. More than 250 adult individuals were experimented with, including nine species of Coleoptera; *Carausius morosus*, *Mantis religiosa* (Orthoptera); *Sericaria mori* (Lepidoptera); *Notonecta glauca* (Hemiptera). This experimenter found that the insects survived a variable period, up to several days, after decapitation, and in the case of *Mantis religiosa* co-ordinated movements were executed for some days, the species exhibiting great resistance to the effects of the operation, but as regards the grafting the results obtained were all negative, even where the head of certain adult beetles after being cut off was immediately replaced. Finkler (*L'Anzeiger der Akad. der wissenschaftliche Klasse*, 1921, No. 18, 157-8; 1922, Nos. 2-3, p. 13), who also carried out grafting experiments with certain decapitated insects obtained positive results in certain cases; when a male head was grafted on to a female body, and vice versa, the sexual instincts of the male body became female, and vice versa. The experiments of de Kerville failed to confirm these results, and this author also refers to the experiments of Kahn, R. H. (*Zool. Anzeig.* 1925, 75), who similarly obtained negative results.

The importance of biological methods in the control of certain insects is now widely recognised throughout the world, and several papers bearing on this subject, by several distinguished American entomologists, form a symposium on Insect Parasitism (*Jl. Econ. Entom.*, 19, 271-325).

Some recent work by Hem Singh-Pruthi (*British Jl. Exper. Biol.*, 3, 161) on metamorphosis in the blow-fly appears to disprove the theory of Bataillon (1923), who, maintaining that there was an accumulation of CO<sub>2</sub> in the blood and body cavity of silkworms undergoing metamorphosis, advanced the theory that in the metamorphosis of insects the first event is the destruction of larval organs (histolysis) and that this is brought about by the suffocation effect of the assimilated CO<sub>2</sub>.

The function of insect sense organs is in many cases little understood, although we have a fair knowledge of their structure, and a review of the morphology of insect sense organs and the sensory nervous system by R. E. Snodgrass (*Smithsonian Miscell. Collection*, 77, No. 8, 80 pp.) is a welcome contribution to the literature; the various sense organs are classified according to structure into nine classes: the true sense organs are formed from a part of the body wall and they exhibit various degrees of complexity, the simplest type being formed by a hair which is sensitised by a nerve connection at its base: with regard to the modification of structure and the stimuli which the different sense organs may perceive, it is interesting to note that the cuticular walls of certain sense organs are only half a micron

thick, which doubtless readily allows the passage of certain chemical stimuli.

B. H. Weiss (*Amer. Nat.*, **60**, 102-4) brings further evidence to show that in relation to the insect fauna of every area, regardless of its size, there appears to be a fixed set of ratios between the types of food-habits, these ratios depending upon the vegetation: if the area is large and embraces different types of vegetation, a certain definite set of ratios will prevail: if this larger unit is divided into smaller units, each with a different type of vegetation, the ratios between the types of food habits in the small units will vary in accordance with the vegetation.

*Orthoptera*.—The influence of temperature on the development of the eggs in certain grasshoppers is the subject of a paper by J. H. Bodine (*Journ. Exper. Zool.*, **42**, 91-109): it was found that the increment rate of development, within the normal limits, increased in direct proportion to the increase in temperature; exposures to low temperatures produced an acceleration effect on the subsequent development of the eggs kept in constant high temperatures; protoplasmic rhythms in susceptibility to low temperatures seem to occur in the eggs: it is possible to calculate the time of hatching if the previous temperature history is known. V. P. Pospelov (*Bull. Ent. Res.*, **16**, 363-7) describes the results of experiments on the effect of temperature on the development of *Locusta migratoria* L. Young insects reared at 35-8° C. with humidity near saturation point developed rapidly, became adult and oviposited: when the insects were removed in the 3rd instar from these conditions, and transferred to 30° C. by day and 20° C. by night, with humidity about 70 per cent., they became sluggish, ate little and those which became adult did not mature sexually. Observations on the organism *Coccobacillus acridiorum* found in the blood of this insect confirms the opinion of Mereshkovsky, that it is a normal symbiont, but under unfavourable temperature and humidity conditions it becomes a parasite. N. Ford (*Canad. Entomologist*, **58**, 66-70) has some interesting observations on the habits of *Grylloblatta campodeiformis*, the primitive orthopteran which exhibits characters indicating affinities with both the saltatorial and cursorial groups, and has a simple campodeiform body. The optimum temperature for this insect appears to be a few degrees above zero (centigrade), and individuals were kept alive for some months in vessels packed with ice: specimens were collected on the Sulphur mountain in Alberta, where they inhabit crevices near the ice, being evidently nocturnal in habit: normally they do not attack living insects, and their food is probably composed of dead insects, which perish on the approach of nightfall in the higher parts of the mountain;

copulation was observed in captivity, and lasted twelve hours, and one mated female deposited thirty to forty eggs.

*Coleoptera*.—W. T. M. Forbes (*Journ. N. York Entom. Soc.*, 34, 42–68) finds that the folding patterns of the wings of beetles prove to be developed by somewhat simple modifications from a single fundamental plan, the modifications having taken certain definite directions, each of which characterises a large series of *Coleoptera*: the results of these studies do not fit in with the present placing of many genera and families, and the author discusses his observations from this point of view, which will be continued in a later paper.

The nerve endings in insects is the subject of a paper by W. A. Hilton (*Trans. Amer. Micr. Soc.*, 44, 132–7), especially with reference to the larvæ of *Dendroides*. These larvæ are transparent, and with the aid of intra-vitam staining, the author shows that the nerve terminations on a muscle fibre show great variation: there may be two separate strands to one muscle fibre, or several branches from a single strand; there may be as many as ten motor cells supplying a muscle fibre, and fine fibrils penetrate into the interior of the fibre: the writer distinguishes (1) somatic bipolar receptive cells; (2) bipolar or tripolar visceral receptive cells about the mouth and alimentary tract; (3) somatic effector motor cells in the ganglia, whose processes form the extensive end plates on the muscle fibres; (4) the visceral effectors located in the visceral ganglia sending fibres to the muscles of the food canal.

*Lepidoptera*.—The question of colour in *Lepidoptera*, which was discussed by H. A. Baylis (*The Entomologist*, 57), showing that it is due to interference colours produced by the structure and position of their scales, is the subject of a further paper by the same author (59, 124–6), with special reference to the results obtained by F. Süffert (*Zeitschr. f. morphologie u. Okol. der Tiere*, 1, (2), 171–308). Further evidence is produced by the latter author to show that horizontal stratification does actually occur in the scales of *Urania*, several species of *Papilio* and other forms having the *Urania* type of structure, which is the cause of the colours: the evidence regarding the action of the vertical plates in the *Morpho* type of scales indicates a stratification in the plates themselves; the strata are tilted, making an angle of about 5° with the surface of the scale, thus forming almost horizontal sections of the scales; in forms such as the scales of *Chlorippe* and *Apatura* the angle between the strata and the surface of the scales is greater and varies in different species. This explains why light behaves differently according as it falls on scales of the *Morpho* type in a direction parallel or transverse to the vertical plots. It also explains cases such as *Apatura iris*, *Chlorippe laurentia* and *Euplœa* spp., in which

a brilliant colour appears only when light is falling obliquely upon the scales, parallel to their length from the fixed ends and when the scales are viewed obliquely from the same side as the source of light : as pointed out by Süffert, the scales in these cases are behaving as though their reflecting surfaces had been tilted through a considerable angle. Previous explanations which accounted for certain cases in which the scales are actually tilted or curled towards their distal ends did not satisfy in a type like *Papilio erithalion* in which the scales lie flat ; if, however, there are oblique alternating layers and columns of chitin and air in the vertical plates, the sum of their surfaces supplies exactly the tilted plane of reflection required to account for the phenomenon.

The subject of melanism in Lepidoptera is important on account of the rapidity with which the changes develop, and of the close correlation between the appearance of melanic forms and the conditions obtaining in industrial areas. An important paper by J. W. H. Harrison and F. C. Garrett (*Proc. Roy. Soc. Series B*, 99, 241-63) contains the results of experiments with *Selenia bilunaria*, *Tephrosia bistorta* and *T. crepuscularia* obtained from certain districts in which melanic forms were induced by impregnating the hawthorn leaves on which the larvæ were reared with solutions of lead nitrate and manganeous sulphate ; the melanism thus induced was found to be inherited as a simple mendelian unit character. The same authors have given a short account of these experiments in *The Entomologist*, 59, 121-3. The reader is also referred to a paper by G. T. Porritt (*Entom. Mo. Mag.*, 62, 107-11) on the subject. K. Hasebroek (*Fermentforschung herausgegeben von Abderhalden*, 8, 197-226) also has a paper on the frequency of melanism in lepidoptera in industrial areas, and considers the cause as not due to the effect of contaminated food, but to the influence of noxious vapours acting through the tracheal system ; it was found that the use of substances such as methane, sulphuretted hydrogen, pyridine and ammonia induced melanic changes in the species used. Some aspects of the results obtained by this experimenter are discussed by E. A. Cockayne (*Entom. Record*, 38, 44-5).

A masterly and stimulating treatise entitled *Biologie der Schmetterlinge* (Berlin, Julius Springer, 1926, 480 pp. Price 18 Marks), by M. Hering, is one of the *Biologischen studienbücher* series, edited by Professor W. Schoenichen. Space will not allow of a detailed review of this work, which not only gives the reader the position regarding our present knowledge on the many problems associated with the biology of the Lepidoptera, but indicates the nature of the problems which require investigation. A paper by N. Schultz (*Biochem. Zeitschr.*, 156,

124-9) deals with the metabolism of the larva of the clothes-moth, *Tinea pellionella* L.; it feeds essentially on the keratin substance of wool and hair, and selects only the wool from mixtures of wool and cotton.

*Hemiptera*.—It is well known that the offspring of an apterous parthenogenetic aphid may consist of all apteros individuals or all winged individuals or of both winged and apterous forms, and this variation in winged production has been studied by several investigators, more particularly with reference to the nutritional factor, temperature, and the genetical aspect. L. Ackermann (*Journ. Expt. Zool.*, **44**, 1-60) discusses the question from all points of view particularly with reference to the grain aphid *Rhopalosiphum prunifoliae*. As a result of investigations on the globules in the hæmolymp of this species, this author considers that the physiological considerations are important, and that wing production is dependent upon changes in the proportion or concentration of certain materials in the hæmolymp, as brought to pass by the rupture of the brown globules. S. Hoke (*Ann. Ento. Soc. America*, **19**, 13-29) has a preliminary paper on the wing venation in Heteroptera.

In the greenhouse white fly *Trialeurodes vaporariorum* there are two parthenogenetic races, an American one showing arrhenotoky, and an English one exhibiting thelytoky. M. Thomsen (*Nature*, 1926, 428) shows that in *T. vaporariorum* the mated females produce eggs which give rise to both males and females, and in Denmark both races occur: the English race is almost exclusively female producing and the author briefly discusses the cytological aspect of the problem. F. Schrader (*Ann. Appl. Biol.*, **13**, 189-96) is of the opinion that only in recent years owing to its marked increase in numbers has it been recorded as a serious pest, and the American race appears to be spreading rapidly; the explanation suggested by Williams (1917), that some conditions have caused the English race to pair more frequently, resulting in an increase in males, and favouring a stabilisation of the sexes, does not hold any longer.

K. M. Smith (*Ann. Appl. Biol.*, **13**, 109-39) has an interesting illustrated paper on the feeding methods of certain Hemiptera and the resulting effects upon plant tissues. The first volume of *The Plant Lice, or Aphididae of Great Britain*, by F. V. Theobald, has appeared (Ashford, Kent, Headley Bros., 1926, pp. ix. + 372, price 25s.). This work, when completed, will form an up-to-date treatise on the British species, and will embody the results of about twenty-six years' continued observations and collecting by the author: the present volume deals with the groups *Macrosiphina* and *Pentalonina*, consisting of

fourteen genera : descriptions and notes on the biology of the species are given.

*Hymenoptera*.—Die Ameisenmimikry ein exakten Beitrag zum Mimikryproblem und zur Theorie der Anpassung is the title of a work by E. Wasman (Berlin, 1925, xii. + 164 pp.) in which the author discusses mimicry between ants and other insects, in addition to other relationships.

*Diptera*.—Th. Borgmeier (*Entomologische Mitteilungen*, **14**, 237–9) describes a remarkable Phorid, *Zikania degenerata* sp. et gen. nov., from the nest of the ant *Solenopsis sævissima*, a single female only being found. M. R. Clare (*Biol. Bull.*, **49**, 440–60) has a paper on the oxygen metabolism in *Drosophila melanogaster*. The thoracic sclerites of the Psychodidæ discussed from the phylogenetic standpoint have been studied by C. Crampton (*Ent News*, **37**, 33–70).

The syrphid flies of the genus *Volucella* exhibit great variation in colour, which has long been a source of difficulty with the taxonomist ; in some forms parasitic in the nests of bumblebees, the colour variations apparently mimic the bee host : Verral (1901) records the mating of the two varieties of *V. bombylans*, namely, *bombylans* and *plumata*, and recently Gabritschewsky (*Zeitschr. f. indukt. Abstamm. u. Vererbungslehre* (1924), **32**, 321–53) gave an account of crosses between three varieties of this species, showing that a mendelian ratio was obtained : C. E. Kceker (*Psyche*, **32**, 22–7) has further examined the data obtained by the latter author, and shows that the relationships obtained agree very closely with ratios expected upon a mendelian interpretation.

Three more parts (9–11) of Lindner's *Die Fliegen der Palæarktischen Region* have appeared during the last half year, and a further volume in the *Faune de France* series by E. Seguy (Paris, Paul Chevalier, 109 pp.) deals with the blood-sucking Nematocera, including the Culicidæ, Simuliidæ, and Phlebotominæ ; synoptic keys are freely used. A. H. Sturtevant (*Journ. N. York Entom. Soc.*, **33**, 195–215), **34**, 1–20) has made a study of the seminal receptacles and accessory glands of the females in the families of the Acalypteræ and discusses their classification based on the observations made. F. G. S. Whitfield (*Proc. Zool. Soc.*, 1925, 599–638) shows that the modification of mouth parts in association with habit is well seen in members of the Asilidæ, which represent the climax of predaceous diptera, and with the exception of certain Empidæ are the only British flies whose food consists entirely of living prey : the labium, which in diptera is generally a highly developed part of the trophi, is less important in the predaceous types, and in the Asilidæ the hypopharynx assumes the chief function, being somewhat modified in association with the habits of these insects.

*Other orders.*—J. M. Brown (*Journ. Linn. Soc. (Zool.)*, **36**, 201–18) describes some Collembola collected in Mesopotamia, being the first examples of these insects taken in that region. M. Oscar John (*Treubia*, **8**), deals with the termites of Ceylon, the Malay Peninsula, and the islands of Sumatra, Java, etc., a discussion on the post-embryonic development and caste differentiation being given. C. Macnamara (*Canad. Entom.*, **58**, 53–4) has made some observations on the “drumming” of Stone-flies (Plecoptera) at his home in Ontario. These insects make a fairly loud drumming sound, which can be heard from 15–20 feet away, which is due to the action of a kind of percussion hammer, situated on the 9th abdominal segment, with which it is thought the male beats on the surface on which it is resting: the drumming resembles successive taps, which probably occur somewhat less than 30 beats per second, as the note is almost a continuous one. B. Schwartz (*Philippine Journ. Sci.*, **27**, 235–9) describes a new Strepsipteron (*Macroxinos piercei*, gen. et. sp. nov.) the pupæ of which stylopise the Eumenid wasp *Rhynchium atrum* in the Philippines. E. B. Worthington (*The Entomologist*, **59**, 138–42) has some observations on the life-cycle of the common earwig *Forficula auricularia*, the main points being (1) the male and female live together before oviposition, and pairing takes place frequently at this period; (2) the males leave the nest about the time of oviposition (January to April); (3) young nymphs as well as eggs are cared for by the mother; (4) nymphs as well as adults survive the winter.

**AGRICULTURAL PHYSIOLOGY.** By JOHN HAMMOND, M.A., School of Agriculture, Cambridge.

*Milk Secretion.*—During the last few years statistical methods have been very largely used to elucidate the physiological processes of milk secretion, not only with regard to the yield obtained, but also as to the mode of secretion of the various constituents. The errors introduced in approximate methods of estimating milk yield and butter fat production during a lactation have been calculated by Sheehy (*Sci. Proc. R. Dublin Soc.*, **18**, 1926), who finds that, while weekly records agree very closely with the true figures and fortnightly tests do not diverge considerably, the tests taken at longer intervals than this are liable to give single results which are seriously above or below the correct figure. Mesdag (*C.R. Cong. Int. p. l'élev. de l'Espèce Bovine*, The Hague, 1923) also had concluded that testings should be made at least once a fortnight to obtain accurate results.

Gowen (*Milk Secretion*, Baltimore, 1924) investigated the milk records and butter-fat tests of the American Friesian

Societies' advanced register, and in addition to many findings bearing on the inheritance and selection of high milking cows, his results are of physiological interest. He shows that the milk yield and butter-fat production of a cow increase up to her eighth year, and then decline again, as was also found by Clark (*Jour. Dairy Sci.*, **7**, 1924). Gowen investigated the relation between conformation and milk yield, and concludes that conformation is nearly, if not entirely, worthless for predicting butter-fat percentage, but that a cow must have size to produce large quantities of milk. In a later paper (*Jour. Agr. Res.*, **30**, 1925) he finds that all body measurements are related to milk yield, so that increase in any one results in increase of milk in the seven-day test; weight was the most important factor, but body length, width, and girth followed closely. It should be noted, however, that this applies to measurements within one breed only, and the actual correlation coefficients were low.

Hammond and Sanders (*Jour. Agr. Sci.*, **13**, 1923) from an investigation of milk records attempted to measure the extent to which various environmental factors, such as month of calving, dry period, service period (pregnancy), and age affected the total lactation yield of a cow, so that by using corrections for these the true capacity of the animal for milk production might be more nearly determined. The shape of the lactation curve was also used to determine exactly how the effects on total yield were produced in each case. The highest lactation yields were given by cows calving in the autumn months. Very similar results were also obtained by Turner (*Jour. Dairy Sci.*, **6**, 1923), and by Wylie (*Jour. Dairy Sci.*, **8**, 1925) for American cows, while the latter also found that the yearly fat production followed much the same course. Zwagerman (*Off. Org. Algem. Nederlands. Zuivelbond*, No. 1008, 1925) obtained similar variations for cows in Zeeland.

Sanders (*Jour. Agr. Sci.*, **13**, 1923), using a "shape figure" for the lactation curve, found that in high yielders the maximum daily yield is lower relative to the total yield than in bad milkers, and that heifers give a smaller maximum relative to their total yield; by this "shape figure" he measures the persistence in yield of a cow. McCandlish (*Jour. Dairy Sci.*, **7**, 1924) has also shown the importance of persistence of yield as well as a high maximum yield in a cow. Brody, Ragsdale, and Turner (*Jour. Gen. Physiol.*, **5**, 1922-3, p. 441) have determined the effect of breed on the rate of decline of milk secretion with the advance of the period of lactation, the rate of secretion falling off very abruptly in scrubs or beef-bred animals as compared with milking breeds. The same authors (*Jour. Gen. Physiol.*, **5**, 1922-3, p. 777) also show that the rate of

decline is much greater when the cow is pregnant, and suggest that the difference in milk flow equals the amount which would be required for the formation of a calf. Gaines and Davidson (*Jour. Gen. Physiol.*, **9**, 1926, and *Illinois Agr. Exp. Stn. Bul.*, **272**, 1926) conclude that up to five months pregnancy does not appreciably affect the rate of milk secretion but that advanced pregnancy causes a marked decrease; they reject the idea that this is due to the demands of the growing foetus for nutriment, and believe that the cause is a hormone thrown into the circulation during pregnancy, the source of the inhibitor being removed with the birth of the foetus. Both Gavin (*Jour. Agr. Sci.*, **5**, 1913) and Hammond and Sanders (*Jour. Agr. Sci.*, **13**, 1923) found that the inhibition begins about the twentieth week of pregnancy, and it has been shown by Woodman and Hammond (*Jour. Agr. Sci.*, **12**, 1922, and **13**, 1923) that the character of the secretion obtained from the teats of first calf heifers changes at this time, from a serous fluid to a thick secretion containing a high percentage of globulin, which, mixed with the milk formed at a later stage, forms colostrum. Anatomical and histological work has also shown (Hammond, in *Cattle Breeding (Proc. Scottish Cattle Breeding Conference, Edinburgh, 1925)*) that at the fifth month of pregnancy alveolar growth begins in preparation for the ensuing lactation. Drummond-Robinson and Asdell (*Jour. Physiol.*, **61**, 1926) found that in goats, in which this thick secretion also occurs at mid-pregnancy, removal of the *corpus luteum*, which results in abortion, leads to the production of milk when the operation is performed after the advent of the thick secretion, but does not do so when the operation is performed before the thick secretion appears. It was found by Woodman and Hammond that when the thick secretion was milked out regularly, milk is formed long before the birth of the calf. Asdell (*Jour. Agri. Sci.*, **15**, 1925) has investigated the quantity and composition of the secretions throughout the whole "up curve" of lactation during the first pregnancy in heifers and goats; he considers the globulin as being an excretion of the cell when it changes from the growth phase to the secretory phase of its life. The rate of change in composition from colostrum to milk after parturition has been shown by Grimmer (*Milchwirtsch. Forschungen*, **2**, 1924) to follow regular logarithmic curves.

Hunt (*Jour. Dairy Sci.*, **7**, 1924) suggests that there is a relation between high fertility and high milk production. McCandlish (*Jour. Dairy Sci.*, **9**, 1926) found a decrease in milk production on the day of heat, and on the day following, but notes that wide individual differences are found; this was also observed by Isaachsen (*Proc. World's Dairy Congress*,

U.S.A., 1923), who states that the depressing effect can be counteracted by careful milking on these days. Fitch and Copeland (*Jour. Dairy Sci.*, **7**, 1924) find that no one quarter of the cow's udder regularly exceeds the others very much in either quantity of milk or percentage of fat, although individual differences were found.

Ragsdale, Turner, and Brody (*Jour. Dairy Sci.*, **7**, 1924) find that the rate of milk secretion is governed by the milk accumulated in the udder or intervals between milkings, and like a chemical action is slowed by the accumulation of products, the relative amounts secreted in each successive hour after the first being approximately 95 per cent. of the previous hours' yield. Davidson (*Jour. Dairy Sci.*, **7**, 1924) investigated the effects of incomplete removal of the milk on subsequent milkings and found that the percentage lactose tends to decrease following incomplete milking, although the average yield of milk and percentage fat tends to increase. Mackenzie and Marshall (*Jour. Agr. Sci.*, **15**, 1925) when investigating the occurrence of supernumerary glands and teats, which exist in rather over 50 per cent. of the cows in this country, found that the milk formed by these glands is absorbed into the circulation, lactose being excreted in the urine.

Gowen (*Milk Secretion*, Baltimore, 1924) concluded that, as regards the mode of secretion of milk, we must look to the solids-not-fat as the major cause of the variations in yield of milk, and to some extent as the cause of variation in butter-fat yield. Gaines and Davidson (*Illinois Agr. Exp. Sta. Bul.*, **245**, 1923) from the yields and fat percentage records of several breeds conclude that the milk yield of cows with varying fat percentage is such that the total energy value of the milk is constant if the effects of all factors other than the composition are excluded, *i.e.* the milk yield is inversely proportional to the energy value of the milk solids per unit of milk. Gaines (*Jour. Dairy Sci.*, **8**, 1925), from a statistical treatment of milk analyses, states that the secretion of fat and protein are intimately related, but that the secretion of water and fat or protein are largely independent. Andersen and Langmack (*Kgl. Vet. og Landbohøjskoles Lab. f. landøk. Førsøg*, Copenhagen, **113**, 1923) have shown that the percentage of protein in milk varies in a regular way with the time of year, being highest in November-January, and lowest in May-July, and that the percentage of fat follows much the same course. It is interesting to compare this with the mean weekly yields (given by Hammond and Sanders) which are lowest in November-January, and highest in May-July. Hays (*Jour. Dairy Sci.*, **9**, 1926) states that temperature is the major factor in the seasonal variation in fat percentage,

a low fat percentage being associated with a high temperature and vice versa. Tocher (*Variations in the Composition of Milk*, Edinburgh, 1926), among other results in a large statistical work on the composition of milk in Scotland, finds the maximum yield per milking in May and June and lowest in December and January; but he obtained the highest fat percentage in September and October, and the lowest in February and March. He agrees with Gowen that a rise in percentage of solids-not-fat is associated with a rise in fat percentage, and also shows that the percentage of albumin nitrogen increases and the percentage of lactose decreases during the lactation period. One of his main conclusions is that the higher the yield of milk the greater is the percentage of lactose. Husband and Taylor (*Jour. Agr. Sci.*, **12**, 1922) suggested that the formation of lactose governs the rate of milk secretion by its effect on osmotic pressure.

*Nutrition.*—A comparative account of the processes of digestion in vertebrates has been compiled by Scheunert (*Handb. d. Bioch. d. Menschen u. d. Tiere V*, **2**, Jena, 1924), in which the domestic species are treated in detail. Tocher notes a greater yield of milk when cows first go out to grass, and this was also shown in the lactation curves by Hammond and Sanders. Sanders, in *Cattle Breeding* (*Proc. Scottish Cattle Breeding Conference*), Edinburgh, 1925, suggests that the drop in yield which occurs at the end of winter in the Penrith district, and which is not paralleled in Norfolk records, may be due to the greater root feeding practised in the latter locality. Jesse (*Jour. Ministry of Agri. and Fish.*, **32**, August 1925) also concludes that the yield of milk rises with the quantity of roots fed. Nils Hansson (*Centralanst. f. Försöksv.*, Stockholm, Meddel. No. 268, 1924) finds that roots up to 60–70 lb. are better than no roots at all, but the optimum quantity for high-yielding cows is just over 30 lb. per day. Boutflour (*Jour. Brit. Dairy Farmers' Ass.*, **37**, 1925), however, states that high yields can be better maintained by dry feeding on meals, cake, and hay, and in particular emphasises the necessity for cutting down the bulk of the ration in a heavy-milking cow. These opinions have led to considerable discussion in agricultural circles (see Mackintosh, *Jour. Farmer's Club*, March 1926).

Woodman, Blunt, and Stewart (*Jour. Agr. Sci.*, **16**, 1926), from a comprehensive investigation of the nutritive value of pasture, concluded that well-grazed pasture grass in respect of digestibility compares favourably with concentrates like linseed cake, and is far superior to meadow hay of the best quality; the fibre in young grass has a very high degree of digestibility, almost approaching that of carbohydrates, while

grass at this stage is also particularly rich in digestible protein. Ereky (*A Zöldtakarmánymalom és a hagy Istállóüzemek*, Budapest, 1925) has also drawn attention to the high nutritive value of young growing herbage as compared with that at a later stage of growth, and has developed a process for preserving it for winter feeding in this stage in place of haymaking. Wiegner (*C. R. Cong. Int. p. l'élev. de l'Espèce Bovine*, The Hague, 1923), who investigated the electrical process of preserving green crops, states that, although this offers many advantages, in that material with high water content can be preserved, the loss in production is equal to that of silage making. With regard to the nutritive value of roots and coarse forage Møllgaard (*Kgl. Vet. og Landbohøjskoles Lab. f. landøsk. Forsøg.*, Copenhagen, 111 Ber., 1923), on the basis of metabolism experiments, has shown that when the protein content in the feed is low the biological value of the protein supplied has probably a great influence on the net energy value, *i.e.* in feeding cattle on roots with a low albuminoid ratio (1 : 15), the biological value of the protein supplied is probably of determining influence on the gain made. His results do not support Armsby's conclusion that the metabolisable energy of a single feeding stuff has a constant value according to the digestible organic matter. An account of recent metabolic experiments has been published in a monograph by Armsby and Moulton (*The Animal as a Converter of Matter and Energy*, New York, 1925).

Isaachsen (*C. R. Cong. Int. p. l'élev. de l'Espèce Bovine*, The Hague, 1923) has found that the efficiency of sugar in milk production is only about 90 per cent. of that of starch. Møllgaard (*C. R. Cong. Int. p. l'élev. de l'Espèce Bovine*, The Hague, 1923) has shown that the net energy of a feeding stuff is greater in milk production than in fat production, which, according to Nils Hansson (*C. R. Cong. Int. p. l'élev. de l'Espèce Bovine*, The Hague, 1923) depends on the fact that in milk production the proteins of the food are better utilised than in fattening. Møllgaard also states that the production equivalent of the milk is probably independent of the composition of the milk, although Gaines (*Jour. Agr. Res.*, 29, 1924) bases the feed cost of milk production on Haccker's law that the nutrients required for lactation are directly proportional to the energy value of the milk solids.

Using indirect calorimetry Magee, and Magee and Orr (*Jour. Agr. Sci.*, 14, 1924) found that the critical range of temperature for the goat is 55°–70° F., and that during pregnancy the metabolism remains steady from the fourth to the thirteenth week, but steadily increases from the fourteenth to twenty-first week, when it is 50 per cent. higher than at the fourteenth

week ; they also investigated the metabolism after feeding. Kriss (*Jour. Agr. Res.*, **30**, 1925) compared the direct with the indirect method of calorimetry in cattle and found that, whereas the former is more accurate in heat computation, they are both about equally accurate in the measurement of the nutritive value of feeds.

Cary and Meigs (*Jour. Agr. Res.*, **29**, 1924) have studied the changes in intermediary metabolism in milking cows, following the changes in amino acid content of the blood plasma resulting from changes of feed. Little and Wright (*Brit. Jour. Exp. Path.*, **6**, 1925) have shown that the calcium content of the blood plasma of cows suffering from milk fever is considerably decreased. The effect of ultra-violet rays in increasing the retention of calcium and phosphorus during pregnancy, and in reducing its loss during lactation, has been shown by Henderson and Magee (*Bioch. Jour.*, **20**, 1926). Drummond *et al.* (*Jour. Agr. Sci.*, **14**, 1924) find that the feeding of cod-liver oil in small quantities to cows on a winter ration of concentrates, roots, and brown hay prevents the falling off in vitamin-A content of the butter produced, as would otherwise occur during the period. They also found that a large addition of the oil depressed the fat percentage in the milk, without, however, affecting the yield of milk.

## ARTICLES

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### HOST-PARASITE SPECIFICITY AMONG HUMAN PROTOZOA

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ONE of the most interesting phases of the biology of host-parasite relationships is that of host-parasite specificity. The relations between a host and its parasites, as recently described by the writer (Hegner, 1926), may be divided into those concerned (1) with the epidemiology of transmission, (2) with the interactions of the organisms during a natural infection, and (3) with host-parasite specificity. When infective stages gain entrance to a susceptible host they are distributed and become localised in some particular region which is the primary site of infection; later the parasites may establish themselves in secondary sites of infection. Certain natural resistances of both host and parasite come into play; if those of the host are too effective the parasite is unable to initiate an infection; if the parasite is able to overcome the host resistance it successfully establishes itself. The method of attack determines the changes that are brought about by a parasitic invasion; symptoms develop as a result of changes in the physiological condition of the host; the tissues of the host are modified (pathogenesis); and in some cases the host succeeds in building up a resistance (immunity) which the parasite cannot withstand. On the other hand, the parasite may likewise acquire resistance as a result of residence in a host and changes may occur in the direction of greater invasive powers. Various adjustments may take place during an infection resulting in the carrier condition, latency, and relapse. All of these interactions have an influence upon host-parasite specificity.

By host-parasite specificity is meant the association of a particular species of host with a particular species of parasite. Such an association may be rigid, that is, a species of parasite may be able to live in only one host species; in this case the host tolerates the parasite and is known as a natural host, whereas other hosts are refractory to this species of parasite

and are known as foreign hosts. Often host-parasite specificity is not so limited: for example, *Balantidium coli*, which sometimes occurs in the large intestine of man, may also live successfully in pigs and monkeys and possibly in guinea-pigs. Various degrees of host-parasite specificity are also exhibited by parasites and their intermediate hosts. Apparently this relationship is less rigid since several species of intermediate hosts may serve as transmitting agents of a single parasitic species.

The situation as regards the twenty-five species of protozoa that are generally admitted by all protozoologists to be inhabitants of man is in brief as follows.

*Endamoeba histolytica* is a common inhabitant of the large intestine of man. Specimens indistinguishable from it are known to occur in monkeys and in frogs. The latter do not become infected when fed cysts from man. Dogs have been reported with infections in nature, but no cysts were passed. Cats, especially young ones, may be infected in the laboratory but pass no cysts. Rabbits and guinea-pigs have been infected experimentally. Apparently rats and mice are susceptible to infection; the rats pass cysts which are infective to other rats.

Certain other human amœbæ, according to Kessel (1923, 1924), may be successfully transmitted to rats and monkeys; these include *Endamoeba coli*, *Endolimax nana*, and *Iodamoeba williamsi*. An iodamoeba that may be the same as that in man has been reported from pigs.

The "intestinal" flagellates, *Trichomonas vaginalis*, *T. buccalis*, *T. hominis*, *Chilomastix mesnili*, *Embadoomonas intestinalis*, *Tricercomonas intestinalis*, and *Giardia lamblia* all seem limited to man. The cysts of these flagellates as well as of the amœbæ may pass through the digestive tract of flies and other insects without injury but do not set up infections in them.

The human trypanosomes are limited to man, certain blood-sucking insects and animal reservoirs. *Trypanosoma gambiense* passes part of its life cycle in the tsetse fly, *Glossina palpalis*, and possibly in other species of this genus, and in wild game, especially antelopes. *T. rhodesiense* parasitises *Glossina morsitans* and may also infect animals. *T. cruzi* is carried by *Triatoma megista* and several other species of this genus of bugs; the armadillo is its animal reservoir.

The leishmanias of man probably live in flies of the genus *Phlebotomus*. They have also been experimentally established in certain other blood-sucking insects. The leishmania of the dog is by some considered the same as the species that causes kala-azar in man.

Of the Sporozoa that occur in man, the coccidium, *Isospora hominis*, is not known to infect any other animal; the muscle parasite, *Sarcocystis*, is probably a species from a lower animal

that very infrequently succeeds in setting up a human infection ; and the three species of malarial parasites can live only in certain Anopheline mosquitoes, although the organisms of monkey malaria may belong to the same species as those of man.

The ciliate, *Balantidium coli*, lives in the large intestine of the pig and man and has been reported from monkeys. Monkeys have been infected with specimens from the pig and man, and the pig with specimens from the monkey ; and it is supposed that human beings usually become infected by ingesting cysts passed by pigs. A species of *Balantidium* that lives in the guinea-pig may be the same as that in man.

Biological studies of the relations between protozoan parasites and their hosts, especially man, have within the past thirty years brought about a marked change in our ideas regarding host-parasite specificity. Until quite recently the belief was prevalent that cross-infection is the rule in nature ; for example, that man is infected with protozoa of lower animals and that lower animals are regularly parasitised by human protozoa. Thus, where several decades ago one species was supposed to inhabit a number of species of hosts we know to-day that in many cases each species of host is parasitised by its own species of parasites, which appear to be rigidly adjusted to it and unable to live in any other species of host. Some of the questions involved in the study of host-parasite specificity are stated in the following paragraphs and suggestions are presented to account for the facts observed. The conclusion reached is that we know very little about this interesting and important subject, but that further experimental study is possible and desirable.

*To what Extent does the Behaviour of the Host and that of the Parasite determine Host-parasite Specificity?*—This problem involves particularly the question of transmission, which the writer has treated at some length in another place (Hegner, 1926 b). It is obvious that host and parasite must be brought together under favourable conditions when the host is susceptible and the parasite infective. This can be done in the laboratory with hosts and parasites that do not ordinarily encounter each other in nature. A study of protozoan transmission in nature, however, reveals the fact that the parasite is passive during its passage from one host to another and that it is the behaviour of the host or intermediate host that is responsible for transmission. " Intestinal " protozoa are transmitted in the active (trophozoite) stage or in the form of cysts. Those inhabiting the mouth and vagina are transferred by contact entirely by the host during kissing or coitus. These appear to be present in from one-third to one-half of the general

population. Those that live in the intestine are transmitted by the contamination of food or drink with feces containing cysts or trophozoites. The host, man, is responsible for the proper disposal of his own feces so that food or drink may not become contaminated. By certain methods of control, such as the elimination of infected food handlers, of the common towel, of soil pollution and house flies, he can to a considerable degree prevent the spread of infection. That insanitary conditions are prevalent is indicated by the high incidence of infection among the general population, which is estimated approximately as follows: 50 per cent. with *Endamœba coli*, 25 per cent. with *Endolimax nana*, 10 per cent. with *Endamœba histolytica*, 10 per cent. with *Iodamœba williamsi*, 15 per cent. with *Giardia lamblia*, and 10 per cent. with *Chilomastix mesnili*. Frequently one individual is infected with two or more of these species at the same time. Fortunately *Endamœba histolytica* is the only pathogenic species of great importance in this list.

The differences in the percentages noted above and the small numbers of infections that have been recorded for the other nine species of intestinal protozoa that occur in man are probably due principally to two factors: first, the success of the species in gaining entrance to the digestive tract, and second, the infectivity of the species in the human host. *Trichomonas hominis*, for example, does not possess a cyst stage in its life-cycle and hence must pass from man to man in the trophozoite stage (Hegner, 1924 a). The trophozoite stage is not as resistant as the cyst stage, hence it is more often destroyed before ingested by man than are cysts. This may account for the fact that less than 10 per cent. of the general population seems to be infected with this species. But great differences exist between species that are spread by cysts. *Endamœba coli*, with infections in about 50 per cent. of its possible hosts, seems much more successful than *Endamœba histolytica* with an incidence of infection of only about 10 per cent. This is true in spite of the fact that *E. histolytica* cysts are apparently more abundant in fecal material from a host than those of *E. coli*. The chances of reaching new, susceptible hosts seems about the same for the two species. There may be a difference in the resistance of the cysts of the two species while outside of the body, but this is hardly probable. Perhaps *E. coli* is more successful because its ripe cysts normally contain eight nuclei and presumably give rise to eight offspring within the intestine, whereas the cysts of *E. histolytica* possess only four nuclei. This would give *E. coli* a better chance of starting an infection. The activities of the two species are also different within the intestine. *E. coli* lives in the lumen on bacteria and food particles, whereas *E. histolytica* depends on

tissue elements from the intestinal wall. *E. histolytica* must therefore gain access to this tissue and successfully attack it against the resistance of the host, whereas *E. coli* is continually bathed in its nutrient medium. Furthermore, *E. histolytica* often brings about a diarrheic or dysenteric condition during which no infective (cyst) stages are passed by the host, and sometimes this species actually brings about the death of the host, thus destroying its own chances of further distribution.

Blood-inhabiting protozoa, that all seem to be distributed by blood-sucking insects, are likewise passive while outside of the human host. Their spread is due to both the human host and the insect host, and in certain cases, for example, the trypanosomes, to animal reservoirs in addition. The usual course of events includes the sucking up of human blood containing infective stages; the parasitism of the insect host; and the infection of susceptible human hosts by the inoculation of infective parasites into them by the insect. Infected human beings allow themselves to be bitten by susceptible insect hosts and the latter in due time carry the parasites to other human beings who do not protect themselves from attack. The factors of transmission are (1) infected persons, (2) susceptible insects, and (3) susceptible human beings; these must be present in the same locality at the same time. The parasite simply allows itself, because of the careless behaviour of man, to be transmitted by its hosts. When animal reservoirs are available the situation is more complex, but the chances of the successful transmission of the parasite are even greater. In the case of human malaria there are no known animal reservoirs, and man carries the parasites over the winter in his body and relapses during the spring just when the mosquitoes become active and available as transmitting agents. All the malarial parasite does is to multiply itself by asexual reproduction and form gametocytes in the blood of man and to undergo sexual reproduction in the mosquito during which enough sporozoites are formed to furnish the salivary glands with numbers sufficient to bring about infection in susceptible human beings that the mosquito may bite.

The conclusion is reached that the behaviour of the host or intermediate host plays an important rôle in host-parasite specificity since the infective stage of a parasite can reach its specific host only by being passively transferred by the latter; and this must occur regularly in nature in order that the race of parasites may continue to exist.

*Do Species of Protozoan Parasites that are restricted to one Species of Host gain Access to other Species of Hosts?*—The answer to this question differs for the different species of parasites and depends, as above, on the behaviour of the hosts and

intermediate hosts. Man's food and drink, for example, are no doubt frequently contaminated by the feces of rats, mice, cats, dogs, and other domestic animals that contain living, infective cysts of various intestinal protozoa, such as *Endamoeba muris* of the rat, *Giardia canis* of the dog, and *Isospora felis* of the cat. No human beings, however, have ever been reported with infections due to these species. The culicine mosquitoes that transmit the organism of bird malaria and the horse-flies that transmit the organism of surra in horses no doubt frequently inject infective stages of these species into the blood of man; but the parasites do not bring about infections. On the other hand, man does not succeed in protecting his feces from lower animals, and there can be no doubt that rats, mice, cats, dogs, etc., ingest living trophozoites and cysts of human intestinal species. These animals, however, with very few exceptions, do not seem to become infected in nature with species from man. Similarly, the insect hosts of blood-inhabiting human protozoa no doubt are continually inoculating infective stages into domestic animals without bringing about infection; for example, anopheline mosquitoes that are parasitised by the organisms of human malaria often transfer these parasites to horses, cattle, and pigs, but no infections result.

We may conclude therefore that the infective stages of human protozoa frequently gain access to lower animals and that those of the latter gain access to man. The entrance of the infective stages of a species of parasite into a host is necessary for host-parasite specificity, but is only one factor in this relationship.

*What Factors within a Host enable Natural Parasites and prevent Foreign Parasites from bringing about an Infection?—*By "natural" parasite is meant one that is infective to a certain species of host in nature and a "foreign" parasite one that is not. To answer this question we should consider that part of the host in which the parasite lives as its particular habitat, just as we look upon certain fresh-water ponds as the habitat of free-living species. Both free-living and parasitic protozoa are at times subjected to certain factors in their habitats that are harmful, and successful life and reproduction depend on the severity of these harmful factors. The digestive juices of the host, for example, have been considered destructive to trophozoites even of natural protozoan parasites. Recent experiments by the writer (Hegner, 1924, 1926 a), however, indicate that the injurious effects of the digestive juices are probably overestimated and that trophozoites may reach the large intestine without being harmed. The cyst wall of intestinal protozoa protects the organism from many conditions

outside of the body and may play an important rôle in the initiation of an infection ; for example, it may react to the digestive juices of the host, or to secretions of the parasite within the cyst stimulated by the intestinal environment, so as to liberate the enclosed parasite and give it a chance to maintain itself there ; or it may fail to liberate the parasite and thus prevent infection. We know so little about excystation that nothing definite can be said on this subject.

Among the variable conditions within the intestine are those due to the character of the diet. As pointed out fully in earlier papers (Hegner, 1923, 1924) carnivorous animals are very seldom parasitised by intestinal protozoa and omnivorous species such as the rat and man can be relieved of some of these organisms if fed on a carnivorous diet. This result seems to be due not to changes in the hydrogen-ion concentration of the intestinal contents (Hegner and Andrews, 1925) but to the change from a predominantly acidophilus type to a predominantly putrefactive type of bacteria. Such a change, of course, brings about many profound changes in the intestinal contents, which apparently make them unfit as a medium for the growth and multiplication of certain protozoa. The character of the intestinal contents resulting from the normal dietary of the host may thus prevent a foreign species from initiating an infection even if it succeeded in reaching the normal location in the host unharmed.

A very interesting situation is provided by the malarial parasites of man and bird in relation to their insect hosts. The human species, *Plasmodium vivax*, is transmitted by certain species of anopheline mosquitoes. Both gametocytes and asexual stages that are present in human patients are sucked into the stomach of the mosquito with the blood. Here the asexual stages are destroyed but the gametocytes are stimulated to further development, undergoing maturation and fertilisation. The resulting zygotes penetrate the stomach wall of the mosquito, where they become oocysts, in which sporozoites are developed. Different species of anopheline mosquitoes, however, differ with respect to their susceptibility ; some species almost always become infected, whereas others are parasitised only with difficulty ; some do not appear to acquire infection in nature but may be parasitised in the laboratory. All degrees of susceptibility are exhibited by various species of anophelines. In the case of the organism of bird malaria, *Plasmodium præcox*, both asexual and sexual stages are destroyed in the stomach of anophelines, but the latter are stimulated to develop in certain culicine mosquitoes. The conditions within the bodies of anopheline and culicine mosquitoes, and especially of closely allied species of anophelines,

must be very similar, which indicates that the adjustments of the malarial parasites to their insect hosts are very delicate. Just what factors are responsible for these apparently minute differences is unknown, but the problems involved are open to experimental study.

The character of the digestive juices or of the blood-stream, failure of cysts to excyst, the character of the diet, and various other factors may, therefore, encourage or prevent parasites that gain access to the body of the host from setting up an infection.

*How may we account for Laboratory Infections in Foreign Hosts?*—As stated above, it is possible in certain cases to bring about an infection in a certain host species in the laboratory that appears never to become parasitised in nature. Several explanations suggest themselves to account for this phenomenon. In the first place, the host or intermediate host may behave in such a way as never to encounter the infective stages of the parasite in nature. For example, we would hardly expect an animal that does not live in association with man to become infected with human parasites, although it might be susceptible as indicated by laboratory experiments. The number of parasites that gain access to a host may be an important factor; that is, a few specimens may not succeed in bringing about an infection, whereas large numbers of specimens might. Darling (1910), for example, found that on an average twelve gametocytes of human malarial parasites per cubic millimetre of blood were necessary to infect certain mosquitoes. In this case, probably the successful consummation of fertilisation within the stomach requires a considerable number of gametocytes. This necessity for the presence of large numbers of parasites may account for the great number of clinical cases of amoebiasis that occur in the tropics, where the ingestion of large numbers of cysts is favoured by meteorological and insanitary conditions.

The method of entrance of the parasites may play a rôle in the initiation of an infection. For example, cats apparently do not often become parasitised by *Endamæba histolytica* in nature but may be infected in the laboratory in several ways. The method that results in the greatest success seems to be that of Sellards and Theiler (1924), who produce stasis by surgical ligature of the large intestine and then inoculate cysts anterior to the ligature. Their experiments show that excystation occurs at the point of stasis and that no excystation might take place if stasis was not induced. Many infections are no doubt prevented in nature by the rapid passage of the cysts through the digestive tract.

We can thus account for laboratory infections in foreign

hosts by the bringing together of a host and parasite that do not ordinarily become associated in nature ; or by the use of very large numbers of parasites ; or by procedures not possible in nature.

*What Conditions are responsible for Differences in Susceptibility between Young and Adult Animals?* The greater susceptibility of young animals to infection has been abundantly demonstrated in the case of many species of protozoa. Surveys of intestinal protozoa in various parts of the world have established the fact that children, as a rule, are more highly infected than adults. Perhaps hosts that are infected while young acquire immunity before the adult stage is reached ; but a protozoan that is able to live in a host for only a brief period cannot be considered entirely successful as a specific parasite. Similar results have been obtained in laboratory experiments ; for example, kittens become infected with *Endamoeba histolytica* much more readily than adult cats. There are no essential differences among the cysts used in such experiments, hence the factors involved must reside in the hosts. Some type of resistance develops with age. Are the cysts of intestinal protozoa unable to excyst ? Are the trophozoites prevented in some way from entering the tissues ? Does the medium (intestinal content or blood-stream) become unfavourable as the host grows older ?

*What Host-parasite Interactions terminate an Infection or bring about Periods of Latency and Relapse?*—Spontaneous recovery occurs in many parasitic infections. This is due largely to the building up of some sort of resistance by the host. In free-living protozoa, the products of metabolism of the protozoa themselves may render the medium unfit and thus exterminate the organisms. This may possibly occur in the case of certain parasites. Failure of the food-supply probably plays a minor rôle in the lives of parasitic protozoa. In malaria, amœbiasis and certain other protozoan infections an acute attack is often followed by a period of latency during which the parasites are few in number ; another acute period (a relapse) may ensue to be followed by a second latent period, and alternate periods of latency and relapse may continue over a number of years.

Two factors probably work together to bring about this result ; one is the change that occurs in both host and parasite in their attempt to counteract resistances built up against each other, and the other consists of changes in the parasite's environment due to modification in the physiological state of the host. Both of these factors may be illustrated by events that occur in human malaria ; the first when most of the parasites are destroyed by some type of host resistance in cases of

spontaneous recovery ; and the second when a relapse develops from latency because of some change in the physiological condition of the host.

*Summary.*—A rather rigid host-parasite specificity exists among human protozoa ; certain species are not known to occur in any other host, and others in only one or several animal reservoirs. Species that pass part of their life-cycle in an insect host are usually restricted to one principal species or to several species belonging to one genus.

Host-parasite specificity depends on the behaviour of the host and intermediate host ; the parasites are passive and the association can only be maintained by the activities of the host or insect vectors.

The circumstances that bring about the invasion of a host by a parasite represent only one factor in host-parasite specificity, since man is frequently invaded by the parasites of lower animals and the latter by those of man without initiating infections.

Of great importance are conditions within the host and intermediate host that constitute the environment of the invading parasite. The blood-stream and various parts of the digestive tract differ in content in different species to such an extent that infection is impossible or transitory. In other words, each parasite is adapted to life in one or a very few hosts or intermediate hosts.

Experimental infections of foreign hosts in the laboratory are due to the association of organisms not ordinarily occurring in nature ; to the inoculation of large numbers of infective parasites, or to methods of entrance into the host not possible in nature.

Young animals, as a rule, are more susceptible to infection with either natural or foreign parasites than adult animals. Apparently the natural resistance of the host increases with age, but the mechanism of this resistance is not known.

Acute infections with parasitic protozoa often come to an end or change to latent infections which later give rise to relapses. These modifications in the host-parasite relations are due principally to struggles between host and parasite which result in the building up of some type of resistance by one or both, and to changes in the physiological condition of the host which affect for better or worse the environment of the parasite.

In conclusion, the point may be emphasised that the subject of host-parasite specificity is one that needs and is worthy of careful investigation, and that this paper is intended merely to indicate some of the interesting problems involved.

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# A STATISTICAL STUDY OF THE RELATIONSHIP BETWEEN MENTAL OR ATHLETIC ACTIVITY AND PHYSICAL OR RESPIRATORY DEVELOPMENT

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THOUGH both mental and physical activity are accompanied by fatigue there seems at first little else to connect the one with the other. It would seem as if they must be derived from a common source, since excessive expenditure in one involves restricted expenditure in the other. In children an additional factor obscures the issue, namely, the process of growth, which absorbs a considerable amount of bodily energy. But in boys of like age, who may be supposed normally to have attained about the same growth, one might expect to find some correlation between physical or respiratory development and mental or athletic activity. This article describes an attempt to discover such a relationship.

This was tested in different ways on different groups of boys in Manchester Grammar School. Physical measurements have been taken there on a uniform basis since 1881 and breathing measurements since 1921. The record of every boy in the school, in work, in games, and in physical development, is kept on a separate card and these cards are all filed on a suitable card-index system. For a more detailed account of the nature of some of the material used reference may be made to a paper in *Biometrika* (August 1923), by Mumford and Young, on the "Interrelationships of the Physical Measurements and the Vital Capacity." It will suffice here to explain that there are about 1,200 boys in the school, of whom some 55 per cent. come from elementary schools. Further, about 15 per cent. are of Jewish origin.

(1) The first test consisted in a direct comparison of the mean height (H), weight (W), and chest-girth (C) of (a) a group of boys of high mental ability, and (b) a group of boys chosen for the gymnasium team with the corresponding measurements of the average boy.

Boys who reached the Classical or Science sixth forms and who afterwards gained open scholarships at Oxford or Cambridge, first classes in examinations for an honours degree, or some similar distinction were defined as boys of special ability, and the combined judgment of the High Master and Form Masters also helped in their determination. The mean H, W, and C of these boys, during the period of their stay at the school, were then compared with the corresponding mean measurements for boys of the same age in the whole school at about the same period of time. The difference between the means was estimated as significant or otherwise according to its size in relation to the size of its probable error.

A like comparison was possible between a number of boys chosen from time to time for the gymnasium champion "eight" and the average boy. Alertness and agility counted for more than physical strength in their selection.

The results are shown in Table I. They indicate that the gymnast is possibly a trifle shorter than the normal boy between the ages of 13 and 16, a trifle heavier between 17 and 19, and broader-chested at all ages, markedly so after 14. The scholar is a trifle taller than the normal boy between 14 and 19, a trifle heavier between 14 and 17, and rather broader-chested between 14 and 18, if not at all ages.

The material used in this first method was spread over twenty years, during which there has been an appreciable improvement in physique. The median of the series of measurements would be somewhere in the region of 1902-3, but, as it is not easy to fix with precision a standard of normality applicable for so extended a period of time, it would perhaps be unsafe to lay great stress on *absolute* differences from the normal.

Table II enables a comparison to be made of *relative* differences: it shows the growth from year to year in H, W, and C for the normal boy, the gymnast, and the scholar; but it should be remembered that a rather stringent selective process takes place in the school at the higher ages as the less studious boys leave and the comparison is then of less value.

Between the ages of  $13\frac{1}{2}$  and  $17\frac{1}{2}$ , for which the table is fairly reliable, the gymnast develops more rapidly than the normal boy, particularly in chest-girth; while the scholar, although developing faster between  $13\frac{1}{2}$  and  $14\frac{1}{2}$ , afterwards tends to slacken his pace and even to fall below the normal rate of growth. This same deterioration will be noticed again presently and an explanation offered.

TABLE I  
TEST OF SIGNIFICANCE OF DIFFERENCES BETWEEN MEAN MEASUREMENTS OF GYMNASIUM BOYS (G),  
SCHOLARS (S), AND NORMAL BOYS (N), OVER A PERIOD OF YEARS

Class.	Age.	No. of Boys.	Mean Height (inches).	Mean Weight (lb.).	Mean Chest-girth (inches).	Height (inches).		Weight (lb.).		Chest-girth (inches).	
						(1) G-N. (2) S-N.	Is Difference Significant?	(1) G-N. (2) S-N.	Is Difference Significant?	(1) G-N. (2) S-N.	Is Difference Significant?
N	11-12	186 to 256	54.99	72.05	25.38		No	(1) - 1.89 ± .89 (2) + 2.31 ± 1.04	No	(1) + .30 ± .13 (2) + .48 ± .16	Poss. Poss.
G		50	54.62	70.16	25.68	(1) - .37 ± .25 (2) + .27 ± .26	No		No		
S		56	55.26	74.36	25.86		No		No		
N	12-13	340 to 391	56.70	77.30	25.93		No	(1) - 1.16 ± .72 (2) + 2.67 ± .77	No	(1) + .40 ± .11 (2) + .60 ± .11	Poss. Poss.
G		86	56.32	76.14	26.33	(1) - .38 ± .22 (2) + .50 ± .21	No		No		
S		127	57.20	79.97	26.53		No		No		
N	13-14	470	58.84	85.47	27.05		Poss.	(1) - 2.04 ± .85 (2) + 2.21 ± .82	No	(1) + .58 ± .11 (2) + .46 ± .11	Poss. Poss.
G		130	57.92	83.43	27.63	(1) - .92 ± .21 (2) + .50 ± .19	No		No		
S		188	59.34	87.68	27.51		Poss.		No		
N	14-15	475	61.08	95.15	28.17		Poss.	(1) - 0.62 ± 1.01 (2) + 3.81 ± .94	No	(1) + .97 ± .13 (2) + .76 ± .12	Yes Yes
G		151	60.45	94.53	28.14	(1) - .63 ± .22 (2) + .70 ± .20	Poss.		No	(1) + 1.32 ± .15 (2) + .79 ± .14	Yes Yes
S		211	61.78	98.96	28.93		Poss.		No		
N	15-16	338	63.40	105.90	29.56		Poss.	(1) + 1.80 ± 1.19 (2) + 3.67 ± 1.09	No	(1) + 1.48 ± .15 (2) + .79 ± .13	Yes Yes
G		149	62.79	107.70	30.88	(1) - .61 ± .24 (2) + .93 ± .21	Poss.		No		
S		190	64.33	109.57	30.35		Poss.		No		
N	16-17	432	65.35	117.90	30.91		No	(1) + 2.53 ± 1.14 (2) + 3.92 ± 1.00	No	(1) + 2.18 ± .17 (2) + .83 ± .15	Yes Yes
G		107	65.09	120.43	32.29	(1) - .26 ± .22 (2) + .74 ± .19	Poss.		No		
S		167	66.09	121.82	31.70		Poss.		No		
N	17-18	187	66.47	124.84	31.85		No	(1) + 6.52 ± 1.41 (2) + 3.26 ± 1.23	Poss.	(1) + 2.00 ± .27 (2) + .72 ± .25	Yes Poss.
G		74	66.51	131.30	34.03	(1) + .04 ± .24 (2) + .83 ± .20	Poss.		Poss.		
S		126	67.30	128.10	32.86		Poss.		Poss.		
N	18-19	89	66.39	127.03	32.41		No	(1) + 7.09 ± 1.92 (2) + 4.31 ± 1.71	Poss.		
G		32	66.66	134.12	34.41	(1) + .27 ± .33 (2) + 1.31 ± .28	Poss.		Poss.		
S		59	67.70	131.34	33.13		Poss.		Poss.		

TABLE II

PERCENTAGE GROWTH IN H, W, C FROM YEAR TO YEAR FOR NORMAL BOY (N), GYMNAST (G), AND SCHOLAR (S).

Growth between years.	H			W			C		
	N	G	S	N	G	S	N	G	S
11½ and 12½ . .	3.1	3.1	3.5	7.3	8.5	7.5	2.2	2.5	2.6
12½ " 13½ . .	3.8	2.8	3.8	10.6	9.6	9.6	4.3	4.9	3.7
13½ " 14½ . .	3.8	4.3	4.1	11.3	13.2	12.9	4.1	5.4	5.2
14½ " 15½ . .	3.8	3.9	4.1	11.3	14.0	10.8	4.9	5.9	4.9
15½ " 16½ . .	3.0	3.6	2.8	11.4	11.5	11.1	4.6	4.9	4.5
16½ " 17½ . .	1.7	2.2	1.8	5.8	9.4	5.2	3.0	5.1	3.0
17½ " 18½ . .	- 0.1	0.2	0.6	1.7	2.1	2.5	1.7	1.1	1.4

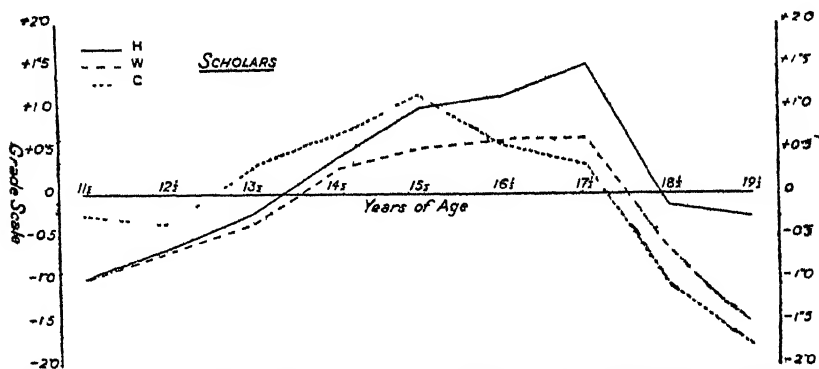
(2) The second test consisted in a comparison of the curves of growth of (a) a smaller but more recent group of able, or mentally accelerated, boys, (b) a similar group of mentally retarded boys, and (c) a group of gymnasts with the growth curve of the average boy.

The point to notice about this method is that we attempt to compare, not two groups of boys all of the same age, but two groups of varying ages. It was necessary, therefore, to devise some means of determining what a normal boy's H, W, or C should be at any given age. For this purpose the actually observed measurements in 1921 and 1923 were combined and parabolas fitted to them by the method of least squares in order to get rid of differences due to random sampling. Charts were then constructed from these curves on the principle that a boy six months in advance of his age in any measurement would be graded as a + 1 boy; if he were a year and a half in advance of his age, he would be graded as + 3; if he were a year behind normal, he would be - 2, and so on.

About 50 boys, conspicuous for their scholarship, who were either at the school at the time of observation or who had only left recently, were chosen. Their cards gave their H, W, and C measurements throughout their school career, in some cases extending from about the age of 11 to 19. Thus it was possible to grade each boy by means of the parabola charts. The mean grades for the group at ages 11½, 12½, . . . 19½ were then found and graphs drawn showing the progress in H, W, and C of the "average" scholar. These were compared with the normal development in the same measurements, which is represented by a horizontal straight line, since the "normal" boy, by definition, is the one who makes normal growth, his grade remaining at the zero or standard level from age to age.

These graphs, it will be observed, neither rise nor fall much beyond one grade (representing six months' normal growth),

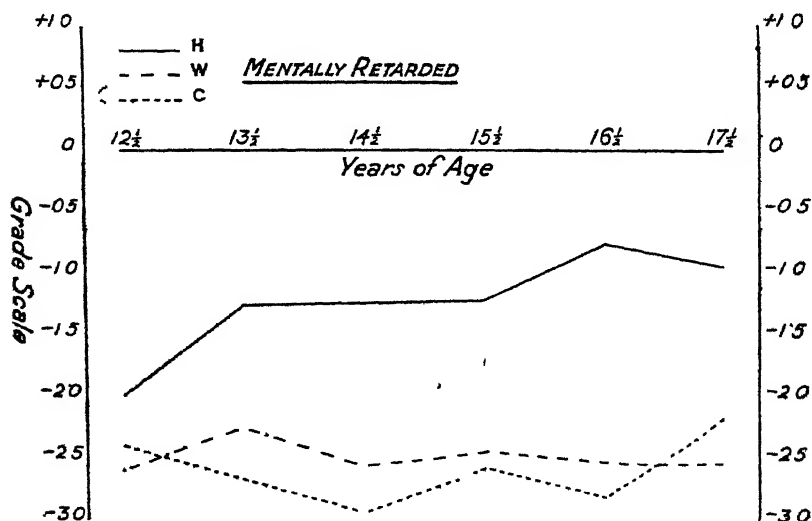
or a grade and a half, from the horizontal level; e.g. the maximum deviation in height is  $1\frac{1}{2}$  grades at age  $17\frac{1}{2}$ , and 1



MEAN GRADING OF SCHOLARS IN HEIGHT, WEIGHT, AND CHEST-GIRTH BETWEEN  $11\frac{1}{2}$  AND  $19\frac{1}{2}$ .

(Each unit of grade represents six months' growth.)

grade in the neighbourhood of the normal for this age corresponds to a difference of about 1.5 cms., so that  $1\frac{1}{2}$  grades repre-

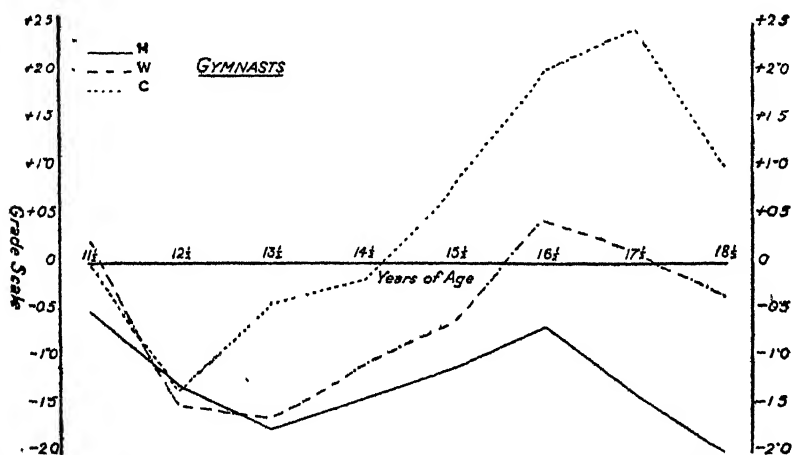


MEAN GRADING OF MENTALLY RETARDED BOYS IN HEIGHT, WEIGHT, AND CHEST-GIRTH BETWEEN  $12\frac{1}{2}$  AND  $17\frac{1}{2}$ .

(Each unit of grade represents six months' growth.)

sents a difference of between 2 and 3 cms. Now the probable error of a mean based on 34 observations, which was the particular number used to determine the grade of height at age

$17\frac{1}{2}$ , is  $0.675\sigma/\sqrt{34}$ , where  $\sigma$  is the standard deviation, in cms., of height measurements for boys of mean age  $17\frac{1}{2}$ ; this gives 0.70 cms. as the probable error. Hence a difference of  $1\frac{1}{2}$  grades is approaching the region of significance. Again, the maximum deviation of weight from the normal—if we omit the extreme ages, as they are based on too few observations to be trustworthy—is a little over half a grade, say 1.2 kgs., at age  $17\frac{1}{2}$ , and, as the probable error here is 0.76 kgs., this is not significant. Also, the deviation of chest-girth from the normal at age  $15\frac{1}{2}$  is less than 1.2 grades, or about 1.5 cms., with a probable error of 0.5 cm., which, like the height measurement, is not very definitely significant.



MEAN GRADING OF GYMNASTS IN HEIGHT, WEIGHT, AND CHEST-GIRTH BETWEEN AGES  $11\frac{1}{2}$  AND  $18\frac{1}{2}$ .

(Each unit of grade represents six months' growth.)

In order to view the same problem from a different angle another group of boys who had recently passed through the school was also tested. They may be regarded as mentally retarded boys, because they had failed to reach a standard fitting them for admission to matriculation classes at the age of 17. They numbered in the central ages from 30 to 35, but the numbers under  $12\frac{1}{2}$  years and over  $17\frac{1}{2}$  were too small to give satisfactory averages. On comparing their graphs with those of the scholarship type the differences appear to be significant. The general impression they give, taken all together, is that the scholar may be a trifle below the normal level in physical development when he first enters the school, but he makes up for this deficiency and becomes somewhat taller, broader-chested, and heavier than the normal boy somewhere

between the ages of  $14\frac{1}{2}$  and  $17\frac{1}{2}$ , *though the difference is never very pronounced*. After that age all three graphs tend down to and below the normal; this decline, if it be true to fact—and general observation of the boys themselves, one gathers, tends to confirm it—may be the result of a nervous strain on the physique under pressure of work for examinations in which the competition is severe and much depends upon the result. The mentally retarded boy, on the other hand, who is not subject to a like examination strain, seems to maintain a fairly consistent level of development, rather below normal, more markedly below in weight and chest-girth than in height.

A similar examination was made of the curves of growth of a group of boys chosen for the gymnasium team during the years 1919–23. The graphs are based on 25 to 30 records in the central ages. It is of interest to note that, while both the mean height and weight in this sample are below normal during practically the whole period of time covered, the chest graph rises well above normal after the age  $14\frac{1}{2}$ , almost certainly as the effect of the gymnastic training. There is a fall in the later ages, as in the scholar-type graphs, possibly assignable to the same cause.

(3) In the two methods so far described the estimate of a boy's ability depended partly upon the judgment of his masters and partly upon his own achievements in school or university. I now pass on to a method which permitted of a finer division in mental attainment. Each year some 250 boys from Manchester Grammar School enter for the Northern Universities' School Certificate Examination, and, by the courtesy of Dr. Crofts, Secretary to the Board, access was obtained to some recent marks sheets. It was thus possible to measure the amount of correlation between examination marks and various bodily measurements.

When the number of observations is large and several cases are to be tested the calculation of the coefficient of correlation by the strict Pearsonian product formula becomes a somewhat laborious task. An approximate formula was therefore adopted which saved much labour and possessed the additional advantage that it was unnecessary to arrange the material in any particular order. It may certainly be taken in this case to give values for the coefficient of correlation close enough to the true values to enable one to judge whether, in view of the associated probable error, one is somewhere in the region of significance or not, which is the chief thing one wishes to know. It may be defined as follows: let  $x$  typify one variable and  $y$  another, and let there be a one to one correspondence between an equal number of values of the two variables. Suppose further that each variable is measured from the arithmetic

TABLE III

CORRELATION BETWEEN DIFFERENT VARIABLES FOR M.G.S. BOYS WHO TOOK THE CERTIFICATE EXAMINATION, 1923

Group.	Coefficient of Correlation between :	Method of Calculation.
100 Boys, age 16-17 . . .	V and H = $0.67 \pm 0.04$ V and W = $0.67 \pm 0.04$ V and C = $0.67 \pm 0.04$	Approx.
100 Boys, age 16-17 . . . 76, age 15-16. . . . . 23, age 14-15. . . . .	H and } = $0.11$ E, F, M } = $-0.10$ Marks } = $-0.33 \pm 0.13$	Approx. K.P. Approx.
100 Boys, age 16-17 . . . 76, age 15-16 . . . . . 23, age 14-15 . . . . .	V and } = $0.12$ E, F, M } = $-0.02$ Marks } = $-0.23$	Approx. K.P. Approx.
16 Classical Boys 'age 16-17, taught by same Master	W and F Marks = $-0.24$ V and F Marks = $-0.05$	K.P.
100 Boys, age 16-17 . . .	W and Marks = $\pm 0.04$ V and Marks = $0.09$	Approx.
23 Boys, age 14-15 . . .	V and H = $0.84 \pm 0.04$ V and W = $0.72 \pm 0.07$ V and C = $0.61 \pm 0.09$	Approx.
100, Boys age 16-17 . . . 76, age 15-16 . . . . . 23, age 14-15 . . . . .	W and } = $\pm 0.03$ E, F, M } = $0.01$ Marks } = $-0.11$	Approx. K.P. Approx.
29 Boys, age 16-17, taught by same Master	W and F Marks = $-0.41 \pm 0.10$ V and F Marks = $-0.15$	Approx.
10 Science Boys age 16-17, taught by same Master	W and E, F, M Marks = $0.14$ V and E, F, M Marks = $0.17$	K.P.
100 Boys, age 16-17 . . .	V/W and Marks = $0.17 \pm 0.07$	Approx.

mean of its own series as origin. If then  $\bar{X}$  denote the mean of all positive  $x$ 's and  $\bar{Y}$  the mean of the correlative  $y$ 's, with corresponding meanings for  $\bar{Y}$  and  $\bar{X}$ , the value of  $r$ , the coefficient of correlation, is given approximately by  $r^2 = YX/\bar{X}\bar{Y}$ .<sup>1</sup>

The approximate formula was first tested by calculating the coefficient of correlation between vital capacity (V) and H, W, and C respectively at ages  $16\frac{1}{2}$  and  $14\frac{1}{2}$ . V is measured on a spirometer by the amount of air an individual can expel from his lungs after a deep breath and is thus an index of the respiratory exchange available in time of great activity. The results, given in Table III, may be compared with those obtained

<sup>1</sup> The reader who is interested will find that this formula may be readily deduced from others given in *Calculus of Observations*, Whittaker and Robinson, p. 330.

by Mumford and Young (*Biometrika*, August 1923) for all boys in the school in 1921, which were as follows :

At age 16-17 ; coefficient of correlation between	.	.	V and H	=	0.68	±	0.03
" " " " "	.	.	V " W	.	0.74	±	0.02
" " " " "	.	.	V " C	"	0.60	±	0.03
At age 14-15 ;	"	"	V " H	.	0.73	±	0.02
" " " " "	.	.	V " W	.	0.69	±	0.02
" " " " "	.	.	V " C	"	0.70	±	0.02

The agreement is in all cases close enough to enable us to conclude that the method is quite satisfactory for our purpose.

The correlation between H, W, and V respectively and the marks gained in examination in English, French, and Mathematics, taken together, were next calculated for boys of the same age, and the Pearsonian formula for  $r$  was used for boys of 15½ as a check on the other. The result was somewhat surprising : in every single case the coefficient came out insignificantly small, sometimes positive and sometimes negative. It was not even worth while working out the probable errors.

It seemed possible that the teaching factor might be cloaking any relationship which might exist between mental attainment and body measurement. In order to discover whether this was likely or not, a number of boys were divided into groups according to the masters who had taught them, and the average marks in each group were calculated, with the following result :

AVERAGE MARKS OUT OF 300 OBTAINED BY GROUPS OF BOYS, AGED 16½, TAUGHT BY DIFFERENT MASTERS

Group No. in Group	Mathematics.						French.				
	A 10	B 18	C 16	D 16	E 19	F 9	P 9	Q 29	R 29	S 23	T 16
Average Marks	182	167	165	158	148	114	191	180	178	152	102

One important cause of these marked differences is the selection which occurs in drafting boys into different classes ; sharp boys are often chosen for advanced classes to be taught by particular masters.

To allow for the teaching factor 29 boys, all taught by the same master, were tested, the correlation between marks and weight, and between marks and vital capacity, being found. In both cases the resulting coefficient was negative, and in the first it was possibly significant though it was not pronounced.

Again, it seemed hardly fair to compare the attainments in French of boys on the Classical side of the school with those

of boys on the Science side. If the 16 Classical boys are separated from the rest of the group the correlation between the French marks and W and V, respectively, is insignificant. The Pearsonian formula for  $r$  was used in this case as an additional check. A similar test was made with ten boys on the Science side, of age 16-17, who had been under the same masters in all three subjects, English, French, and Mathematics, but once more no appreciable correlation was discovered.

Finally, 100 boys of age 16-17 were taken and the marks obtained by each boy in his five best subjects were added together. These totals were then correlated with W and V in turn, but without any more significant result. The correlation of V/W—a "vital index" which finds some favour in America—with marks also proved negligible.

One of two conclusions seems inevitable: either the marks method is unsatisfactory for measuring mental attainment because such attainment is masked by other factors which are difficult to eliminate, or there is very trifling correlation between any of the mental and the physical indices which have been under discussion. The second conclusion receives some support from the other two tests and it is worth noting that such careful observers as Venn and Galton also failed to discover any correlation between success in examinations and like physical measurements. (See *Nature*, March 13, 1890.) Venn, in consequence, made the suggestion at that early date that examinations for public services should be supplemented by tests of physical fitness.

For the data used in this article I am indebted to Dr. Alfred Mumford, Medical Officer of Manchester Grammar School, without whose kind and constant co-operation the labour of analysis would not have been attempted. It may be of interest to add that Dr. Mumford is at present preparing for the press a complete account of the studies of the relation between physique and school attainments which he has been carrying on at the school during the last seventeen years.

# THE SCIENTIFIC RENAISSANCE

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AFTER a thousand years of scientific advance in ancient Greece and Alexandria, there followed a thousand years or more of stagnation. A widespread revival of interest came during the seventeenth century. This revival is usually thought of as comprehended in the work of Galileo (1564-1642) and Newton (1642-1727). Yet there are other factors of equal importance in the awakening that are often overlooked. It will be the purpose of this article to review these other factors and to say something about the character of the men most involved.

During the Middle Ages the authority of the written word had almost completely displaced first-hand observation and experiment in the search for truth. This unquestioning vassalage had to be broken down before the rebirth of science could come. Herein lay the great service of Galileo. In addition to his positive contributions to scientific advance he broke the shackles of mediæval authority. In this he was aided by Kepler, Gilbert, and Harvey in his own generation, and by Cordus and Vesalius in the preceding one. The achievement of these men forms a scientific epic well worth the reading. Although it will not be entered upon here, it should be understood to represent the spade-work without which the scientific renaissance would have been impossible.

## I

Among the factors in the rebirth of science to be discussed in this article, the most important have to do with the work of two men—Francis Bacon (1561-1626) in England and René Descartes (1596-1650) on the Continent. It was not enough that scientific thinkers like Galileo, Kepler, Gilbert, and Harvey should break away from the thralldom of the written word. The popular imagination had to be stirred to a realisation of the meaning and value of science and its methods before the scientific awakening could come. Bacon and Descartes, each emphasising different phases of scientific method—phases, in fact, that seem diametrically opposed—caught the popular fancy

and carried it with them. Francis Bacon put exclusive emphasis upon the inductive method, that is, direct observation of nature and inferences drawn therefrom, as against deductions from the written words of the past. René Descartes emphasised new theories and new deductions as against the old. Each in his own way created a sensation and drew able thinkers in great numbers to the sciences.

Apparently unrelated to his illustrious namesake, Roger, who lived three and one-half centuries before, Francis Bacon was a man of many contradictions. He has stirred up more controversy about himself than any other scientific writer in history. To-day his personal character is both vehemently assailed and defended. He is thought of by some to have written the plays of William Shakespeare, while others treat this claim as preposterous. Some authorities consider him the originator, as he himself claimed, of a new inductive method. Other equally reliable authorities insist that he added nothing whatsoever to the methods of scientific induction laid down by Aristotle. Be the virtue of these various claims and counter-claims what they may—we cannot assess them here—there is one thing certain. Francis Bacon had a remarkably trenchant pen and a matchless facility of expression, and he carried the popular imagination with him in his emphasis on observation and experiment as against blind acceptance of the written word. The following passage accredited to him illustrates admirably the irony and sarcasm he could muster to emphasise his contentions :

“In the year of our Lord 1432, there arose a grievous quarrel among the brethren over the number of teeth in the mouth of a horse. For thirteen days the disputation raged without ceasing. All the ancient books and chronicles were fetched out, and wonderful and ponderous erudition, such as was never before heard of in this region, was made manifest. At the beginning of the fourteenth day, a youthful friar of goodly bearing asked his learned superiors for permission to add a word, and straightway, to the wonderment of the disputants whose deep wisdom he sore vexed, he beseeched them to unbend in a manner coarse and unheard-of, and to look in the open mouth of a horse and find answer to their questionings. At this, their dignity being grievously hurt, they waxed exceedingly wroth ; and, joining in a mighty uproar, they flew upon him and smote him hip and thigh, and cast him out forthwith. For, said they, surely Satan hath tempted this bold neophyte to declare unholy and unheard-of ways of finding truth contrary to all the teachings of the fathers. After many days more of grievous strife the dove of peace sat on the assembly, and they as one man, declaring the problem to be an everlast-

ing mystery because of a grievous dearth of historical and theological evidence thereof, so ordered the same writ down."

Born into the nobility, son of the Lord Chancellor of England and nephew of the Prime Minister, Francis Bacon became acquainted very early with the Machiavellian intrigues of the court life of his day. Precocious as a child and inheriting both intellectual brilliance and courtly self-confidence, he soon conceived of himself as destined to improve man's reasoning faculties, and to this conception he held fast throughout his career, despite the ignominy into which his intrigues drew him. By the time he was fifteen he had already completed a two years' course of study at Trinity College, Cambridge. He then went to Paris, where he spent three years as clerk in the office of the English Embassy. At eighteen he returned to London, the city of his birth, to study law. From law he stepped into politics, where he became engaged in one intrigue after another in his efforts to gain influential political recognition. At first these efforts were unsuccessful, but he eventually secured the post of Attorney-General, after which he was made Lord Keeper of the Great Seal, and finally Lord High Chancellor of England. As Lord High Chancellor he was the foremost judge of the land and the chief member of the King's Council.

Bacon's intrigues brought him not only political preferment. They also brought him political ruin. In 1621, at the height of his public success, he was tried and convicted in the House of Lords on charges of bribery and corruption which he could not deny. He was sentenced to life imprisonment and ordered to pay a fine of £40,000. Although pardoned by the King, Bacon's public activities ended in disgrace and he retired to his literary occupations. In respect to these, however, he had already become known through several publications of scientific interest, especially through the *Novum Organum*, the New Instrument, which sets forth his views on the scientific method as well as on other subjects. Before drawing the curtain on Bacon's unhappy public career, it should be noted that he had been led, as a youth, to believe the key to worldly success lay in intrigue and double-dealing and that he consistently regarded his worldly occupations merely as a means to an end, namely, to provide him with the leisure and income to pursue his literary work to best advantage.

Few have been greater than Francis Bacon in literary achievement. "He is historian, essayist, logician, legal writer, philosophic speculator, writer on science," all in one. But in these things, as in everything else apparently, he is a paradox. He despised the English language and strove to write chiefly in Latin, and yet it is as a great master of the English tongue

that he is remembered. His intuition, his speculative power, his imaginative insight have rarely been surpassed, and yet he discountenanced these faculties ; it was the value of observation, experiment, and carefully-drawn inductions which he laid exclusive emphasis upon in his scientific and philosophic writings. He strove to establish a trustworthy system for gathering scientific truth, and yet he often guessed at scientific facts in a most unscientific way, and failed to keep up with the accomplishments of his own generation. He apparently knew nothing of the advances being made by Harvey, Gilbert, Kepler, and Galileo. His influence was, nevertheless, tremendous and his work of the greatest importance.

How Francis Bacon, in emphasising the meaning and need of observation and experiment in the search for truth, ridiculed the methods of the ecclesiastics and scholastics of his day has been indicated in the passage already quoted. In another passage he caustically says of them :

" Having sharp and strong wits, and abundance of leisure, and small variety of reading, but their wits being shut up in the cells of a few authors (chiefly Aristotle their dictator), as their persons were shut up in the cells of monasteries and colleges, and knowing little history, either of nature or of time, they did out of no great quantity of matter and infinite agitation of wit spin out unto us those laborious webs of learning which are extant in their books."

On the constructive side, his fertile pen described in many ways how scientific endeavour should be organised. The most charming of his sketches along these lines is the *New Atlantis*, in which he pictures an imaginary island, discovered accidentally by a group of sailors lost at sea, on which there was a great temple of learning, a college of science, through which all knowledge was being made manifest. In this temple there were deep caves for carrying on experiments in refrigeration, high turrets for making meteorological observations, a chamber of health for conducting studies in disease, gardens for carrying out experiments in horticulture, operating-rooms for practising vivisection on beasts and birds, perspective-houses for the study of optics, and, finally, an establishment for investigating illusions, magical tricks, spirit manifestations, and the like. This temple of learning was controlled by a group of fellows chosen because of merit. They were of differing rank, each rank exercising different functions. Some travelled and collected material, others compiled knowledge from books, others experimented in the laboratories, others applied theoretical discoveries to practical ends.

Bacon's imaginative temple of learning was fanciful in his own day, yet we have only to look about us now to see his

dream come true. Practically every detail of his *New Atlantis* speculation has become a reality. He urged, among other things, that a nation cannot be truly great unless it sets aside some of its material resources for the endowment of research and exploration. His powerful appeals stimulated public interest and spurred the establishment of learned societies and academies all over Europe.

Francis Bacon gained for science the receptive ear of the general public of England, from which country his influence extended itself to the rest of the world. Eighteen years after his sudden death in 1626 (which occurred as the result of an experiment in refrigeration), René Descartes, the great antagonist of Baconian ideas, was catching the popular fancy in the interests of science in France and Holland.

Descartes was Bacon's junior by thirty-five years. He was thirty when the latter died, and he did not begin to publish his scientific and philosophical conclusions till he was forty-one. Like Bacon, Descartes was of noble lineage and early showed a high degree of mentality. At the age of eight he was sent to a Jesuit school in France, where he quickly mastered all that was offered—that is, the “learned languages and polite literature.” This did not seem to satisfy him, however, and at sixteen he decided he did not wish to study further. Two influences handicapped Descartes at this time. He had a delicate constitution, and an over-indulgent father granted his every whim. His whole upbringing was that of an idle gentleman of wealth and station, and he found it hard to take life seriously. After floundering around from the age of sixteen till he was twenty-one, he became satiated with the idle pleasures of Paris, to which city he had aimlessly betaken himself, and joined the Army.

Soon afterwards an incident occurred which stirred his latent genius. A placard had been posted in the Dutch town of Breda in which he was garrisoned, challenging the world to solve a problem in geometry. A couple of years before, in Paris, two mathematician friends of Descartes had endeavoured to arouse his interest in mathematics without apparent success; but now the problem on the poster intrigued him, and to his surprise he solved it with ease. Before long he had a circle of friends around him in Breda who discussed mathematical and philosophical subjects with him. At the same time he undertook a serious study of mathematics, his military duties at the time being merely nominal.

Two years later, while pondering over a possible relationship between the hitherto separate subjects of algebra and geometry, an idea flashed across his mind which he subsequently developed into a system linking the two subjects together and

marking a distinct step in advance in mathematical reasoning. This has come to be known as the Cartesian system of analytics or co-ordinate geometry. It enables us to represent lines and circles and other curves by means of algebraic equations, and *vice versa*. It is the system which forms the basis of the plots and graphs, of weather or population or other changes, that have become familiar to everyone. We take what are called axes, usually drawn at right angles to each other, and lay off years (let us say) along one axis and population along the other. A curve is then plotted to show population changes. In a similar manner any two variables can be linked together by a graphic curve.

The night this new mathematical idea flashed across his mind, Descartes tells us, he determined to give up his former life and dedicate himself to the development of mathematical reasoning. Three years later he resigned his commission in the Army. After devoting himself for another five years to study and travel, he returned to Paris in 1626 to begin his self-imposed task. Eleven years afterwards his celebrated *Discours* on co-ordinate geometry and other subjects appeared.

In the meantime, he had been working, also, on a philosophical treatise which was to present a new system of the world, but Galileo's fate at the hands of the Roman Inquisition caused publication to be withheld until 1644, when it appeared under the title of *Principia Philosophiæ*. This book, containing many matters of extreme importance for science and philosophy, was immediately put under the ban by the Church, but it nevertheless received wide publicity and created a tremendous public interest. Especially was this true of one of the theories it set forth, namely, the theory of vortices, which associated itself with the Copernican system of astronomy and, illustrating it in a way readily understood by the lay mind, made a powerful appeal to the popular imagination. So enthusiastically was this theory received everywhere, and particularly in his native France, that in 1647 the French Government recognised Descartes's services by voting him a pension.

The theory of vortices was defective in many respects and has long since been discarded. It was at variance even with Kepler's law of elliptical orbits, the truth of which had been clearly demonstrated twenty-six years before. In its broad general outline, however, it paved the way for Laplace's famous nebular hypothesis to explain the origin of the solar system. The important purpose served at the time by the publication of the theory was to focus public attention on new theories as against ancient ones, and it thus hastened the full collapse of the authority of the past.

We may now pause to sum up the essential differences between Descartes's approach to science and that advocated but not practised by Bacon. Descartes's theories of analytic geometry and of vortices were due more to that flash of insight which genius exhibits than to anything else. Bacon's inductive method, therefore, tells only half the story of constructive scientific procedure. Imaginative insight is quite as necessary to scientific advance as is observation and experiment. Yet when it comes to determining which of Descartes's two theories is true and which false, Bacon's method provides the only test. The theory of vortices did not accord with the facts and was, therefore, discarded. The theory of co-ordinate geometry has been found useful to an understanding of physical phenomena, for which reason it has been retained.

The complete scientific method includes not only unbiased observation and repeated experiment, but also the setting up of carefully-drawn hypotheses or theories to account for the facts observed. Such hypotheses are regarded as provisional and are not accepted as proved until tested time and again by additional observations and experiment. Yet proved theories form the basis of scientific generalisations, and without these there could be no progress in science. The great virtue of the inductive test is to separate the true from the false in our theorising, and the virtue of scientific theories is to explain the observed facts. Both hold a place of importance in the completed scientific method. And the argument as to which is the more essential is just as futile as the argument about the relative importance of a good stomach and good food.

Primitive man mixed up his dream fancies with his waking experiences, and no adequate way was found to separate the true from the false in man's thinking till the Greeks discovered the inductive test. The Middle Ages brought back primitive conditions by submerging observation and experiment. The mixing up of true and false took on a new impetus. The mistakes of the past continued to be perpetuated. Nothing could have been of greater importance in the seventeenth century than the resurrection of the inductive approach. Therein lay the value of Francis Bacon's critical writings. But the pendulum would undoubtedly have swung to the opposite extreme, discouraging all forms of theorising, had it not been for the contribution of Descartes. We have here again the old contrast the Greeks encountered, intuition over against observation, the imagination versus experiment, theories opposed to facts, deduction and induction. Descartes demonstrated the one; Francis Bacon emphasised the other. Neither appreciated the dual and complementary character of scientific method. And with the breakdown of the authority vested in

the written word and with the popularisation of science and its methods, the conflict between these two viewpoints began anew and lasted over a considerable period. Even in our own day each phase of scientific procedure has its exclusive advocates, although it is now quite generally recognised that the one supplements the other and that alone each is incomplete.

## II

So much for the dual character of scientific method and for the contributions of Bacon and Descartes in bringing the significance of science to the people. There is still another phase of the work of these two men, as bearing upon the scientific renaissance, which deserves emphasis. This pertains to the influence their writings had upon the least-established of the fields of scientific inquiry, upon that branch which attempts to study mental as distinguished from physical phenomena, that is, the field of psychology.

In bringing to the fore again the old controversy waged between Plato and Aristotle as to the relative importance of intuition and observation, Descartes and Bacon opened wide the door to closely-related metaphysical speculations previously kept closed through the power of ecclesiasticism. In man's new-found mental freedom he attempted once more to solve the main riddles of the universe—the existence of God and the soul, the freedom of the will, the relative importance of the outer and the inner worlds—believing that many problems related to the inner life of the mind which have since been placed outside the pale of scientific inquiry, could be studied by the exact methods of science. It was not appreciated then that these insistent metaphysical questions—though of ultimate concern to every rational individual—are beyond the scope of scientific investigation. With the shackles of mediæval despotism removed, Bacon and Descartes and their followers tackled without discrimination the whole gamut of metaphysical problems, even as the ancient Greeks had tackled them—fearlessly and honestly. And the same differences in personality and interests which led Bacon and Descartes to stand at opposite poles in their treatment of scientific method caused them to found opposing schools of metaphysics and mental philosophy.

As in astronomy, the revolt from mediæval authority in metaphysics started in Italy, where it led to the martyrdom of Bruno, who not only supported the Copernican theory but broke away from current theological dogmas as well. He was followed by the Italian Campanella, who laid emphasis upon certain basic psychological facts; but the vigorous reprisals

of the Roman Inquisition soon stilled Italian metaphysical and psychological utterance. Meanwhile in England, the seventeenth century opened with the virulent attacks of Francis Bacon, not only on all forms of speculation—scientific, ecclesiastical, and metaphysical alike—but on many other current modes of thinking. Bacon demonstrated, among other things, that the human mind has a tendency to accept many statements as proved even though no proof whatsoever has been submitted. He pointed out that people have numerous prejudices which they have never analysed—prejudices handed down from one generation to the next, and unquestioningly adhered to; prejudices accepted because of personal idiosyncrasy; prejudices that we get from public addresses or from our friends; and prejudices arising out of dogmatic statements which we accept by virtue of the fact that they have been dogmatically made and for no other reason. These prepossessions haunt the minds of all people, Bacon explained, and hamper us in our attempts to think rationally. We must purge ourselves of our prejudices before we can hope to think scientifically in all respects.

Modern psychology has but served to verify Francis Bacon's analysis. It has pointed out, in addition, that we engage in much day-dreaming, make many snap judgments, easily find excuses for holding fast to our prepossessions, and attempt critical thinking very seldom. The formula that will purge us from prejudice and enable us to think scientifically most of the time has not yet been found.

A number of clear thinkers in Great Britain followed Bacon's critical analysis of the human mind. Among the outstanding exponents of the English school of thought were Hobbes, Locke, and Berkeley in the seventeenth century and Hume in the eighteenth. It would be going too far afield to enter into the various speculations of the Bacon-Hume school—for speculate these men did, despite their belief that they were following the inductive method pure and simple, as one significant fact will serve to demonstrate. The same reasoning which led Thomas Hobbes, a young contemporary of Bacon, to a materialistic conclusion in his analysis of the inner and outer worlds, led Bishop Berkeley a half-century later to a spiritualistic one. In the interim, John Locke had taken a position somewhere in between, and in the next century David Hume once more attempted to find a middle ground on this vexing question of the relative importance of mind and matter.

Contemporaneously with Hobbes, René Descartes in France was throwing his full genius into the scale of speculative theorising, basing his contentions on mathematical grounds and insisting that by introspection the great truths of metaphysics

and psychology can be deduced. His invention of analytical geometry had given him a supreme confidence in intuition and introspection for understanding reality. His famous dictum, "I think, therefore I am" (*cogito, ergo sum*), has echoed and re-echoed from his day to our own. He postulated what he felt were three fundamental hypotheses for metaphysics: first, that the ego exists; second, that God exists; and third, that the corporeal world exists. Out of these hypotheses he evolved a dualism of mind versus matter, and in doing so rendered an important service to psychology. But at the same time he rendered her a disservice also, by maintaining that the scientific methods of investigating matter must differ absolutely from those used to investigate the mind. He thus became the founder of the so-called introspective school in psychology, which holds that introspection is the distinctive and only method for obtaining and analysing psychological facts.

Following Descartes, Baruch Spinoza in Holland indicated many ways in which the dual elements of mind and matter are intimately correlated and yet distinct. And a decade or two after this, about 1684, Gottfried Leibnitz, the great German scientist and metaphysician, reasoned that there is nothing contradictory in assuming that one and the same "essence" can be both mind and matter in different manifestations. Leibnitz became the precursor of the spiritualistic school of metaphysicians, as opposed to the materialism of Hobbes, by hypothesising that matter in its essence does not consist of deadness and inertia but of energy and force, an assumption quite generally borne out by modern advances in physics but having no known facts to back it up in the seventeenth century. In the eighteenth century Emmanuel Kant carried the Descartes-Leibnitz reasoning much further along.

The metaphysical speculations of Leibnitz resulted in 1690 in the publication of John Locke's famous *Essay Concerning Human Understanding*, in which he took sharp issue with his German contemporary. Building upon what Bacon and Hobbes had started, Locke insisted that all this speculation about the soul, material "essences," and the like was so much folderol, that all knowledge comes to us from the outside world by observation and sensation, and that there is no innate knowledge whatsoever. It was the same battle over again between speculation and observation, with this difference: concurrent advances in physics and mathematics, especially those having to do with sound and light, had given a new meaning to the superficial psychological facts then available regarding sensation, and Locke and Leibnitz and their adherents made use of these new discoveries.

The significant point for us here is that, despite this help

from physics, the essential character of the metaphysical controversy beginning with Bacon and Descartes and continuing for two centuries, remained unchanged. Bearing in mind what was said earlier in this article regarding the mutual and complementary character of speculation and observation, of deduction and induction, it is apparent that neither the Descartes-Leibnitz-Kant school nor the Bacon-Locke-Hume school had the whole truth on its side. The contributions of the one lay in pointing out the need for common-sense hypotheses in psychology as in other sciences, and in emphasising the obvious dualism of mind versus matter as a starting-point for psychological investigation. Its shortcomings lay in its insistence upon introspection as the only psychological method, which meant that this science must secure its facts in ways different from those of other sciences, and in its failure to appreciate the value of observation and experiment for purposes of separating useless and futile speculations from those that can be utilised as tentative hypotheses for further psychological fact-gathering.

The chief contribution of the English school lay in insisting upon observational and experimental methods, which insistence led finally to the abandonment of untrammelled speculation in psychology. Its shortcomings lay in its inability to appreciate the value of carefully-drawn hypotheses for scientific purposes and in its use of metaphysical theorising, despite its protests to the contrary. Its attempts to explain mind away in terms of matter (Hobbes) and matter away in terms of mind (Berkeley) and its efforts to determine whether we can really know anything or not were just as futile metaphysical excursions as the theorising of the school it opposed.

The real difficulty of seventeenth- and eighteenth-century psychology was appreciated by neither school, namely, that there was too much speculation with too few facts to go on, which facts, in addition, were exceedingly superficial. Advance for psychology lay in reducing the speculative theories to an irreducible minimum—a minimum on which all workers could agree—and in collecting more and better psychological facts. Yet it must not be overlooked that it was just such discussions as Locke and Leibnitz engaged in, wholly speculative and largely futile as far as psychology proper was concerned, which led to a clearing of the atmosphere, a separation of metaphysical from psychological interests, and a defining of the scope of the new science.

Out of the smoke of argument and counter-argument lasting for another century after Locke, it ultimately became clear that psychology must rest her claims to be called a science on less pretentious grounds than metaphysical ones. Once more the main riddles of the universe had to be put aside.

Not that this renders these vital questions one whit less important or insistent. It simply emphasises that, as yet constituted, scientific knowledge is unable to encompass them. First, the demonstration of the existence of God or the soul had to be excluded from psychological consideration. Then the scientific proof of an "essence," linking mind and body together and making them one, was likewise set aside. And, finally, it came to be appreciated that to try to argue matter away in terms of mind or mind away in terms of matter was equally futile for psychology and that the validity of our knowledge must likewise be taken for granted. Not until psychology settled down to accepting the dualism of mind and body as a working hypothesis and turned over to metaphysics the other questions mentioned, could it proceed to build upon a secure and acceptable basis. And yet even to-day there are psychologists who persist in confusing psychological with metaphysical issues, not content apparently with the more modest hypotheses underlying modern psychology.

With respect to the meagreness and superficiality of seventeenth- and eighteenth-century psychological facts, two points will bear emphasis: one is that the Cartesian insistence on introspection as the distinctive method for psychological fact-gathering served for a considerable period to prevent psychologists from looking in the right direction for further facts. The other concerns itself with the intimate relation between psychological and biological facts. The biological data upon which modern psychology depends were not available in the seventeenth and eighteenth centuries. In the interim, clearing the atmosphere of metaphysical presuppositions was a most necessary first step to later constructive advance.

### III

Biological and geological progress in the seventeenth century should also be regarded as part of the scientific renaissance. Cordus, Vesalius, and Harvey had broken away from the authority of the ancients in biology, and physics had advanced to the point where the microscope could be made use of both by biologists and geologists. Many of the mathematical physicists of Galileo's day and immediately following worked on the problems of optics, and by the middle of the seventeenth century lens makers arose in England and on the Continent who supplied usable microscopes and telescopes. Just as the latter stimulated astronomical progress, so the former stimulated biological and geological progress.

In 1665 a remarkable book, entitled the *Micrographia*, was published by the versatile naturalist and physicist Robert

Hooke, in the preface of which the value of the microscope to the naturalist is expressed thus :

"The next care to be taken, in respect to the senses, is a supplying of their infirmities with instruments, and, as it were, the adding of artificial organs to the natural ; this for one of the senses has been of late years accomplished with prodigious benefit to all sorts of useful knowledge, by the invention of optical glasses. . . . It seems not improbable, but that by these helps the subtlety of the composition of bodies, the structure of their parts, the various texture of their matter, the instruments and manner of their inward motions, and all the other possible appearances of things, may come to be more fully discovered."

Hooke's book is replete with illustrations of microscopic examinations of animals and plants and makes mention for the first time of those "little boxes or cells" which were later to be recognised as among the foundation facts of scientific biology. Two contemporaries of Hooke, Leeuwenhoek and Swammerdam, opened up the "world of the infinitely small" with their microscopes as Galileo had opened up the heavens with his telescope. The work of these two Hollanders was of outstanding importance. The microscopic school, which they represent, created an epoch for biological research.

The greatest biological thinker of the renaissance period, however, was Marcello Malpighi of Bologna (1628-1694), a most remarkable genius. Malpighi developed many branches of biological study previously non-existent. At seventeen he was already amusing himself with the microscope. At twenty-one he entered the University of Bologna to study medicine. Later he was Professor of Medicine at the Universities of Pisa, Messina, and Bologna. He served his native university for over twenty-five years, until in failing health he withdrew to Rome to become private physician to Pope Innocent XII. Upon Malpighi's death three years later, his writings were carefully preserved and published.

Malpighi's discoveries in human physiology are on a par with those of his illustrious predecessor, Harvey, and in a way completed them. Six years before the latter's death, Malpighi discovered, and Leeuwenhoek later amplified the discovery, that the blood circulates not only by way of the arteries and veins, but likewise by means of fine, hair-like tubes or capillaries connecting the arteries and veins at various points and now known to extend to the minutest parts of the body. Without the microscope these hair-like tubes could not have been discovered, nor could the blood corpuscles, which Malpighi discerned several years afterwards. Malpighi was among the first to use the microscope with scientific effect in the study

of animal and vegetable structures. He combined such study with his practice of vivisection with striking results. In this way he analysed the structure of the lungs, the kidneys, the spleen, the skin, and the brain. Many paths opened out from Malpighi's work, among them being such specialised fields known to-day as classification, histology, plant physiology, embryology, and genetics. He did pioneer work in geology also, which at this time, however, had not as yet been set apart as a separate branch of scientific inquiry.

But although Malpighi, Leeuwenhoek, Swammerdam, and others during this period were making notable biological advances by means of the microscope, the basic facts of anatomy, physiology, and embryology still remained to be worked out. Until they were so many false theories held the field that a proper scientific perspective in biological research was impossible. Malpighi himself, though he paved the way for important later discoveries, held false theories concerning the origin of life and the structure of animal tissues. Regarding the former, many theories, fantastic to us with our wider knowledge, were being put forth. Highly developed vertebrates, such as eels, were conceived of as spontaneously generated out of the void or out of mud, and fossils were still being thought of as the remains of half-formed beings, uncompleted, much as though the Creator got tired while He was fashioning them and left them only partly finished. Redi and Steno, contemporaries of Malpighi, made noteworthy attempts to dispel these false notions; but Steno's pamphlet on geological formations published in 1669, the most important geological document of that century, received no attention till many years later, and Redi's pioneer experiments to show that maggots are not spontaneously generated out of decaying meat, but are produced from flies' eggs deposited on the meat, proved inconclusive. Only within recent years has the last of these misconceptions been thrown into the discard. In Malpighi's day, too few microscopic facts had been brought to light, and practically nothing was known about the important foundation-subjects of organic and inorganic chemistry to dispel the false theories thrown about biology and geology.

In this respect the observation must be repeated which was made regarding the psychological development of the renaissance period. Just as the complete clearing of the atmosphere in psychology had to await the firm establishment of biology, certain fundamentals in physics and chemistry had to be established before the false theories of the fields of nature study could be swept away. The revolutionising effect on biology of the application of the microscope (a product of physics) has already been indicated. Other advances in

physics and chemistry had to come before biology and geology could reach the point of laying the foundation-stones of their respective sciences. The fact-gathering and exploration of the world of the microscope were being carried on as necessary preliminaries.

#### IV

The relation of the scientific renaissance to chemistry and physics was even more important than that pertaining to the fields already reviewed. Chemistry had not yet become a distinct department of scientific inquiry in the seventeenth century, but the foundation-stones of physics were ready to be laid when Galileo had completed his epoch-making researches in mechanics. Descartes's invention of analytic geometry was likewise important for advances in physics. Newton, after the middle of the seventeenth century, put physics upon a scientific basis. Somewhat earlier other scientists interested in borderline problems involving both chemistry and physics were doing constructive pioneer work in clearing away misconceptions surrounding these fields. This work will be reviewed here.

At the beginning of the seventeenth century many false notions of age-old standing were still clinging to those intangible phases of physical and chemical phenomena having to do with the atmosphere, with heat, with light, with sound, and with electricity and magnetism. Galileo and Gilbert had exposed some of the erroneous beliefs bound up in astrology and alchemy, but many others were rife and some continued for a century or more. Prominent at this time among the pioneers who struck out boldly against age-old superstition was a young English contemporary of Galileo, the Honourable Robert Boyle (1627-91).

Born in Ireland a year after the death of Francis Bacon and fifteen years before Galileo died, Robert Boyle, the seventh son of the Earl of Cork, was favoured both by heritage and environment. Robert was precocious as a child and highly susceptible to suggestion, as is evidenced by his picking up the habit of stuttering from some of his playmates, a habit which proved to be unbreakable later. When eight years old he was sent to England to be educated at Eton, where he remained four years. By the age of twelve he had mastered a considerable portion of Latin, French, and mathematics, and was engaged in writing verse and studying the Scriptures. No signs of scientific predilection had manifested themselves at this time. His emotional susceptibility was in fact inclining him towards the ministry and he remained devoutly religious

throughout life. In 1638 he began a six-year trip with a brother, visiting and studying in Holland, France, Switzerland, and Italy. He arrived in Italy at the time of the passing of Galileo and made a study of his life and works. This impressed him profoundly and probably did more than anything else to start him on a scientific career.

Robert Boyle was seventeen when he returned home in 1644, following the death of his father (his mother having died when he was a child of three), and found himself heir to a considerable estate. The next ten years was an unsettled period for him, although by no means an unprofitable one. Most of his time was divided between his Irish estates in Ulster and the home of a sister who lived in London. England was in a political turmoil; but a small group, uninterested in politics, had banded themselves together in London to discuss philosophic and scientific problems, and Boyle spent many pleasant and profitable hours attending these gatherings. When in 1654 he moved to Oxford to equip a laboratory and settle down to serious experimental work, he and others continued these informal gatherings there. Soon their importance became generally recognised, and in 1660, through the aid of King Charles of England, the Royal Society for the Advancement of Learning was officially created. To-day the highest distinction a scientist can receive in England is to be elected a Fellow of the Royal Society and to be allowed to place "F.R.S." after his name.

Boyle was a member of the Council of the Royal Society from its inception, and most of the meetings which preceded its formation and for some time afterwards were held in his laboratory at Oxford. These meetings brought together the most eminent English scientists of the day, and many important problems came up for discussion.

As a result of these meetings, Boyle became particularly interested in the current practices and claims of the alchemists on the one hand, and on the other in experiments with the atmosphere and its relation to pressure and suction being carried on in Italy, France, and Germany.

In 1661 he published an important work entitled *The Sceptical Chymist*, in which he severely criticised the alchemists and urged that chemistry be studied for its own sake. Up to that time chemical phenomena had been studied chiefly as an aid to the practice of alchemy and medicine. In his book Boyle questions the value of the practices of the alchemists. He complains that they play with words, that their writings are intolerably ambiguous, and that they deliberately obscure their meanings. Continuing, he writes:

"If judicious men skilled in chymical affairs shall agree

to write clearly and plainly of them, and thereby keep men from being stunned, as it were, or imposed upon by dark and empty words ; 'tis to be hoped that these men [the alchemists] finding that they can no longer write impertinently and absurdly, without being laughed at for doing so, will be reduced either to write nothing, or books that may teach us something, and not rob men, as formerly, of invaluable time ; and so ceasing to trouble the world with riddles or impertinencies, we shall either by their books receive an advantage, or by their silence escape an inconvenience."

The alchemists had had things largely their own way. They (as well as the astrologers) had taken the so-called " elements " of the ancient Greeks—earth, air, fire, and water—and had built upon them a mysterious " system " through which they claimed base metals could be transmuted into gold and an elixir of youth could be compounded. In these latter days we have actually succeeded in transmuting one metal into another and by gland transplantation and other means have made the aged young again, so that we cannot say that the dreams of the alchemists were entirely fantastic. It was not their dreams which were misleading, but their practices. Leaving out of account those who practised as charlatans and fattened on tribute from their dupes, those alchemists who were in earnest were not going at things in a scientific way. They accepted the written words of previous alchemists and by various processes of hocus-pocus hoped to hit upon the objects of their search. Boyle's book discredited this whole procedure and urged a new approach.

He insisted, among other things, that the alchemists' use of the term " element " was misleading, and that it should not be applied to compounds of more elementary substances, but only to " certain primitive and simple, or perfectly unmingled bodies ; which, not being made of any other bodies or of one another," are the ingredients out of which compounds are made and " into which they are ultimately resolved." Such a distinction between chemical elements and their compounds, first proposed by Boyle in 1661, is the distinction still made to-day. It is one that has to be firmly grasped if modern chemistry is to be understood.

To-day we know that none of the four things labelled " elements " by the ancient Greeks are such in any real sense. Water is a compound of the two elements hydrogen and oxygen ; the earth and the air are composed of many elements ; while fire is neither an element nor a compound, but a misleading phenomenon which accompanies combustion and which was not understood until comparatively recent years. In Boyle's day fire was thought of as material in character (which

it is not), whereas the material nature of the air had not yet been fully recognised.

Prior to Galileo, practically nothing was known about the atmosphere, the prevailing ignorance being summed up in the dictum laid down by Aristotle and then still held that nature "abhors a vacuum" and quite horrified at seeing she causes explosions and other noises in her haste to fill empty spaces with air. This crude notion served to explain in part many otherwise inexplicable happenings. If the air, for example, is sucked out of a vessel having only one small opening, no way being provided for other air to get in and the walls of the vessel being fairly thin, why should the walls collapse with a sudden bang unless nature were disturbed about the matter? It was not realised that air is material and has weight and, therefore, exerts a considerable pressure against anything with which it is in contact. The air in a vessel exerts a pressure against the inside walls which counteracts the pressure from without. Remove the internal pressure of the air by withdrawing it, and the walls may be unable to sustain the outside pressure. With the actual causes unknown, the effect of a vessel's collapse under circumstances as just described must have been startling.

Galileo had satisfied himself that the air has weight by weighing a vessel filled with it and then forcing in additional air and weighing again. Two of his pupils (Torricelli and Viviani) went further and demonstrated that, under suitable conditions, the pressure of the atmosphere will actually hold up or sustain a column of mercury. To understand the Torricelli experiment, as it is still called to-day, one need only picture an inverted glass test-tube suspended over a dish of mercury with the open end of the tube just immersed in the mercury and with the air in the tube removed (by some means we need not go into). The pressure of the air on the mercury in the dish will force the mercury up into the inverted test-tube for a certain distance, there being no air pressure in the tube to counteract and prevent this.

Pascal, a young French genius, took the next step. He argued that if Torricelli's conclusions about the air exerting pressure are correct, the height of his mercury column should grow less as one ascends high altitudes, where the air is rarer and therefore must exert less pressure. Arranging that a Torricelli tube be taken to the top of a mountain, Pascal found that the mercury column dropped three inches in the ascent, thus giving further force to Torricelli's experiment and inaugurating the use of barometers for measuring atmospheric pressure. Torricelli's tube constituted the first barometer.

Meanwhile in Germany even more surprising results of ex-

periments on pressure were forthcoming. A German scientist by the name of Guericke had tried to draw water from a tightly sealed cask, but he could not prevent air from getting in. Then he substituted a copper sphere for the cask, with the result that after he got most of the water out, the sphere collapsed "with a loud clap and to the terror of all." He finally did succeed with heavier material in securing a partial vacuum and carried out experiments under varying air pressures. Some of his results were: "A clock in a vacuum cannot be heard to strike; a flame dies out in it; a bird opens its bill wide, struggles for air, and dies; fishes perish; grapes can be preserved six months in vacuo." Guericke then proceeded to show how great the pressure of the air is by performing his famous experiment of the Magdeburg hemispheres, which were hollow metal shells about a foot in diameter. After putting the hemispheres together and exhausting the air in the hollow interior, he demonstrated before Emperor Ferdinand of Germany and the Reichstag in 1654 that it required the combined strength of sixteen horses, eight pulling on one hemisphere and eight on the other, to separate them. The hemispheres were held together only by the pressure of the air on the outside.

This spectacular experiment in air pressure occurred in Germany the year Robert Boyle at the age of twenty-seven settled in Oxford, established a laboratory, and employed Robert Hooke as his assistant. Hooke was himself to become famous as a scientist, and we have already had occasion to mention his name in connection with the microscopist school of biologists and the publication of his *Micrographia*. This book appeared eleven years after he began work as Boyle's assistant.

Robert Boyle had followed with considerable interest the experiments on atmospheric behaviour conducted by his contemporaries in Germany, Italy, and France; and with the assistance of Hooke he determined to go further. To begin with, he perfected an air pump along the lines of Guericke's, but much more efficient, which enabled him to experiment with ease at pressures both less and greater than that of free atmosphere. He then verified a number of Guericke's results, showing that sound will not travel in a vacuum, that combustion cannot proceed without air, and that animals, birds, and fish cannot live without air. He also demonstrated that, although sound does not travel in a vacuum, both electricity and magnetism do, a distinction which proved later to be of the greatest importance for the science of physics. He performed many other interesting experiments which we cannot go into here. To those interested further his books provide

both profit and entertainment, for he on occasion indulges in exquisite sarcasm in speaking of nature's supposed aversion for a vacuum and of other crude conceptions current in his day.

The most important of Boyle's discoveries was the formulation of the law which bears his name, the law indicating the relation of gas pressure to volume. By means of a series of carefully planned experiments he found that at a constant temperature the volume of a gas such as air is decreased in direct proportion as the pressure applied to it is increased, and *vice versa*. For example, if the pressure applied to a gas is doubled, its volume is decreased 50 per cent.; if the pressure is cut in half, the volume is doubled, and so on. If  $v$  is taken to mean volume and  $p$  pressure, the following equation expresses this relationship:  $p = \frac{k}{v}$ , where

$k$  is a constant which remains fixed for a given gas but has different values for different gases. Expressed otherwise, the equation reads:  $pv = k$ . It is to be remembered that this relationship holds only when the gas is kept at a constant temperature. Later it was discovered by another physicist that if the pressure is kept uniform, a constant ratio is found to exist between temperature and volume. As temperature increases, the volume expands in direct ratio, and *vice versa*. Both these laws are now known to hold only for ordinary temperatures and pressures.

Boyle's law has proved to be of supreme scientific importance, both for chemistry and physics. It has been found by experiment to apply not only to air, with which Boyle experimented, but to nearly all other gases. In Boyle's day many matters pertaining to gases were undiscovered. Air was the only gas known, and this, as is now well understood, is itself a mixture of a number of gases. There are also other gases not found in ordinary air.

Boyle's painstaking experiments brought universal recognition. They gave added meaning to the inductive method emphasised by Francis Bacon and helped prevent the swing towards Cartesianism and speculation from going too far. Boyle, a humble and devoutly religious man, declined all the honours that a grateful contemporary world would heap upon him. Not that he failed to arouse opposition—many hailed the results of his experiments as destructive of religion and as undermining the authority of the universities. Physical science, however, had become popularised. Its benefits were being recognised. And the violent and destructive criticism of even a generation before had been greatly diminished. Boyle had a delicate constitution and during his later years was frequently

ill, but his resources were plentiful and enabled him to live a protected life. He took great delight in his work. As he said : " In my laboratory I find that water of Lethe which causes me to forget everything but the joy of making experiments."

## V

What the scientific renaissance of the seventeenth century accomplished may now be briefly summarised. In the first place, science and its methods became popularised. Direct observation of nature and careful experiment took on a scope theretofore undreamed of. But just as it happened on a smaller scale in Aristotle's day, speculative theories, once the rebirth of scientific inquiry came, were advanced on every hand regardless of whether they were carefully considered or not. Nor did Bacon and Descartes and their contemporaries and followers appreciate that the foundation-stones for much of their theorising had not yet been laid. The methods of science were becoming more fully understood, but that there was a sequence to the unfoldment of scientific knowledge no one even vaguely surmised. It was not known that a formulation of the fundamental laws of physics had to precede the founding of chemistry as a science, that an understanding of chemical principles had to come before geology and biology could be securely rooted, and that biology in turn is fundamental to psychology. It was only natural, therefore, that many false theories should be put forth to account for chemical, geological, biological, and psychological phenomena, at the same time that the foundation-stones of physics (the next in the sequence after mathematics and astronomy) were being securely laid. And, of course, the new false theories had to be broken down later, just as the false theories of the ancients were broken down, except that the process had become easier because of the wider adherence to the inductive method of testing theories by careful observation and experiment.

In the second place, during this period physics was being placed upon a solid basis at the hands of the incomparable Newton. This achievement—the fullest expression of the scientific renaissance—constitutes another scientific epic which cannot be entered into here. Newton's achievement is so extensive in character that a separate article would be required to do it justice. Thirdly, the groundwork was being prepared by Boyle and others for the later establishment of chemistry. Fourthly, the invention of the microscope in physics was opening up a new era for geological and biological advance, so that after further assistance from physics and chemistry, geology and biology could emerge as exact sciences. And,

lastly, the relationship between psychology and metaphysics was being thrashed out, the resultant clarifying discussion continuing in a highly speculative way until, the physiology and anatomy of the brain and nervous system made clear by biological advance, the foundation-stones of modern psychology could finally be laid down.

Before the scientific renaissance, mathematics and astronomy had been placed upon a scientific basis and the authority of the past had been broken. With the rebirth of scientific interest, the way was being prepared by the pioneers we have been considering in this article for the work of Newton and his followers, the establishment of physics as a science, and the inauguration of the modern era.

# POPULAR SCIENCE

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## METHODS OF MAKING MAPS

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STRICTLY speaking, a small hilly country cannot be represented properly on a flat map, for the flatness of the latter is in striking contrast to the uneven surface of the Earth. To some extent the difficulty is overcome by the conventional colourings and shadings, but such devices are, at best, only artifices. The question is not really a serious one, for variations in altitude are but matters of feet, while those of distance are matters of miles, and so the ratio of the vertical to the horizontal is small.

In the case of a large hilly country the horizontal distances are increased, and the relative error is still less. But in the case of maps of very large countries or of continents another kind of difficulty arises in an acute form, though it is also present to a negligible extent even in the map of a single parish—we refer, of course, to the rotundity of the Earth. This problem becomes a very complex one when we endeavour to transfer the whole world, or half of it, from the surface of a globe to a plane, and it is primarily with this aspect of the question that this article deals.

The difficulty may be illustrated by attempting to flatten out a large piece of orange peel or a rubber ball; it is obvious immediately that such a task is impossible. There has to be stretching or cutting, or perhaps both, and distortion occurs.

If we take a large sheet of paper and paste on it maps of all the different countries of the world, a "solution" which erroneously suggests itself, gross inaccuracies will occur in our representation of the oceans.

If, on the other hand, we return to the former idea and imagine the world to be drawn correctly on a rubber sheath

fitting over a globe, the covering may be slit along a meridian and removed, and then stretched out to form a square or rectangle, a process possible with rubber though not with the Earth, but the north and south parts of the map will be gravely distorted and will convey to the uninitiated totally inadequate, and even false, impressions. On such a map all lines of latitude will be of equal length, and the Poles themselves, mere points of latitude 90 degrees, will become as long as the Equator.

Yet, unless some such device is adopted, the task is hopeless, and so, in practical life, we have to make the best use we can of what may truly be called "artificial" maps. Convention may rule that a map may be a good representation; it can never be, in any sense, a reproduction.

The most important features which have to be embodied in maps may be classified in three main categories:

1. Exactness of shape,
2. Exactness of area, and
3. Exactness of relative positions.

One of these three features must always be ignored, not infrequently two are neglected, and the decision as to which shall be regarded as the essential feature in any given case depends entirely upon the use to which it is intended the map shall be put. For nautical purposes, for instance, accuracy of relative positions is vital; given this, a captain could navigate his boat from Tilbury to Calcutta successfully, even if his chart showed Greenland larger than the United States of America. On the other hand, there are many statistical purposes for which it is necessary to have maps on which areas are drawn accurately, and a list of this kind might be extended indefinitely. To procure these different kinds of maps different methods have to be adopted, and each of the resulting charts is called a particular type of "projection."

To obtain a description of these projections which can be understood readily by the average general student seems to be a matter of the greatest difficulty, for the published diagrams invariably attempt to show only one plane, and the reader has to visualise, as best he can, the three-dimensional models some cross-section of which they claim to represent. It is true that these one-plane figures are very easily drawn and are frequently used, but it is equally true that they are rarely understood because the model is so difficult to reconstruct from them, whilst for younger or inexperienced students they are notoriously unsuited. Now, most people agree with Dr. T. P. Nunn in his remark that the power to think "freely,"

*i.e.* without models, varies greatly with the maturity of the mind and with the familiarity of the subject-matter, and most will probably also agree that the topic under discussion in this article is a good illustration of his subsequent argument for the use of models in teaching.<sup>1</sup> And, as he further points out, "even educated persons of good intelligence can 'see' difficult ideas much more easily when they are presented in concrete symbolism, and there have been minds of the highest order that could work in no other way, *e.g.* the great Lord Kelvin, who confessed that he could never accept the electromagnetic theory of light, because he could not devise a model of it." Teachers are continually coming across pupils who cannot make mental pictures of models from one-plane cross-sectional diagrams, and it is to meet these cases that the authors have devised and used the "perspective" diagrams published herewith, the reference at the moment being more particularly to Figs. 2, 5, and 10. They have found that some of the illustrations become a little simpler if the Earth is drawn with its axis horizontal instead of vertical, and this is the method adopted in Figs. 2 and 5, although in the cases of the map-diagrams (Figs. 3, 4, 7, 8, and 9) they revert to the usual custom and take the axis vertical, so that their argument may be followed more easily.

In order to illustrate the distortion which may arise in any particular case, the writers adopt the principle of a system used by Mr. E. A. Reeves, of the Royal Geographical Society, and by other writers, of projecting a human head instead of a continent, for there are some people, unconnected with schools and colleges, who cannot easily gain access to a globe, and so have no real knowledge of what a continent actually looks like, though they may have a rough idea of its general outline. But this system suffers from the serious defect of not giving a picture of the ideal model head from which all the others are drawn; instead of it there is usually substituted something that can scarcely be called even a verbal delineation, for it consists merely of the words "the average man's head," or something similar. This is very unsatisfactory, for heads vary so much among themselves that no two people will have quite the same conception of the "average," and this is a grave drawback when distortions are being considered, for it is almost essential to be able to compare the normal figure with its caricature.

But the real reason for the absence of such a figure is fundamental, and is, indeed, the whole subject of this article, for the standard figure, or "average human head," would have to be drawn on a sphere and the whole then represented in a

<sup>1</sup> Nunn: *Education, Its Data and First Principles*, p. 184.

plane picture for reproduction on the printed page. To meet these difficulties as far as possible the writers adopt (with consent) as their standard figure a well-known character, because, in view of the wide advertisement he has received, his features must be very widely known, and any distortion will be recognised at once. They then have this figure painted on a sphere—for this purpose they have imagined the character at drill—

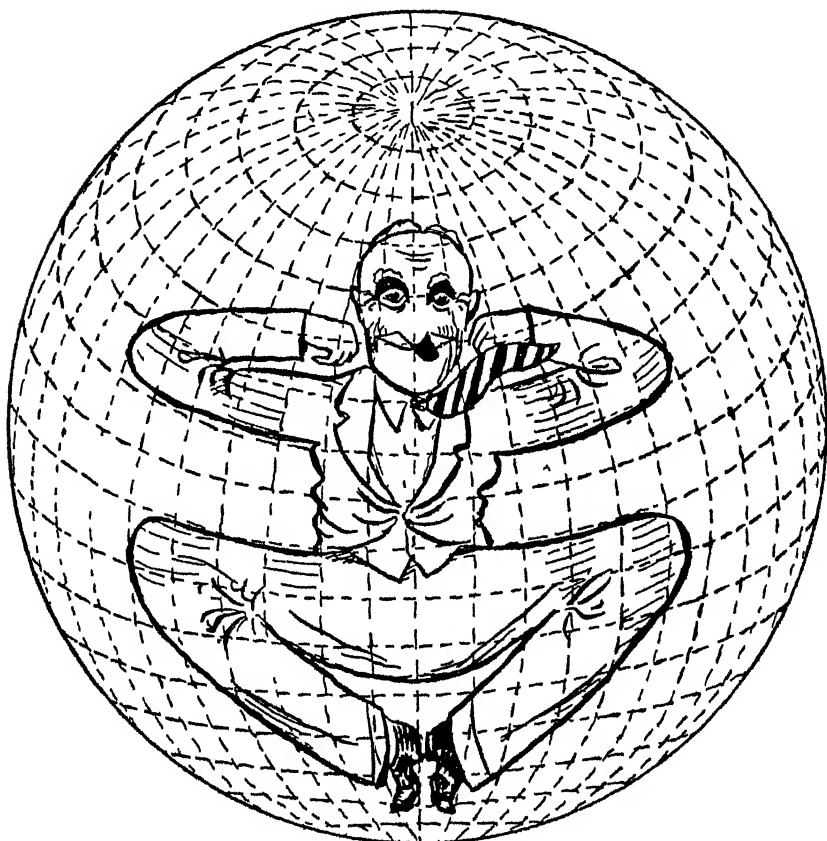


FIG. 1.—The Standard Figure painted on a Globe.

and attempt to show it in "perspective" in Fig. 1, where the extent of their success or failure can be judged by individual readers. They find, however, in practice, that this method serves its purpose well and produces good results. It may be pointed out that Fig. 1 represents about 216 degrees of longitude, instead of the 180 degrees of the hemisphere, and a corresponding extension of latitude, so that in the centre of the picture, to which the model is confined, the distortion

is not great, as can be seen. In subsequent diagrams each portion of the body is plotted with the same latitude and longitude as it has in Fig. 1.

We will now pass on to consider some of the chief projections in detail.

*Orthographic Projection.*—This is frequently used to show the world in hemispheres and is obtained by imagining the observer to be at an infinite distance from the Earth, so that all rays reaching him from it are parallel to each other. Let us suppose that the globe is transparent and that the observer is looking straight at the North Pole, as shown in Fig. 2. The northern hemisphere will appear to him to be circular, bounded

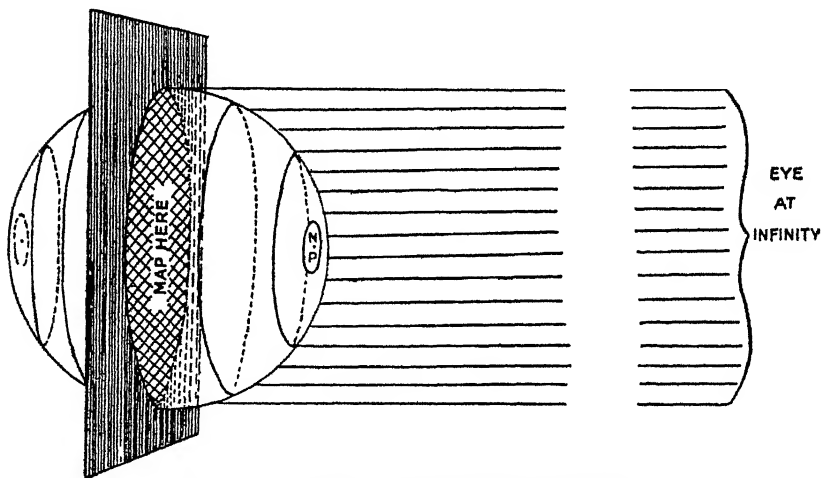


FIG. 2.—Principle of Orthographic Projection.

by the Equator, and lying in the plane of the latter. The southern hemisphere could be projected in a similar way and would make a precisely similar map. In either case lines of latitude will be shown as concentric circles about the Pole and lines of longitude as their radii. If, on the other hand, we take the axis of the Earth to be vertical, so that the observer is now looking straight at some point on the Equator, and this is a position more frequently assumed by the map-makers, the lines of latitude will be horizontal, straight, and parallel to the Equator, though with diminishing distances between them as the Poles are approached, but the meridians of longitude will be halves of ellipses, with the exception of the central one, which will be a vertical straight line, and even this could be regarded as the limiting value of the ellipse. This will be seen in Fig. 3, which is designed to show the defects of the

orthographic projection, *viz.* the cramping at the edges of the map, caused by the lines running together, so that the model would probably not feel flattered by this portrait.

The central portions of the map are, however, fairly good, and thus this form of projection would be suitable for a map of the polar regions, where the Pole occupies the central point. But for a land hemisphere the defect outweighs the merit, for the regions adjacent to the central ones would be gravely



FIG. 3.—An Orthographic Map.

distorted. The hemisphere with North and South America in the middle could be projected fairly well, for the rest of the map would be chiefly water. For certain astronomical purposes this system has its uses, as, from the great distances of the heavenly bodies, the Earth appears to be projected orthographically. Such diagrams were used with great advantage, for instance, by the late R. A. Proctor in his *Universe and the Coming Transits* and in various numbers of the *Monthly Notices of the Royal Astronomical Society*.

*Stereographic and Allied Projections.*—If the observer, whom we imagined at infinity in the last case, now move up to the surface of the Earth, and look at the hemisphere remote from, and concave to, him from, say, the South Pole, the map he will see on the equatorial plane or on a tangent-plane at the North Pole will be markedly different. To economise in blocks, no perspective diagram of this, the stereographic projection, is shown, as the reader can easily construct one for

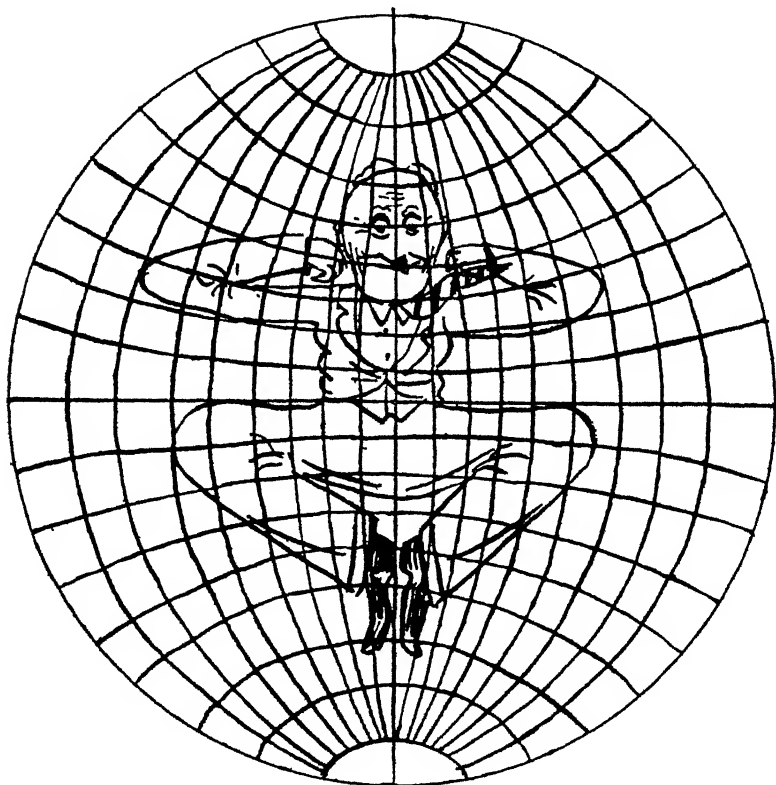


FIG. 4.—A Stereographic Map.

himself on the lines of Fig. 2 or of Fig. 5, the latter of which will be considered in a moment. The defect of the stereographic projection is the reverse of that of the orthographic, for the outside portions of the map are now expanded, but in spite of this feature the method is sometimes adopted for large areas, though their shape is not well preserved. Fig. 4 shows the stereographic portrait of "the model," and illustrates the great merit of the system, that such a network is easily constructed geometrically. Another advantage is that the angles

at which two lines intersect on the map are the same as those at which they intersect on the sphere.

If the plane on which the representation is to be taken be moved towards the observer, it becomes possible to obtain a map of an area greater than a hemisphere, and thus to "look partways round the globe." This is illustrated in Fig. 5, in

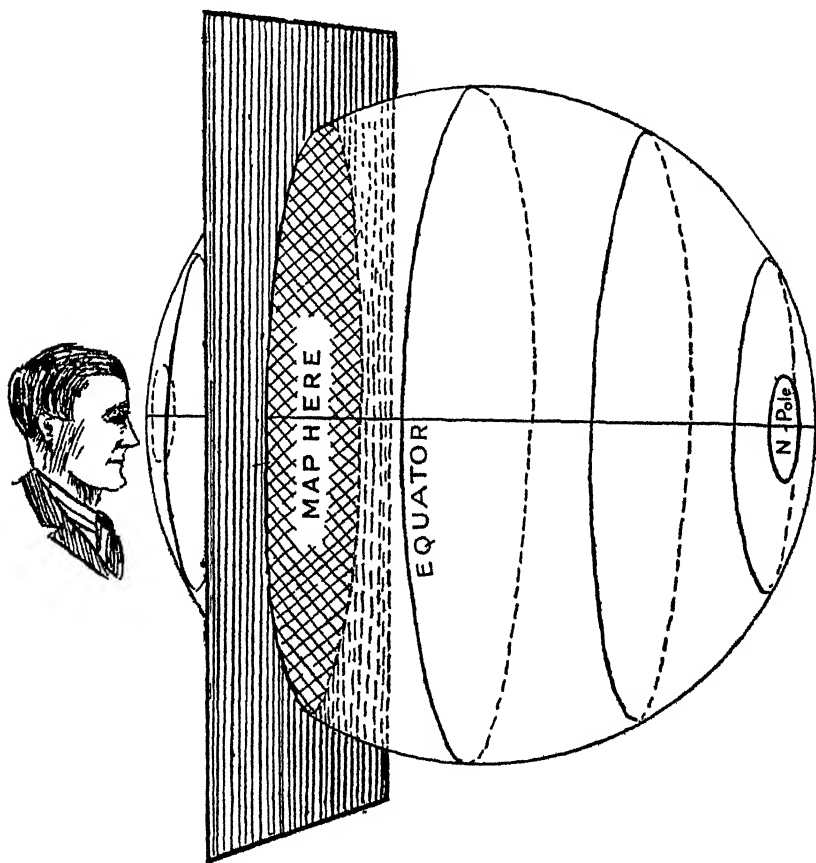


FIG. 5.—"Looking round the Globe."

which the observer is supposed to be looking through a transparent plane, 30 degrees south of the Equator, at the part of the globe beyond it. By varying the position of the plane and the distance of the observer from the surface, different amounts and different representations of the Earth's surface can be obtained on the map. This is particularly useful for astronomical purposes, such as the plotting of eclipse tracks, in which it is convenient to be able to see a large part of the

Earth. The maps used by Oppolzer in his monumental work on eclipses, the *Canon der Finsternisse*, include two-thirds of the Earth in latitude, the North Pole being at the centre and the outer boundary representing 30 degrees south ; but he uses the Polar Equidistant Projection and so has his concentric circles of latitude equally spaced from each other. This

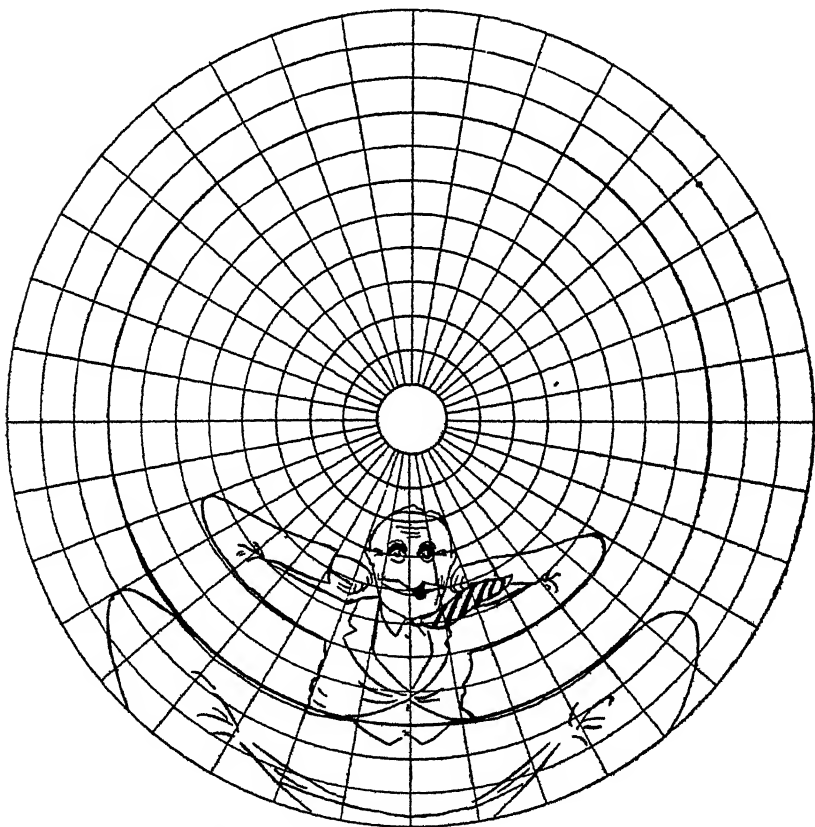


FIG. 6.—Polar Equidistant Projection.

framework is shown in Fig. 6, which shows how very considerable the distortion near the edges may be.

*Globular Projection.*—We have already seen that the defects of the Orthographic and Stereographic projections are opposite to each other, the former cramping the edges of the map, the latter expanding them, while in both cases the central portions are fairly true. It should, therefore, be possible to obtain some intermediate position from which these distortions more

or less cancel each other. This is the basis of the Globular, or Equidistant Map-net, Projection, in which the polar axis and the Equator are of equal length and are divided into equal divisions, as is also the circumference of the circle forming the boundary of the map, the lines of latitude and longitude being constructed to pass through the points so marked. This method of map-making gives a more even and general division throughout, and is often used in school maps and atlases, but

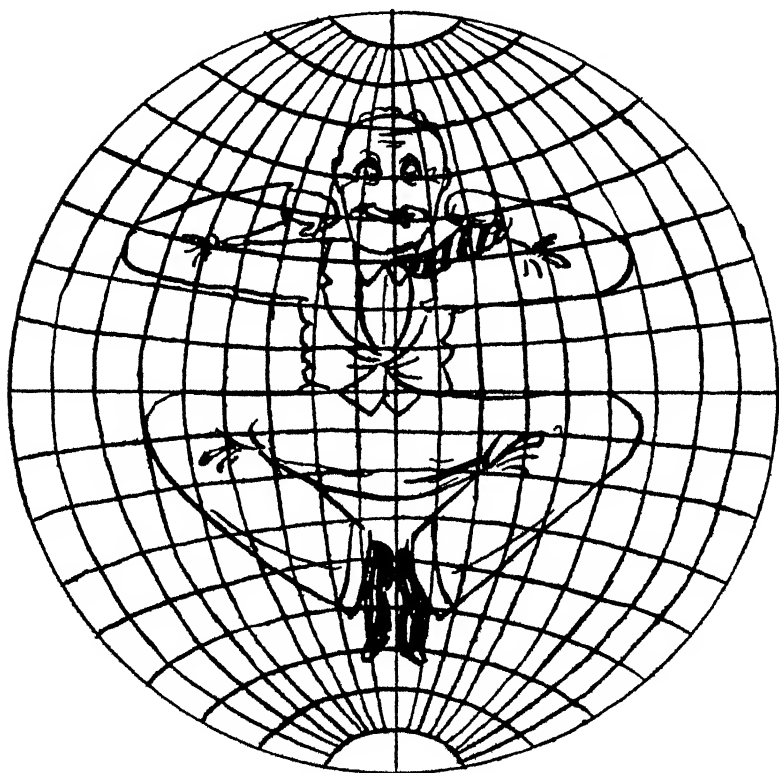


FIG. 7.—A Globular Map.

it suffers from the defect that the scale of the middle differs considerably from that of the outside. Fig. 7 shows a globular portrait of our hero.

*Mercator's Projection.*—Maps made on this system are very well known and are easily recognised by their rectangular shape ; it is often said that they are called after their inventor, but Mercator's real name was Gerhard Kremer ; he was one of the famous map-makers of the sixteenth century. His

system is an improved form of cylindrical projection, a method which explains itself, and has the superlative merit of showing a line of constant bearing between two places as a straight line, obviously a great advantage to sailors, who, indeed, give it a special name, the "rhumb-line," selected because the points of the compass printed on the compass card are known to them as "rhumbs." There are, however, compensating drawbacks, one of which is that the sailors' straight line is not

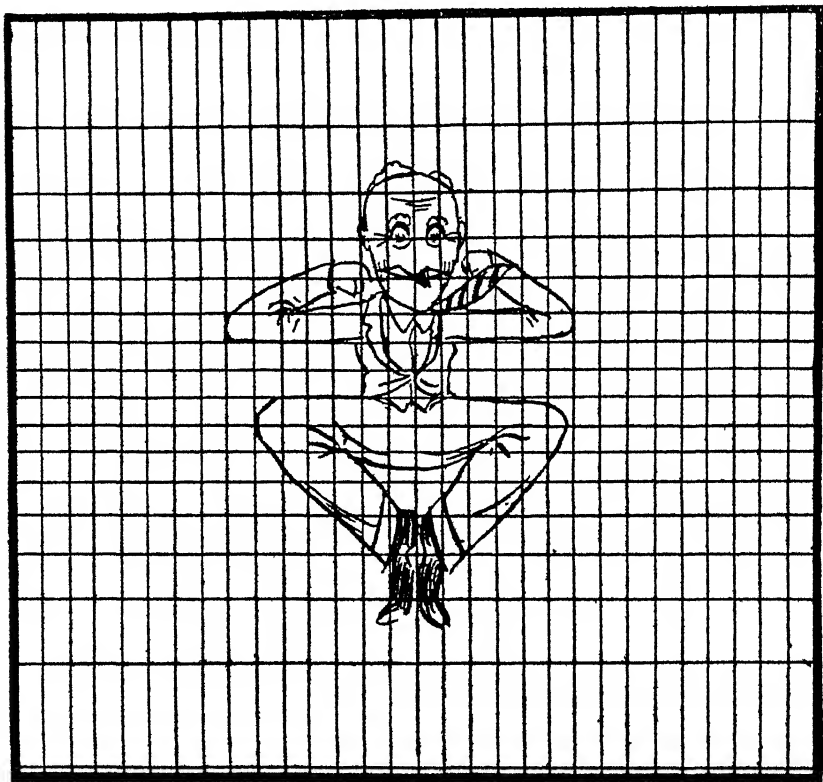


FIG. 8.—Mercator's Projection.

necessarily their shortest route, as it is not always the arc of a great circle, though if they are sailing east or west along the Equator, or due north or south in any part of the world, this drawback disappears.

A purely cylindrical projection would hopelessly exaggerate the polar latitudes; Mercator modifies this somewhat and diminishes the distances of the parallels from the equator according to a definite mathematical scheme, but the resulting distortion is still too gross to make the map of any real use in the far north or south. Fig. 8 shows this clearly, the man's

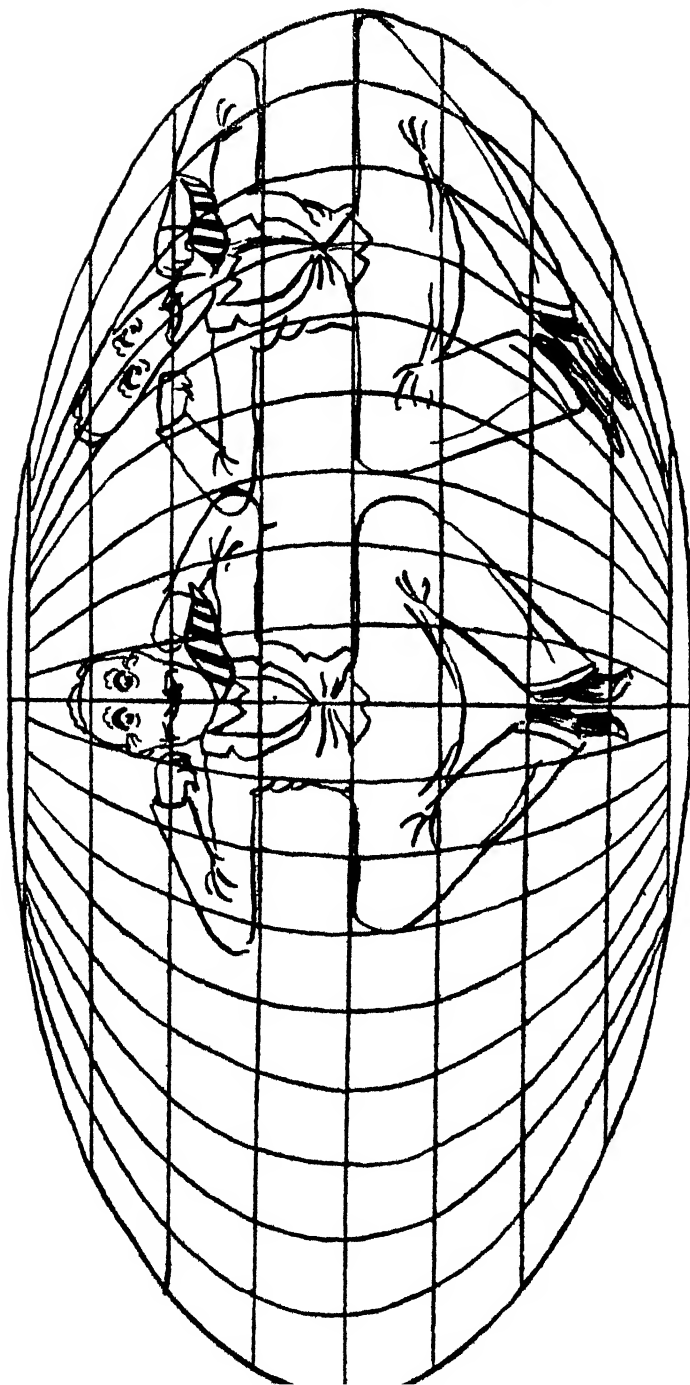


FIG. 9.—Mollweide's Projection, showing grave distortion at edges.

head becoming as wide as his body and of greater area, whilst his feet are abnormally long. All lines of latitude are made of equal length and all lines of longitude are made parallel to one another, so that the scale of the map varies greatly in different parts. On any given line of latitude  $l$  the scale is *sec.  $l$*  times the scale on the Equator; hence, since the secant of 60 degrees is 2, the scale on latitude 60 degrees is twice that on the Equator, which means that this particular line is exactly twice as long as it ought to be. Similarly, 70 degrees is nearly three times too long, 75 degrees nearly four times, and the rate of increase becomes much more rapid as the Poles are approached.

*Mollweide's Projection.*—This, in common with the Mercator method, shows the whole world, but it has the great advantage of representing areas truly, though the shapes of the areas are altered very greatly. The athlete is plotted in the middle of Fig. 9, and is not very greatly distorted, though it is easy to see that, were he plotted near the edges, he would be distorted almost beyond recognition, though the relative areas of the various parts of his body would be pictured correctly; the second sketch drawn on the same framework illustrates this.

Such maps are used to represent the distribution of various natural phenomena, such as rainfall, and also for vegetation, population, and so on.

*Gnomonic Projection.*—If we imagine the globe to be fitted inside the circumscribed cube, which, in Fig. 10, is supposed to be transparent, the projection of the world from its centre on to the sides of the cube is called Gnomonic. Professor H. H. Turner goes into the geometry of this projection at considerable length in a paper on Proper Motions in vol. lxx of the *Monthly Notices of the Royal Astronomical Society*, and those who wish to study the subject in greater detail are referred to it. But the defects are obvious—the map will be in six pieces, with a consequent lack of cohesion, and all distances, areas, and shapes are represented very badly. The one outstanding advantage, however, more than outweighs these drawbacks, for on such a chart any great circle will be represented by a straight line. The great circle joining two places is the shortest distance between them, and hence this shortest distance may be obtained merely by drawing a straight line, whereas on the Mercator maps the straight line is one of constant bearing. Hence the Gnomonic projection is of conspicuous utility to sailors, though less so in practice than in theory, for continuous great circle sailing on long voyages

would often take the ship too far north or south, into the regions of icebergs. Thus, for example, the actual course followed by the boats from New York to England is parallel to the lines of latitude for some distance, to avoid the northern icebergs, and then turns southwards on a great circle.

*Conic Projection.*—Although we are concerned in this article chiefly with the projection of a hemisphere or larger portion of the Earth's surface, we must mention a system which gives us an almost accurate representation of the Earth when we

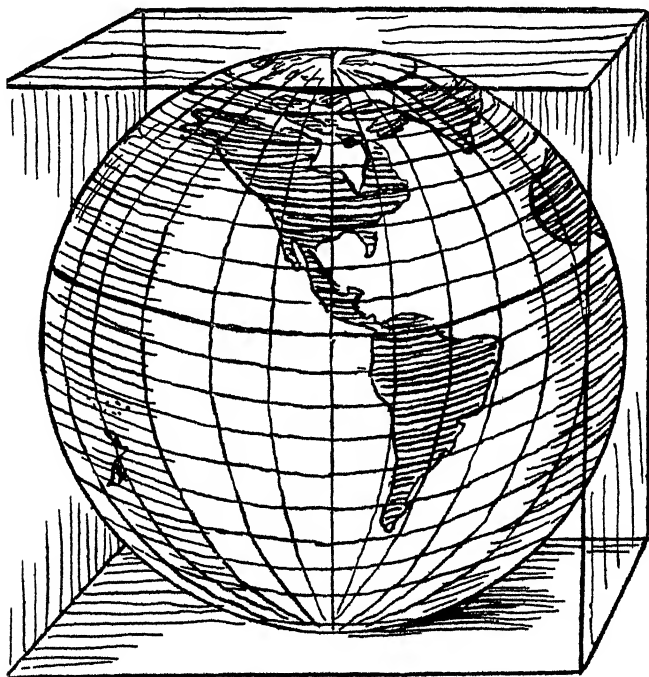


FIG. 10.—Gnomonic Projection (eye imagined at centre of globe).

confine our attention to a region close to some particular line of latitude. This is known as the Conic Projection. The cylinder of Mercator is replaced by a cone which touches the Earth's surface along one of the parallels, so that the slant height of the cone is a tangent to the meridian of longitude at the point of contact. The latitude selected forms the middle of the map, and the belt in close proximity to it is projected on to the cone from the centre of the sphere. The cone is then cut and opened out flat.

If it is desired to map greater ranges of latitude in this way, the cone is made to cut the Earth along two parallels

and the errors in the middle will more or less balance those at the top and bottom. This method is sometimes called the Conic-Secant Projection.

There are many other forms of projection which are, in some cases, mere adaptations of the foregoing, and, in others, are used for areas smaller than a hemisphere, but they will not be mentioned at present, since the writers' purpose in this article has been to discuss the "*flattening* of the sphere."

The writers are indebted to Messrs. E. Griffiths Hughes, Ltd., for permission to use the figure shown in the foregoing diagrams.

## ESSAY

### JOHN NAPIER AND THE INVENTION OF LOGARITHMS

(G. A. Miller, M.A., Ph.D., Professor of Mathematics, University of Illinois, U.S.A.).

THE invention of logarithms has frequently been depicted in popular periodicals with unusual enthusiasm for a mathematical subject, and one finds numerous glowing accounts of the contributions made by the noted Scotchman, John Napier (1550-1617), along this line. For instance, in his Inaugural Address before the Napier Tercentenary Congress held in Edinburgh, 1914 (SCIENCE PROGRESS, 10 : 189), Lord Moulton expressed himself as follows: "The invention of logarithms came on the world as a bolt from the blue. No previous work had led up to it, foreshadowed it or heralded its arrival. It stands isolated, breaking in on human thought abruptly without borrowing from the work of other intellects or following known lines of mathematical thought." This quotation is the more striking in view of the fact that fundamental advances in mathematics are supposed to have always been made in a manner which differs widely from that described therein, and its baneful influence has recently been greatly extended by its appearance with pronounced sanction in Smith's *History of Mathematics*, vol. ii, p. 514.

The noted French mathematician, J. Hadamard, recently directed attention to the fact that the history of science has always been and will always be one of those parts of human knowledge where definite advances are the most difficult; *Bulletin des Sciences Mathématiques*, vol. 1 (1926), p. 97. In particular, it would be very difficult to present a brief but accurate account of the invention of logarithms, and to give due credit to the various writers who have made the most important contributions towards this very important invention. On the other hand, it is very easy to point out that some of the current notions relating thereto are inaccurate, and thus to contribute something towards making the current literature relating to the history of science safer for those who are inclined to believe implicitly what they read. Since the strong points of Napier's work have received so much attention, it seems

desirable to stress here some of the weak points relating thereto, since a correct view can result only from an impartial consideration of both.

Theoretically the science of calculating by means of logarithms was known since 1544, when M. Stifel's noted *Arithmetica integra* was published; what still remained to be done, was to provide means to convert logarithmic theory into practice, according to J. Tropicke, *Geschichte der Elementar Mathematik*, vol. ii (1921), p. 172. It should be noted that Stifel had many predecessors along this line, but none of them seems to have presented the matter as fully and as clearly as was done by Stifel. The theory of logarithms was developed very gradually, beginning with the ancient Greeks, and extending until long after the time of Napier. In particular, the noted Swiss mathematician, L. Euler (1707-83), made very important contributions to this theory, by showing that every number has a infinite number of logarithms. In the history of the theory of logarithms the name of Napier does not occupy a very prominent position. The names of Archimedes, Chuquet, Stifel, Jacob, Euler, etc., are more prominently connected with this theory.

On the practical side Napier's tables were so unsatisfactory that they were immediately replaced by better ones. In fact, it is questionable whether Napier's tables should be called logarithmic tables, since the very fundamental law that the logarithm of the product of two numbers is equal to the sum of the logarithms of these numbers is not satisfied by the so-called logarithms of these tables. It is well known that these tables do not associate 0 with 1. On the contrary, 0 is associated therein with the number which Napier assumed to be  $\sin 90^\circ$ , viz. with 10,000,000. Hence, it is obvious that Napier's tables do not have some of the properties which the modern student naturally associates with every table of logarithms, and the common historical statement that Napier published the first table of logarithms is apt to convey an inaccurate impression on many, if not on most, readers who have not studied the history of this particular subject.

The earlier study of relations between arithmetic and geometric series, such as :

$$\begin{array}{cccccccccccc} \dots & -4 & -3 & -2 & -1 & 0 & 1 & 2 & 3 & 4 & \dots \\ \dots & \frac{1}{16} & \frac{1}{8} & \frac{1}{4} & \frac{1}{2} & 1 & 2 & 4 & 8 & 16 & \dots \end{array}$$

does not constitute the only forerunner of logarithms as we have them to-day. Another important forerunner is the subject of *prosthaphæresis*, which was devoted to changing

multiplication and division into addition or subtraction by such trigonometric formulæ as the following :

$$\sin A \sin B = \frac{\cos (A - B) - \cos (A + B)}{2}$$

Since this subject received its technical name before Napier coined the term "logarithms," and used it in his tables, which might properly have been called prosthaphæresis tables, we can consider ourselves fortunate that we do not have a still longer term for the subject of logarithms. It is interesting to note that the subject of prosthaphæresis continued to receive attention on the part of various writers for a considerable time after the appearance of our common logarithmic tables, but it finally had to give way to the superior subject of logarithms.

These facts relating to Napier's contribution towards the invention of logarithms, when considered in connection with the quotation from Lord Moulton's address cited above, are of greater interest as instances of difficulties presented by the history of science than as evidences of the shortcomings of Napier, or of the comparatively large amount of credit, as regards the invention of logarithms, which is due to writers who preceded him, and to those who succeeded him. It is the business of the historian of science to seek the actual sources of scientific development, even if he is thus led to run counter to the common human failing of giving undue credit to a few individuals, who happen to be very well known and to deserve our genuine admiration. When such undue credit beclouds fundamental historical principles, it is obviously pernicious, even if it resulted from the kindest motives. By the publication of his *Mirifici logarithmorum canonis descriptio*, 1614, Napier contributed powerfully towards the advancement of mathematics, notwithstanding the fact that it is now difficult to harmonise some of its fundamental shortcomings with keen mathematical insight on the part of the author.

The fundamental connection between the imperfect state of the development of trigonometry at the time of Napier and some imperfections of his tables seems to have received insufficient attention. If such formulas as  $\sin^2 A + \cos^2 A = 1$  had been regarded as true (SCIENCE PROGRESS, 20 : 436) at the time of Napier, it appears almost certain that he would have associated 0 and 1 in his tables, instead of associating 0 and 10,000,000. If the former had been done, the fundamental law, that the logarithm of the product of two numbers is equal to the sum of their logarithms, would have held for the logarithms of his tables. As they now stand, we have to subtract the logarithm of 1 from this sum, as well as to

say that the logarithm of the quotient of two numbers is equal to the logarithm of 1 + the logarithm of the numerator, diminished by the logarithm of the denominator, if we regard his tables as logarithmic tables, as is now commonly done and as the title of the book in which they are contained implies. The greatest imperfection of Napier's tables thus stands as a monument to the intimate relation between various mathematical developments, and this very imperfection is a mute witness of the false view expressed in the quotation cited in the first paragraph of the present article. While the adoption of our modern ratio definitions of the trigonometric functions by Napier would have rendered his tables much more useful, it should be explicitly noted that these tables were modified in harmony with these definitions, but independently thereof, by the immediate followers of Napier.

It is not the object of the present article to enter the field so ably covered by Prof. E. W. Hobson in a lecture published under the same heading by the Cambridge University Press, 1914. Our aim has been merely to add a little thereto, especially along lines which could not well be stressed at the time of the Tercentenary Congress in memory of the great achievements of Napier. The association of his name and the subject of logarithms will doubtless be permanent, and hence those interested in the history of science will naturally welcome efforts to secure accurate views relating to the merits of his work. The elementary character of logarithms makes the history of this subject especially well adapted to portray for the general public the slowness with which some very elementary advances in mathematics have been effected, and the extensive collaboration involved in these advances. In particular, imperfect definitions of the trigonometric functions retarded progress, not only in trigonometry, but also in the construction of logarithmic tables.

## NOTES

### **The Annual Meeting of the British Association**

HIS Royal Highness the Prince of Wales is President of the British Association for this year and delivered his Presidential Address at the Annual Meeting held at Oxford on Wednesday, the 4th August. His speech was so complete and well constructed that our readers will doubtless like to see the whole of it placed upon record in this Quarterly. We quote from *The Times* of next day. The Prince said :

My first duty, as President of our great Association, must be to read to you the following message from his Majesty the King :

" I am sensible of the distinction conferred upon my dear son, the Prince of Wales, in presiding at this year's meeting of the British Association for the Advancement of Science ; for I realise that no member of my family has occupied this position since my grandfather was President in 1859. I cannot do better than repeat the assurances then made by the Prince Consort on behalf of Queen Victoria, and express my deep appreciation of the all-important and ceaseless labours in the cause of science of those eminent men who enjoy the membership of your world-renowned Society."

I propose on behalf of the Association to forward the following reply to this message :

" The members of the British Association for the Advancement of Science assembled at Oxford humbly beg to express to your Majesty their loyal appreciation of the patronage extended to the Association by your father and yourself, and of your Majesty's repeated expressions of personal interest in its work. The advancement of science is the constant object of the British Association ; to give a stronger impulse and more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers, to obtain a greater degree of national attention to the objects of science, by removing those disadvantages which impede

its progress, for the well-being of your Majesty's realm and the general good of mankind."

My second duty is to try to tell you—if this be possible—something which you do not know already. For a man who, along with the great majority of his fellow-creatures, can lay claim to no intensive scientific training, it is no light responsibility to be called on to address the annual gathering of the British Association. But I do not intend to shirk that responsibility; for it seems to me that only by discharging it as well as I possibly can shall I be able to show you how highly I value the great honour you paid me when you added my name to those of the distinguished men who have been your Presidents in past years. . . .

Although I claim no insight into pure science, I can fairly claim an onlooker's experience of very many practical instances of science as applied to the needs of our civilisation to-day. For some years past, in war and in peace, I have been privileged to have countless opportunities of examining, at close quarters, the concrete results of such applied science. In things military and naval, in factories, workshops, mines, railroads, in contact with the everyday problems of education, health, land-settlement, agriculture, transport, or housing it has been borne in on me more and more that if civilisation is to go on it can only progress along a road of which the foundations have been laid by scientific thought and research. More than that, I have come to realise that the future solution of practically all of the domestic and social difficulties with which we have to grapple nowadays will only be found by scientific methods.

It is from this experience, and with the convictions it has brought, that I should like to-night to tell you something of my general impressions of the bearing of scientific research on the daily life of the community; and to show how that relationship can be developed by the mutual co-operation of scientific workers and the State. . . . Professor Lamb, in his Presidential address at the last meeting, referred to a certain feeling of dumb hostility toward science and its works, which still survives. No doubt it does; but at least it has ceased to be vocal, as it was in the earlier days of the Association. It became loud (for example) at two of the meetings in this very place. The later of these two occasions was the Oxford meeting in 1860, and the field of battle was the section of Botany and Zoology, in which the theories put forward in Darwin's *Origin of Species* were debated, in a manner which has passed into history, between Wilberforce, Bishop of Oxford, on the one hand, and Huxley and Hooker on the other.

The earlier occasion, however, more appropriately illustrates, by contrast, the modern realisation of our debt to science. The

second meeting of the Association, in 1832, took place in Oxford. The invitation came from Charles Daubeny, who combined the professorships of chemistry, botany, and rural economy, and the President was William Buckland, then Canon of Christ Church and professor of mineralogy and geology. But a strong body of opinion resented the recognition of science by the University when carried to the extent of conferring honorary degrees upon four of the distinguished visitors. The famous Keble, moved for once to anger, referred to those who were thus honoured as a "hodge-podge of philosophers." Their names were David Brewster, Robert Brown, John Dalton, and Michael Faraday. Each of these men has left in the history of his own special branches of science an outstanding memorial. Brewster's researches into optics were his greatest scientific achievement; to our own gratitude he has an especial claim as the leader among the founders of our Association. Brown's services to botany were unsurpassed; perhaps that of widest appeal is his very thorough investigation of the flora of the coastlands of Australia, made during the voyage on which he accompanied Flinders in 1810-14: an early example of what may be termed imperial research. Dalton's name is identified for ever with the atomic theory, and he placed meteorology on a scientific footing. Faraday's labours provide one of the most wonderful examples of scientific research leading to enormous industrial development. Upon his discovery of benzene and its structure the great chemical industries of to-day are largely based, including, in particular, the dyeing industries. Still wider applications have followed upon his discovery of the laws of electrolysis and of the mechanical generation of electricity. It has been said, and with reason, that the two million workers in this country alone who are dependent upon electrical industries are living on the brain of Faraday; but to his discoveries in the first instance many millions more owe the uses of electricity in lighting, traction, communication, and industrial power. Oxford, then, was not dishonoured in the "hodge-podge of philosophers" whom she recognised in 1832.

The services rendered to mankind by the labours of outstanding figures in science, such as Faraday, or Kelvin, or Pasteur, or Lister, are matters of common knowledge. Less generally appreciated is the extent to which the results of scientific research have been brought to bear upon many of the most pressing domestic and industrial problems of the day, and that the co-operation between the laboratory and the State (which means the community) has been greatly strengthened of recent years. The British Association has always supported such co-operation. One of its principal aims is "to obtain more general attention for the objects of

science and the removal of any disadvantages of a public kind which impede its progress." Brewster, in 1830, asserted frankly that "the sciences of England" were "in a wretched state of depression, and their decline is mainly owing to the ignorance and supineness of the Government," as well as to various other causes which he detailed. The same theme (if less forcibly stated) recurs in some of the earlier addresses from the chair of the Association; the Prince Consort, for example, as President in 1859, thus indicates his view of the situation at that time—"We may be justified in hoping," he said, "that by the gradual diffusion of science, and its increasing recognition as a principal part of our national education, the public in general, no less than the Legislature and the State, will more and more recognise the claims of science to their attention; so that it may no longer require the begging-box, but speak to the State, like a favoured child to its parent, sure of his parental solicitude for its welfare; that the State will recognise in science one of its elements of strength and prosperity, to foster which the clearest dictates of self-interest demand."

The position foreshadowed in those words is now, in a large measure, attained. The beginning of a new era was marked by the establishment of the National Physical Laboratory at the end of last century. This was at first set up in Kew Observatory, a building which, as a laboratory for magnetic and meteorological observations, and for the standardising of instruments, owed its maintenance to the British Association for thirty years from 1841, when, as a royal observatory, the Government decided to dismantle it. The building proved incapable of extension, and in 1900 Bushey House, Teddington, was placed at the disposal of the laboratory by the Crown. The laboratory, at its inception, was divided into departments dealing with physics, engineering, and chemistry, and it possesses also the famous William Froude experimental ship tank. The investigations with which it has been so concerned—the testing and standardisation of machines, materials, and scientific instruments, researches into methods of measurement with the utmost accuracy, work on scale-models of ships, and the like—while of the first importance to Government Departments concerned with such applications of science, have also achieved many valuable results for industry in improving standard qualities, indicating scientific methods applicable throughout a variety of manufactures, and thus in bringing about an improvement in the quality of their output for the benefit of consumers.

In historical sequence among the events which have strengthened interaction between Science and the State, there follows the establishment of the Development Commission in

1908. Until that date the only agency for agricultural research in Great Britain was the classical experimental station at Rothamsted, a private benefaction; and the expenditure of the State on this prime factor in national economy was trifling. Since 1908 the Rothamsted station has been expanded to cover the whole field of nutrition and disease in the plant, while other institutes have been founded to deal with other aspects of agriculture, such as plant breeding, the nutrition and diseases of animals, agricultural machinery and the economics of the industry. These institutes provide knowledge for our own farmers and form a training-ground for agricultural experts required by the Dominions, India, and the Crown Colonies. At the plant-breeding institute at Cambridge Sir Rowland Biffen has provided several new wheats, of which two are generally grown throughout the country; the extra yield and value of these wheats must already have more than repaid the whole expenditure on agricultural research since the institute was founded. Among other examples of the value of research there may be mentioned the discovery of a variety of potato immune from the wart disease, investigation of the stocks of fruit trees and inquiries into the production and cleansing of milk, which have resulted in an entire reform of rationing, increasing the yield of each cow by one to two hundred gallons a year, and in freeing milk from the risk of contamination with disease.

Research into fisheries has become a matter of necessity in the light of evidence that even the vast resources of the sea have their limit. Great Britain, acting in co-operation with the other nations who share with us the northern seas, has accomplished much in ascertaining the causes of the fluctuating herring supply, and the study of the methods by which the stocks of plaice can be maintained. Research again is active in finding methods by which we can mitigate one of the consequences of our dense population—the pollution of our rivers and estuaries—and a method has been found whereby great supplies of shell-fish that had been condemned are once more available as food. Some of my hearers will know, too, of the remarkable results obtained from the scientific study of the habits of the salmon. Though fishing has been described as “a fool at one end of a string and worm at the other,” the subject is not without its personal interest, I believe, to many learned men.

Reverting to the historical sequence, it is appropriate to recall, with gratitude for its labours, the constitution of the Medical Research Committee in 1913, under the Insurance Act of 1911; this has since (in 1919) been transferred to a committee of the Privy Council under the name of the Medical

Research Council, and its funds are directly voted by Parliament instead of being drawn from the contributions made by or on behalf of insured persons.

Research alone could provide the knowledge on which must be based all wise and effective legislation or administrative action in the interests of the nation's health. Yet until 1913 the State had played at best a subsidiary part in the organisation of such research and the provision of its material support. Under the new conditions the State is actively concerned with the promotion and co-ordination of medical research towards conquest of those infirmities with which ignorance has afflicted humanity. A few only may be mentioned, which have rightly appealed to wide public interest. Insulin, a gift of science and to humanity from young enterprise and enthusiasm in the Dominion of Canada, is not only saving lives that were threatened, and restoring almost to normal health and enjoyment many that were crippled by weakness and restriction, but, as a tool of investigation, is shaping new knowledge that will influence all our ideas of the functions of the body, in health or disease. The discovery of the vitamins has brought understanding of various defects of health and of development, created for us largely by the blindness of civilisation to dangers accompanying its progress—dangers which science can avert. Closely linked with the discovery has been the more recent development of knowledge concerning the need of sunlight for health, in man and his fellow animals as in plants. Sunlight, or its artificial equivalents, have some importance already in the treatment of disease ; but a realisation of its significance for health has a much greater importance in preventive hygiene. There can surely be no plainer duty, for a State charged with the health of an industrial civilisation, than to promote with all its resources the search for such knowledge as this, as well as to provide for its application when obtained. . . .

Looking backward a dozen years or so, one may say that science was definitely, by that time, a working part of the machinery of the State, though, as we see now, not a part working at full power. The Great War caused a broadening of the scientific horizon for men of science themselves in some measure, but for the layman in a measure far greater. We all were brought to recognise the applications of science as adding, it may be, in certain respects to the distresses of warfare ; but also as immensely alleviating the sufferings caused by it, and as indicating many methods of strengthening the arts of defence—some of which methods are no less valuable in strengthening the arts of peace. The creation of the Government Department of Science and Industrial Research fell within the period of the war ; but it certainly is not to be

regarded as merely a war measure ; it was once described as a near relative of "Dora," but that was a mistake. Nevertheless, it needed the whole period of a century between one great war-time and the next—between the Napoleonic and the World Wars—to mature the conception of a State department of scientific research. Some idea of this kind was clearly present in the mind of Brewster, and certain of his contemporaries, concurrently with his idea of the foundation of our own Association in 1831 ; and later (in 1850), when he addressed the Association from the chair, he claimed a strong advance in scientific and public opinion towards his views. Five years later a concrete proposal for the creation of a Board of Science, possessing "at once authority and knowledge," was put forward by the Parliamentary Committee of this Association (a committee no longer existing) ; but our Council at the time considered that the proposal had "yet to receive sanction from public opinion, and more especially from the opinion of men of science themselves."

It was not, in fact, entirely owing to lack of prevision on the side of successive Governments that the developments which have been outlined were so long delayed. There was an element of mutual distrust between science and the State—now almost, if not quite, wholly removed. A strong body of scientific opinion was avowedly afraid of "organisations of any kind dependent on the State." It is to be hoped that modern developments have removed that fear. The progress of science cannot be kept wholly within training-walls, and no one wants to try to keep it so. The waters of a river may be guided artificially to do the work of irrigation ; but not at their sources, nor yet where, at the last, they percolate the soil. The guidance of scientific research, in its inception, lies with the genius of the individual ; its results for the future may lie far beyond the realisation even of the scientific workers themselves. The Oxford Meeting of the Association in 1894 supplies a simple example of this. There was a discussion on flight, in the Section of Mathematics and Physics, opened by Hiram Maxim ; and no less a leader in science than Kelvin afterwards described Maxim's own flying machine as a child's perambulator with a sunshade magnified eight times. Yet it was not many years before research in aeronautics had become the care of the State as well as of the individual ; and the work carried out before 1914 under (what is now) the Aeronautical Research Committee led on to our wonderful development of aircraft during the war.

A recent report of the Committee of the Privy Council for Scientific and Industrial Research shows that under the Department there are eleven research boards, which co-ordinate and

govern researches in chemistry, fabrics, engineering and physics, radio, building ; food-investigation, forest-products, and fuel ; and to these are to be added the board of the Geological Survey and the executive committee of the National Physical Laboratory. Under the general supervision of the Advisory Council there are now upwards of twenty industrial research associations, formed in alliance with the same number of the principal industries of the country, for the purposes of scientific investigations connected with those industries. No attempt can be made here to review the whole field of work of these various bodies, but a few examples may be chosen for the purpose of pointing out what may be called their homely application. The Building Research Board was created in 1920, and in 1925, at the request of the Ministry of Health, considerably extended its activities. Researches are concerned with the study of materials from the chemical and geological aspects, their strength, weathering, moisture condensation on wall coverings, acoustics, and various other problems ; these inquiries, together with the collection and supply of information, represent "an attempt to create a real science of building, to explain and supplement the traditional knowledge possessed to-day in the industry." It can scarcely be questioned that industrial Britain inherits a legacy of discomfort in the housing of its workers, with all which that implies, dating from a period when the building of the home lacked scientific as well as æsthetic guidance. We need that guidance no less to-day, when the saving of labour is one of the main objectives of the "ideal home" and its fitments.

The Food Investigation Board directs committees concerned with meat and fish preservation, fruit and vegetables, oils and fats, and canned foods. There is also a committee for engineering problems associated with the investigations ; conditions of storage have been investigated on ships between this country and Australia, and problems of heat-conductivity at the National Physical Laboratory, while chemical substances suitable for refrigerants have been studied at the Engineering School here in Oxford. At Cambridge a low temperature research station has been established on ground given by the University, and is working in co-operation with the University bio-chemical, botanical, agricultural, and other laboratories. As for the investigations upon fruit and vegetables, the report may again be quoted, for it illustrates in a sentence something approaching the ideal of scientific co-operation brought to bear upon one particular home necessity, and (what is more) upon a particular and important branch of Imperial commerce. "There is (it says) a closely knit scheme of work, which rests, on the one hand, in university schools of botany, and, on the

other, in commercial stores scattered all over the country, where accurate records of results and conditions have been kept, and extends to the conditions of transport by ship, and overseas so far even as the Australasian orchards." Other directions of research which touch upon commonplaces of our daily life are those concerned with fuel, with illumination, with the deterioration of fabrics and the fading of coloured stuffs, and—perhaps most homely example of all—with the application of scientific methods in the laundry industry. . . .

This attitude of the State toward science marks a definite step in human progress, taken after long hesitation, but in itself new ; and because it is new, we may believe with some reason that we live, not merely in an age of science, but at the beginning of it. The movement for co-operation has borne fine fruit already in other lands, and in particular it is active in our own Dominions. The Indian Empire stands in a somewhat different category from these : there is here a tradition, so to say, for the application of science in its government, and the scientific results of its census investigations, its surveys, its agricultural, forestry, and other administrative departments, have long been famous. This is not to imply that brilliant scientific work has been wanting in the Dominions—far from it—but the co-operative movements with their Governments have followed that in this country and with a laudable promptitude. The trend of developments has been similar, broadly speaking : it is sought to take a comprehensive survey of the natural resources and industrial opportunities of each Dominion, to explore the means by which science may be best applied to their exploitation, to provide, whether in State institutions or in university and other laboratories, for the pursuit of the necessary researches, to co-ordinate the work, and to ensure the dissemination of knowledge acquired. The nature of the researches themselves is conditioned to a large extent (though by no means wholly) by geographical circumstances in the respective territories : agricultural, pastoral, and forestry problems, for example, are not identical in all of them, and that very fact adds to the interest and value of co-ordinating the results of research work throughout the Empire. While problems may differ, solutions may point to a common end.

Nothing but good can follow from personal contact between scientific workers in different parts of the Empire. Nothing but good can follow from their researches if they add, as gradually they must add, to the wider knowledge of the Empire not only among the workers themselves, but ultimately among the whole body of informed Imperial citizenship ; not only in the overseas territories, but here at home. For us at home

the Empire is worth knowing. Our knowledge of it begins with the school lessons in geography and history—or should do so; no doubt the ideal here is yet to be attained. Such knowledge may become later of vital importance to those who wish to join the stream of overseas migration. The British Association, in pursuit of its policy of obtaining from time to time “reports of the state of science” in one department or another, has recently, through a committee of the Section of Educational Science, been collecting evidence as to the facilities existing in our schools for training boys and girls for life overseas. In the crowded curriculum of most schools these facilities, at any rate in their particular Imperial application, are not conspicuous. Yet any labour which time allows us to spend, whether in school-days or after them, upon the advancement of scientific knowledge of the Empire, of the means and manner and environment of life in its component territories, must be well spent. The British Association has played its part in this advancement since, in 1884, it admitted the principle and established the practice of holding occasional meetings overseas. Those of our members who travelled from this country to take part in these meetings have had peculiar opportunities to meet and discuss each his own scientific problems with fellow-workers in the Dominions—and it should be added with particular reference to those meetings which have been held in Canada, that they have provided almost unique opportunities for personal contact between British workers in science and their American colleagues. Our travelling members have been able to see how science is cultivated in the universities of the Dominions and in many other institutions; they have gained first-hand acquaintance with the special problems of one territory and another; and when they have returned home they have talked—as anyone who travels the Empire is impelled to talk. . . .

It is impossible in the Imperial connection to overstate the case for science. Sir William Huggins, in his presidential address to the Royal Society in 1901, said that “assuredly not only the prosperity but even the existence of this Empire will be found to depend upon the more complete application of scientific knowledge and methods to every department of industrial and national activity.” To-day we see that application in much fuller progress than when Huggins spoke only a quarter of a century ago, and already we know how truly he prophesied.

It is not for a moment to be supposed, because the State has come to take a more active and practical interest in scientific research, that there is therefore any occasion for the lessening of interest on the part of societies and individuals. The state

interest involves that other interest, and invites it. It can never become the exclusive function of the State to aid the individual research worker. The State may, and does, co-operate in aiding him, as for instance through the universities and the Royal Society. Nevertheless, there are whole departments of research which do not come within the range of public assistance but are no less valuable because they do not. Therefore the support of science remains the concern of our scientific societies, educational institutions, industrial organisations, and private benefactors no less than it ever did; nay, the very fact that the State has lent its aid should encourage them to continue their aid and to reinforce it—indeed, there is satisfactory evidence that this actually happens. One example will suffice which indicates, incidentally, that from the purely materialistic point of view scientific research is not a luxury; for the community it is probably the cheapest possible form of investment. The Government's fuel research station has not yet proved the commercial possibility of the low-temperature treatment of coal which would result in the more economical production of smokeless fuel, oils, and gas, but in attempting this difficult task it has already, by results unforeseen when the task was undertaken, shown at any rate the possibility of economies for the State and for some of its major industries which are well in excess of the cost of the research itself.

There are parallels in many respects, as has been often pointed out and as often forgotten, between the periods of our history following the Napoleonic Wars and the Great War. The application of science in industry and daily life received impetus in the earlier of these periods in such directions as the introduction of steam motive power; it is receiving it now, as it has been attempted here to show. The auspices now are more favourable. Science is more powerful. Men more adequately and more generally recognise its power, and therein should lie a certain ethical value for it as offering a new point of view, in the manifold interest of which all can share. Should not the application of science, for instance, offer a new field for community of interest, not only between one industrial organisation and another, but within the whole body of workers in any single organisation?

But in order that the community may fully realise all that it owes, and all that it might owe, to the advancement of science, the channels of communication between research and the public mind have to be kept clear, maintained, and widened. The non-scientific public is accustomed to view science as it might view a volcano; prepared for the eruption of some new discovery from time to time, but accepting the

effects of the eruption without realising the processes which led up to it during the preceding period of quiescence. The period of preparation by research before science can offer the world some new benefit may be long, but the scientific machine is always running quietly in the laboratory. There is an example ready to our hands. We recall the introduction of wireless telegraphy and telephony as a scientific gift of quite recent years. Do we all realise that it was here in Oxford, at the meeting of the British Association so long ago as 1894, that the first public demonstration of wireless signalling by means of electromagnetic waves was given by Sir Oliver Lodge? It was the work of science to develop the methods then demonstrated until they have been brought to their present marvellous uses. On the other hand it is often the case, whether in industrial or agricultural, domestic or whatever application, that science has knowledge at command, awaiting use, long before mankind can be brought actually to apply it. Though we have quickened, we are not yet so quick in the uptake of the results of applied scientific research as, for instance, some of our commercial competitors. The public support of scientific research, upon all these grounds, should be accorded freely, with understanding and with patience.

This brings me to the close of what I have to say to you this evening. From my opening remarks, you will have gathered that I looked on you as an extremely formidable audience. That was when I only knew you, so to speak, on paper. Now that I have met some of you face to face—and hope to meet others in the Town Hall in a few minutes, I can only say that, if the Presidential Address has not the traditional weight of knowledge behind it, no President in the history of the Association has ever received a more kindly and sympathetic welcome than you have given me to-night. I am deeply grateful for it.

One more duty remains to me—a duty to our hosts and to our guests. The University and City of Oxford have received us all with a high hospitality worthy of this town, to which all who have known it in the past always return with delight, and which never fails to throw its spell on those who see it for the first time. Their friendly reception has made it possible for those who have worked so hard at the organisation of this meeting to bring it to the successful culmination which it promises to attain. Not the least successful feature of it is the large number of distinguished guests whom it has attracted from overseas. To all of these I wish to offer a most cordial welcome with the sincere hope that they may always carry with them, as I shall myself, the most pleasant recollections of a memorable gathering.

**Reports of the Kala-Azar Commission in India**

*The Indian Journal of Medical Research* (Thacker, Spink & Co., Calcutta) publishes a special supplement, namely, "Memoir No. 4," February 1926, on recent researches now being carried out regarding the method by which the minute parasites generally known as *Leishmania* are communicated from man to man. The volume contains 285 pages and describes much admirable work chiefly by Officers of the Indian Medical Service. There are strong reasons for supposing that these parasites are transferable by a species of sand-fly, *Phlebotomus argentipes*; but perhaps it is still too early to declare that this theory has been absolutely proved, though the evidence for it is very strong. The book contains valuable notes on the parasite itself; on infection experiments with several insects; on transmission experiments with the sand-fly; on the morphology, life-history, and bionomics of this fly; and also on epidemiological researches. The Indian Medical Service is to be congratulated on the great benefits it is conferring on the human race by such investigations, and we hope that the Indian Government will provide sufficient funds to bring the researches to a completely successful issue.

**The Cure of Diseases**

It is surprising that one so seldom sees discussions on the subject of medical treatment in general. We are therefore particularly grateful to Major Hugh W. Acton, of the Indian Medical Service, for reading a paper on "Principles in the Testing of a Cure" before the Medical Section of the Asiatic Society of Bengal on Monday, April 12, 1926. Major Acton is the Professor of Bacteriology and Pathology, Calcutta School of Tropical Medicine and Hygiene, and has evidently made a careful survey of the subject before sitting down to write upon it—which is not always the case in medical literature. As he remarks, there are enormous fallacies connected with disease treatment; and he disentangles the complex in a manner which will be instructive to all medical men, but which can scarcely be analysed in brief. The article and subsequent discussion will be found in *The Indian Medical Gazette* for June 1926, p. 271.

**White Acclimatisation in the Wet Tropics (R. R.)**

*The Geographical Review* for last July publishes an article by Glen Trewartha, discussing recent thought on this problem. Dr. Andrew Balfour published three papers with bibliographies on the theme in the *Lancet* for 1923; and it is a very important theme indeed. The general tendency is to decide that whites

cannot be acclimatised in the wet tropics, at least ; but I am not at all so sure of this. The answer depends upon the definitions of the words "acclimatised" and "wet tropics." Does the former word mean that the acclimatised race must be able to live and breed entirely and indefinitely within the given area ? and what precisely is the definition of the words "wet tropics" ? The author thinks that there is no instance of such acclimatisation in the tropics ; but, when we come to consider it, there is the case of the island of Mauritius, which, though scarcely in the tropics, breeds a French race of planters as virile and capable as any people that I know. The rainfall is heavy, but the whites are comparatively healthy. I feel inclined to sum up the case by prophesying that more and more of the tropics, even of the wet tropics, will become colonisable (to invent a word) by the white races, as diseases are more and more effectively conquered and improvements in housing and the conditions of life are effected. The fact is that in many areas, such as Ceylon, Europeans are almost acclimatised already, except that they send their children home for schooling. The same families live in the same colonies for many generations, though generally with this condition. The death-rate in the tropics is large owing chiefly to the fact that colonisers do not know how to live in the new conditions ; they are apt to "pig it" in mean houses amongst the already acclimatised natives from whom they contract disease ; and at the same time they bring vices and diseases with them, especially alcoholism. In a scientifically idealised world the tropics might prove to be really the best home after all for all races. We must also remember what has happened in the cases of the vast migration of northerners into the southern parts of Europe, and even Asia in ancient times. Probably the Aryan invasion of India has been the most successful instance of a vast colonisation of a tropical country by northerners. A book would be required to deal adequately with the subject, but we must deprecate hasty generalisations in this as in other matters

**The Ross Institute and Hospital for Tropical Diseases, Putney Heath, S.W.15**

This Institute was opened by H.R.H. the Prince of Wales on July 15. A large audience of about five hundred people were collected in a marquee in the grounds of the Institute. The Chairman, Sir Charles McLeod, Bart., read an address to the Prince describing the object of the Institute (which is to advance research on tropical diseases) and detailing the history of the incorporation of it. The Prince then spoke as follows :

"The opening of this Institute, which I am very glad to perform, commemorates a definite achievement in the work

which the British Empire has done for civilisation. Not the least important obligation which the development of that Empire has laid on the members of our race is the constant struggle against disease in all the varied forms it can assume in different climates and in different latitudes. The struggle is always fiercest in the tropics, where man, and especially the white man, is the continual prey of diseases from which we in this country are, comparatively speaking, protected by a more temperate climate ; and perhaps the saddest page in the whole history of our Empire is that which tells of the terrible toll taken by plague and by fever of those who helped to build it.

"Of all these tropical enemies, malaria has probably been the most deadly and elusive. But now, thanks to the achievement of one man, whose name we are perpetuating in this Ross Institute, its ultimate defeat is certain. Only a few months ago I was able to see in the Sir Alfred Jones Laboratory at Sierra Leone not only the vital need for a campaign against malaria, but also the remarkable progress which has already been made in it. Perhaps I am biased by many visits to tropical and subtropical parts of the world where malaria is rife, and by having talked with so many men and women whose health has been shattered by a life's work in such districts, but I can think of no other single discovery in recent times which will earn the deep gratitude of so many thousands of human beings of all nationalities as the discovery made in India by Major Ronald Ross—as he then was—on August 20, 1897. The story of its subsequent development and what it has led to is well known to you all. I need only summarise it in the words of a famous writer : " It is not too much to say that Sir Ronald Ross has made a third of the world inhabitable."

"Over a quarter of a century has elapsed since that epoch-making discovery, and this Institute and Hospital now stand as a memorial to the life-work of Sir Ronald and his colleagues. But it is not merely a passive memorial to work accomplished in the past ; it is also, as Sir Charles McLeod has just reminded us, a very active centre for work to be done in the future. All who have any experience in the tropics will know that there is still a vast field for scientific medical research, and here, with all the resources that modern science can provide, such research can be effectively carried out, provided adequate funds are forthcoming. From this building may issue results which will bring back health to thousands who have lost it, or safeguard the lives of countless others threatened by unseen dangers in tropical lands. More than that, it may open out, for the use and benefit of mankind as a whole, huge districts which are at present denied to civilisation.

"It is not surprising, therefore, that when the scheme for

founding this Institute was first put forward, it immediately received widespread public support. That support happily made its inception possible, and once the first financial obstacle was surmounted, its promoters lost no time in making it a reality. To the President, the Duchess of Portland, to Sir Charles McLeod and Mr. Shakespeare, and to Sir Ronald and his two co-Directors, Sir William Simpson and Dr. Castellani, the gratitude of all of us is specially due for the time and energy they have devoted to its execution. They and their fellow-officers of the Institute may well feel proud of the result of their labours, and of the knowledge that they have called into being something which cannot fail to be of incalculable benefit to many generations.

"It is with the sincere hope that further public support for the necessary endowment may be forthcoming, and that the labours of those who will work here may be blessed with every success, that I now declare the Ross Institute and Hospital open."

The Duchess of Portland, President of the Institute, and Sir Ronald Ross proposed thanks to the Prince, and the Bishop of Southwark offered up a dedicatory prayer. A full account of the proceedings can be obtained from the Secretary of the Institute, if required.

### Notes and News

The Honours list published on the occasion of the King's birthday contained the following names, well known in scientific circles : Sir Frank Dyson, Astronomer Royal, who was made a K.B.E. ; Colonel H. G. Lyons, Director of the Science Museum, who received a knighthood ; Dr. G. C. Simpson, Director of the Meteorological Office, and Mr. F. E. Smith, Director of Scientific Research, Admiralty, who received the order C.B. (Civil Division) ; Dr. A. W. Hill, Director, Royal Botanic Gardens, Kew, and Mr. J. O. Shircore, Director of Medical and Sanitary Services, Tanganyika Territory, C.M.G.

The following eminent scientists have been elected Foreign Members of the Royal Society : Prof. M. W. Beijerinck, Delft ; Prof. N. Bohr, Copenhagen ; Prof. Ernst Cohen, Utrecht ; Prof. W. Einthoven, Leyden ; Prof. K. E. R. von Goebel, Munich ; Prof. H. F. Osborn, New York and Washington ; Prof. Max Planck, Berlin ; Prof. A. Sommerfeld, Munich.

Prof. Moscicki, the well-known electro-chemist, has been elected President of Poland. Sir Ernest Rutherford, Sir F. Hopkins, Prof. H. A. Lorentz, and Dr. H. L. le Chatelier have been elected foreign members of the Académie Polonaise des Sciences, Cracow.

Major-General Sir Matthew H. G. Fell has been appointed

Director-General, Army Medical Services, in succession to the late Lieut.-General Sir W. B. Leishman.

Dr. W. H. Eccles has been elected President of the Institution of Electrical Engineers.

Sir Dugald Clerk, K.B.E., has been elected Prime Warden of the Goldsmiths' Company.

Prof. Paul Sabatier, For. Mem. R.S., has been awarded the Albert Medal of the Royal Society of Arts.

The Duke of Northumberland has been re-elected President of the Royal Institution; Sir Arthur Keith is the Treasurer of the Institution and Sir Robert Robertson is Secretary.

Among the names of scientific men whose death has been announced during the past quarter were those of Dr. E. Bles, zoologist; Dr. J. T. Bottomley, physicist of the University of Glasgow; Sir James Cantlie; Mr. J. J. Fletcher, biologist, South Wales, Australia; Mr. F. Harrison Glew, radiologist; Dr. H. B. Guppy; Dr. C. Hering, one of the founders of the American Electrochemical Society; Lieut-General Sir W. Leishman, Director-General of the Army Medical Service; Sir Frederick Mott, pathologist; Mr. Stephen Paget, founder of the Research Defence League; Admiral Sir J. F. Parry, Hydrographer to the Navy, 1914-1919; Prof. W. F. Shanks, physiologist of the University of Leeds; Mr. F. S. Spiers, secretary to the Faraday Society; Sir Stewart Stockman, Director of Veterinary Research at the Ministry of Agriculture and Fisheries; Mr. F. C. F. Wratten, a pioneer of gelatine-plate photography.

A special meeting of the International Research Council was held at Brussels in June last to consider the question of the admission of delegates from "enemy countries" to the work of the Council. The following resolution was passed unanimously: "That this meeting of the extraordinary general assembly of the International Research Council decides to invite Germany, Austria, Hungary, and Bulgaria to join the International Research Council and the unions attached to it and, in doing so, to indicate the institution which will act as adhering body." As reported in *SCIENCE PROGRESS* last October (p. 291), an ordinary meeting of the Council was held at Brussels in July 1925, when this matter was discussed. Owing, however, to an insufficient attendance at the meeting, no statutory alterations could be made. The fact that a sufficient number of delegates could be assembled for the extraordinary meeting is an indication of the strong feeling in certain quarters that the statutes should be amended. They will, in any case, have ceased to apply in 1931.

The Institute of Science and Industry, Commonwealth of Australia, has been entirely remodelled on the lines recom-

mended by Sir Frank Heath in his Report to the Prime Minister of the Commonwealth, dated January 27, 1926. It is to be controlled by a National Council consisting of a chairman and eight members ; the chairman and two members being appointed by the Governor-General, the remaining six members being the chairmen, or their deputies, of Advisory Committees established in each State. The State Advisory Committees will consist of six members ; of these two will be nominated by the State Government, two by the National Council as representatives of the State University, and two by the principal industries of the State as their representatives.

Sir Frank Heath recommended that the three members of the National Council appointed by the Governor-General should form an executive committee, with power to exercise the functions of the Council in the six-monthly intervals between the meetings of that body, and that they should receive an honorarium of £500 each year for their services. He also recommended that the Institute should establish the following sections under the charge of special scientific officers : (a) Agricultural, (b) Food, (c) Forestry and Forest Products, (d) Fuels, (e) Fisheries, (f) such others as may from time to time seem desirable.

The Report indicates that the first duty of the Institute must be to make provision for training men in the methods of scientific investigation after they have graduated at their Universities, and the Bill embodying the Report provides that a Trust Fund of £100,000 shall be formed, the income of which should be used for such training.

These recommendations have already been put into effect. The members of the Executive Committee have been chosen, the Trust Fund has been formed and a capital sum of £250,000 has been put aside for the work of the Council.

A short report on Power Alcohol Production compiled by Sir Robert Robertson, Sir<sup>T</sup> Joseph Petavel, and Mr. W. A. Calder contains a good deal of interesting information, even though the final conclusions are not very hopeful. It appears that the cost at the works of 1 gallon of alcohol prepared from beet would be 1s. 9d. with beet at £1 per ton, and 2s. 9d. with the beet at £2 per ton. To these figures must be added the cost of denaturing, packing and transport, to say nothing of the profits of the wholesalers and retailers. Furthermore, 1 gallon of alcohol is by no means equivalent in fuel value to 1 gallon of petrol. The actual equivalent depends on the design of the engine. Existing engines after suitable adjustment of their carburettors require about 1.8 gallon of alcohol to replace 1 gallon of petrol. By increasing the compression ratio to the limit permissible if petrol is to be used as an alternative

fuel, the 1·8 gallon could be reduced to 1·5, and with still higher compressions (say 7 : 1) the figure might be brought as low as 1·1 ; but such engines would not run at all on undoped petrol. The running of an engine is said to be noticeably sweeter and smoother with alcohol than with petrol, but starting presents a difficulty, since the vapour pressure of alcohol is comparatively low. It will thus be seen that the prospects of alleviating the liquid-fuel position by the manufacture and use of alcohol are not at present very promising.

In the course of his evidence before the committee, Mr. B. H. Morgan, of the British Power Alcohol Association, mentioned that some success had been obtained with a mixture known as Natalite, which contains about 40 per cent. ether, 60 per cent. alcohol, and 0·5 per cent. castor oil to inhibit corrosion. Some 3-4 million gallons of this are produced each year—notably in South Africa, where it sells at from 2s. 3d. to 2s. 6d. per gallon—petrol costing 2s. 9d. in that country. Molasses form the most promising source of supply, but that product is now controlled by the Oil Trust, which, in the last five years, has raised the price from £2 to £4 10s. a ton !

The Fourth Annual Report of the Safety in Mines Research Board, in respect of the year 1925, has recently been published (H.M. Stationery Office, price 1s. net). The Report is mainly devoted to brief accounts of the progress of the numerous researches which are being carried out, either directly under the Board or Committees of the Board, or at universities and other centres with the aid of funds supplied by the Board. The total annual expenditure on these researches approaches £50,000 a year, and is mainly defrayed out of the Miners' Welfare Fund. The subjects of research include problems relating to coal-dust explosions, firedamp explosions, spontaneous combustion of coal, the safe use of electricity in coal mines, mining explosives, safety lamps, devices for trapping dust generated by rock drills, falls of ground, wire ropes, and the control of mine temperatures. Eleven reports have been published by the Board during the year and a list is given of many other papers relating to the researches written by members of the staff and published in scientific journals, etc. A report on the co-operative research which has been arranged between the Safety in Mines Research Board and the United States Bureau of Mines on many of these subjects during 1925 is given in an Appendix, and there is a note describing the organisation of mining research in the U.S.A. The report also embodies an account of the progress of the health inquiries carried on under the supervision of the Health Advisory Committee, notably the inquiries with regard to the injurious effects of certain rock dusts.

How slender is our knowledge of the uppermost regions of

the earth's atmosphere was made clear in two lectures delivered last summer, which dealt with that subject. In his Halley lecture at Oxford, Dr. G. M. B. Dobson gave a general account of the subject, and at the Royal Institution Prof. J. C. McLennan dealt largely with the evidence given by the auroral spectrum. Quoting from the work of Chapman and Milne, Dr. Dobson pointed out that, at the higher levels, *e.g.*, above 100 kilometres, hydrogen and helium should form a far greater proportion of the "air" than they do at the earth's surface. The actual proportion of hydrogen given by the calculation depends on the value assumed for the hydrogen content of the lower air, and we do not know whether that is 1 in 100,000 or 1 in 100,000,000! But observations on the auroral spectrum, though indicating the presence of oxygen and nitrogen at much higher levels than hitherto supposed, fail altogether to indicate the presence of any hydrogen or helium at all! At present it is only possible to make vague speculations as to the cause of this anomaly.

Auroræ are usually formed at heights between 100 and 106 km. Very few have been detected below 80 km., and above 106 km. the number gradually diminishes until their maximum height of from 500–700 km. is reached. The spectrum of the auroral light is largely due to nitrogen, but a bright line in the green, which is always present, has provoked a good deal of controversy, and another, in the red, which sometimes appears, is also of unknown origin. Vegard has attributed the green line to the bombardment of finely divided solid nitrogen by corpuscular radiation from the sun. McClennan and Shrum have obtained the line with oxygen or with a mixture of oxygen and helium. Lord Rayleigh has found traces of it in the light from the night sky. Its origin must therefore still be regarded as uncertain, with the probability that McClennan's explanation will turn out to be correct. There is, of course, little doubt that the cause of the auroræ is an electrical discharge, entering the earth's atmosphere from the sun.

Meteors or shooting stars provide another source of information concerning the upper levels of the air. These objects, often no bigger than a pea, become luminescent through friction when they enter the atmosphere. This luminescence has been observed to appear at heights varying from 40 to 170 km., and to disappear at from 20 to 120 km., the usual figures being about 120 for appearance and 80 for disappearance. The velocity varies from 10 to 120 km. per second, and remains constant for the whole of the luminous path, whence it follows that disappearance is a result of vaporisation. These meteoric trails have led to two items of information concerning the atmosphere through which they move. Firstly, their drift

enables the wind velocity to be determined; and secondly, their length and brightness permit of an estimation of the temperature of the air. The wind velocities are large. Their predominant direction at from 80 to 100 km. seems to be from the north-west over the British Isles, and from the south-west over the United States. The temperature above 60 km. would appear to be surprisingly high, about  $25^{\circ}$  C. Between 10 km. and 55 km. it remains fairly constant at about  $-50^{\circ}$  C. Probably the high values above 60 km. are due to the absorption of short ultra violet waves from the sun by the ozone, which is formed at these levels by the action of the short waves on oxygen. It is this zone which shields us from the injurious effects of the ultra violet radiation, and its extraordinary opacity will be appreciated when it is stated that the proportion of ozone in the whole atmosphere is only one part in three million, *i.e.* sufficient to form a shell round the earth 3 mm. thick at normal pressure. The 60 km. level in the atmosphere is interesting for several reasons. Hereabouts is the ionised layer responsible for the reflection of the electromagnetic waves, which convey wireless signals, while the small diurnal variation of the earth's magnetic field is probably due to the tidal movements of this ionised air. Finally, the increase of temperature at this level is possibly the cause of the downward refraction of sound waves, and thus is responsible for the curious phenomena attendant on the propagation of intense sounds.

Dr. Dobson's lecture is published by the Clarendon Press, Oxford, and costs 2s. 6d. net in paper covers.

Technologic Paper No. 303 of the Bureau of Standards, Washington, entitled *Causes of Some Accidents from Gas Appliances*, is of very considerable interest to the householder. In the early winter of 1922-3 a number of cases of partial or complete asphyxiation by carbon monoxide produced by domestic heating appliances occurred in Baltimore, and public opinion attributed them to the quality of the gas supplied by the local company. Ultimately, the Public Health Department was driven to make an investigation with the assistance of the Bureau of Standards. This showed that the trouble was largely due to ignorance and carelessness, but that faulty apparatus or apparatus easily rendered dangerous by the consumer was sometimes a contributory factor. The Report contains several points, to which attention should be directed in this country. In the first place the manufacturer must design his appliances properly (by no means a simple matter), must employ materials which do not easily corrode in use, and should, as far as is possible, avoid the provision of adjustments whereby, through ignorance, the consumer can make

his apparatus dangerous. The consumer should remember that danger arises from faulty combustion, carbon monoxide being formed only when there is an insufficient supply of air at the places where it is needed. Flames should not be too soft, *i.e.* they should show well-marked inner cones. The gas should not burn above the radiants in a gas fire (test for this in a darkened room), and floating flames licking round the sides of a gas cooker, or up the sides of large boilers placed thereon, or indeed anywhere, are indications of conditions favourable to the formation of the poisonous monoxide. Further, the onset of headache, giddiness, or sickness in anyone in a room where gas is being used indicates the need for expert examination of the appliance. This brings us to the last point; the need for periodic inspection of all gas appliances. The consumer cannot be an expert in such matters; the local plumber or gasfitter probably knows as little as the consumer. The work requires a specially trained man. When the appliance is hired from the gas company, an occasional inspection is sometimes made; but when it is bought outright this service is entirely lacking. It should be provided, either by the manufacturers themselves or by the municipality. Only by such expert examination can the safety of domestic appliances be ensured either immediately after fitting or after long periods of use.

Two of the *Scientific Papers* received from the Bureau at Washington this quarter are of considerable interest. Paper No. 521 contains an account of measurements of the expansion and refractive index of glass at various temperatures from 20° C. to 700° C. The Fizeau interferometer method was used and experiments were made with nine different varieties of glass, including flints, crowns, and pyrex. In every case there was a remarkable change in the behaviour of the glass at temperatures in the neighbourhood of 500° C. The rate of expansion increased from two to seven times, and the index of refraction showed a sudden fall, which continued until the softening temperature, after which the index began to increase once more. Paper No. 526 contains further data concerning the transmission and absorption of sound by building materials. Sounds of various frequencies were obtained from a loud speaker by means of a valve oscillator, and a two-valve amplifier. The intensity was determined by a magnetophone method devised by J. C. Karcher. In this way the actual fraction of the incident energy transmitted or absorbed by various materials at different frequencies could be determined. The results obtained were very curious, both transmission and absorption sometimes varying in an irregular manner with frequency. With this reservation it may be said that, on the whole, the fraction of the sound transmitted decreases with increase of frequency,

while the fraction absorbed at first increases and then decreases. The best and worst of the transmitters tested were of lath and plaster differently constructed, the reduction of the sound varying from a quarter to a seventh (in units of audibility, or from  $10^{-4}$  to  $10^{-7}$  in units of energy). On the other hand, all the plaster panels were poor absorbers, hair felt 1 inch thick being the best.

We have received a copy of a paper by Prof. L. V. King, F.R.S., of McGill University, entitled *Gyromagnetic Electrons and a Classical Theory of Atomic Structure and Radiation*, in which the conception of a spinning electron has been used to deduce the Einstein photo-electric formula and the formula for black-body radiation by means of classical dynamics. With the further conception of a spinning proton the author has been able to extend his results to spectral series whose constants are interpreted in terms of nuclear perturbation frequencies. The quantitative theory of the Zeeman and Stark effects is being worked out in terms of the new hypothesis, and a further paper is to deal with this aspect of the matter. The first paper has been published by Louis Carrier, Mercury Press, Montreal, at a price of 5s. post free, in order to avoid the delay involved by sending it to one of the scientific journals—a course rendered possible by the co-operation of the Principal and Governors of McGill University.

The Health Committee of the League of Nations publishes notes of its proceedings. Its sixth session was held last April, and the work of the Malaria Commission of the League was reported on at the fifth meeting held on April 30 (C. 262, M. 96, 1926, iii, p. 28). There are several points of interest in this Report, but one does not gather that all the statements made were very well informed. For instance, one gentleman said that "the macrogametes . . . were able to cause a relapse, at a given moment, with the invasion of the surface blood by a new generation of parasites." We have always understood that this rather wild hypothesis of the late F. Schaudinn has long been completely discredited. We are also told that the oöcytes of the parasites may be able to develop in the muscles of the *Anopheles* and not only on the surface of the stomach. Several suggestions were made regarding drugs alleged to be as useful as quinine—a very dangerous theme, since such drugs are advertised every few months. There were also remarks regarding "Anophelism without malaria." This is a phrase delighted in by amateurs, but almost meaningless. The whole discussion is not impressive.

On June 11 the West London Medico-Chirurgical Society presented its gold medal to Prof. R. T. Leiper, M.D., D.Sc., F.R.S., of the London School of Hygiene and Tropical Medicine,

for his important contributions to medical Helminthology, especially for his work on the life-history of the guinea-worm and of the Egyptian *Bilharzia* parasite. His work has led to important improvements as regards the diseases caused by these helminths.

The True Temperance Association, Donington House, Norfolk Street, W.C.2, has issued two little pamphlets discussing the "new inn," one by the Rev. the Hon. James Adderley, and another called *Objections to the Improved Public-house*. They discuss the point as to whether improved public-houses of the continental type where people can get moderate alcohol with music and food should be allowed, or whether all houses which supply alcohol should be forbidden, as in America. Many absolute teetotallers look upon the former as being dangerous ; but, in our opinion, anything would be better than the secret drinking-dens which seem to be so common under the name of public-houses in this country. The question whether absolute prohibition is really necessary has not yet been finally and scientifically answered.

The Medical Research Council has issued a very valuable monograph, namely Special Report Series, No. 99, 1926, on the statistics concerned with cancer ; and we also draw attention to the *Franco-British Medical Review*, vol. ii, No. 10, July 1926, containing a series of papers chiefly on the microbic origin of the disease. Some of these are very well written, and suggest that medical opinion is now tending to veer round very much in favour of such an etiology. The labours of Dr. James Young of Edinburgh and Dr. T. J. Glover of Canada seem to have led the way in this line of work, and to have anticipated to some extent the recent investigations of Dr. Gye. Some years ago, also, Dr. H. C. Ross wrote a book called *Induced Cell-reproduction and Cancer* (Murray, 1910), which now seems to be forgotten, but which also moved in the same direction, at least indirectly.

An example of the useful medical work now being done by private British people in foreign countries will be found in the labours of the "Herbert Fund for Albania," details of which can be obtained from the Hon. Secretary, 13A Porchester Terrace, London, W.2. Lord Lamington is President, and Elizabeth Countess of Carnarvon one of the Vice-Presidents. Malaria and famine are the great enemies against which these good people are trying to contend.

*The Times* of August 3 states that fourteen circus elephants were stampeded recently at Edmonton, Alberta, by the barking of a small dog. They broke loose, upset two wagons full of bears and tigers, rampaged through the town, and escaped into a wood, where they were finally recaptured. The cowardice

of these huge animals is a remarkable fact about them, and one of us saw precisely the same thing at Moulmein in Burma about 1887. A puppy a foot long is a terror to the elephantine giants, just as a mouse in a kitchen is to the maidservants there—suggesting that there must be some psychological similarity in the two cases.

The United States Department of Agriculture, in its *Departmental Circular* 367 of April 1926, describes "Airplane Dusting in the Control of Malaria Mosquitoes," by W. V. King and G. H. Bradley. Paris green was used as the larvicide and was scattered from aeroplanes over *Anopheles* breeding-marshes near Tallulah, La. Paris green is an arsenical preparation which, when mixed with fine dust, floats on a water-surface, and poisons the larvæ of *Anopheles* there. Similar dusting has already been carried out by the Department for destroying pests affecting cotton. *The Times* of August 4 reports that a Lincolnshire farmer, Mr. George Caudwell, of Weston, has recently made similar experiments over a field of 40 acres planted with potatoes. The operation would have taken two days to carry out by hand, but was done in twenty-five minutes by aeroplane.

*The Times* of August 9 reports a curious story from a Dorset correspondent of a cow appealing for human aid in the rescue of her calf. The calf had fallen into a river, and the cow came up to the manager of the farm and half pushed and half led him to the river, where he found, and we presume rescued, the calf. The writer of this note had a similar experience many years ago. The cat in his house in Bangalore, India, had three kittens, which were kept in the bath-room. One day the cat issued, making a loud clamour. He followed the animal, which led him into the bath-room, and he found that all her three kittens were covered with huge black ants. Two of them were rescued, but the third died of the bites.

The Zoological Gardens in Regent's Park are having a new house constructed for reptiles with an electrical installation for heating and lighting. The work is being done with the co-operation of the General Electrical Company, working with Mr. Guy Dawber, the architect, and with Miss Procter, who is the Curator of Reptiles. The problem is to keep the cages at suitable temperatures of about 80° Fahrenheit; and this can only be done by electrical heaters being installed within the cages, under the floors, and in the walls, and so on, in such a way that the animals cannot injure themselves by contact. The temperature will be regulated automatically. At the same time the visitors in the reptile house will not be exposed to too great a heat, and the ventilation will be kept good both in the house and in the cages.

## CORRESPONDENCE

*To the Editor of SCIENCE PROGRESS*

### ON ARTIFICIAL FUNCTIONS AND NON-UNIFORM CONVERGENCE

FROM ALBERT EAGLE

DEAR SIR,—Mr. F. P. White's review of my book *A Practical Treatise on Fourier's Theorem and Harmonic Analysis*<sup>1</sup> in the April number of SCIENCE PROGRESS calls for a few words by way of explanation of some of the views adopted in it. Since most of the readers for whom my book was intended would, I knew, not be steeped in the orthodox opinions on the points concerned, I did not think it necessary to particularise in detail how my views differed from those of some writers on pure mathematics.

Mathematics is an infinitely greater subject than any person's views of it; and it is greatly to be regretted if critics, by denouncing authors who prefer to take any unorthodox views, should tend to make the subject as stereotyped as Roman Catholic theology; while for a reviewer to recommend people not to read a portion of an author's work is going as far as possible towards placing it in an *Index Expurgatorius*.

Mr. White takes great exception to my statement that, "for ourselves we can hardly make out how it could have escaped detection in Fourier's days that the series

$$\frac{\sin t}{1} + \frac{\sin 2t}{2} + \dots, \quad (1)$$

when  $t=0$ , can have any value that  $\int_0^x \frac{\sin x}{x} dx$  can take"; and maintains that there is no way of avoiding the conclusion that the value of the series is, then, zero.

Difference of opinion on the point in question is wholly due to the fact that the way one chooses to look at it is entirely a matter of arbitrary definitions which one can lay down as one likes. Zoologists might, *e.g.*, define as insects any non-aquatic animal in which the adults weighed less than one

<sup>1</sup> Longmans, Green & Co., 1925.

ounce. What a delightful opportunity this would give critics of denouncing an author who *preferred* to place shrew-mice in the same class as elephants, instead of with grass-hoppers!

If one *chooses* never to look at a series of *functions* till some constant value has been given to the variable, one can; but no one, save an armchair pedant, would be in the least interested in such a series apart from interest in the *function* it represented. *The* important thing about an infinite series of functions of a variable is that it represents *some function* of that variable; the infinity of the number of terms concerned is merely a trivial and incidental matter, due generally to arriving at the desired function by a method of successive approximation. Other methods of working might, *e.g.*, have given the same function in a finite form; say as a definite integral or a rational fraction. Unfortunately most writers on pure mathematics regard the infinity of the terms as the all-important thing to pay attention to; and pay only incidental regard to the *function* the series represents; otherwise they would not apply to a series of *functions* the definition they give as the sum of a series of *constants*; *viz.*, that it is that constant which is the limit of the sum of the first  $n$  constants when  $n$  is infinite; but would give a new definition, *viz.*, that the sum of an infinite series of *functions* is that *function* which is the limit of the *function* given by the sum of the first  $n$  terms when  $n$  is infinite.

To arbitrarily, and in defiance of common sense, ordain that before an infinite series of functions is evaluated for any particular value of the variable, that value must first be substituted in all the terms of the series, is every bit as ridiculous and illogical as to say that one must not attempt to evaluate

$$\frac{\sin x}{x} \quad \text{or} \quad (1 + xy)^{\frac{1}{2}}$$

when  $x = 0$  till one has first put  $x = 0$  in these expressions. Afterwards, of course, one can make nothing out of these expressions; and so their behaviour would for ever elude us, just as does that of the series (1) if we first put  $t = 0$ .

Mr. White also dislikes the way I have ignored the subject of non-uniform convergence; and doubts whether I have ever heard of it. I ignored it purposely, and very deliberately, because I think it is a very trivial matter indeed and one that many textbooks and teachers make a ridiculous mountain of fuss and trouble about.

*The* important thing about any infinite series of functions of  $x$  that is non-uniformly convergent at  $x = a$ , is not its non-uniform convergence there, but the fact that the series represents what I call an "artificial function" and that on the two sides of  $x = a$  the series represents two different analytic

functions.<sup>1</sup> That the non-uniform convergence at  $x = a$  is a very trivial and incidental thing can be seen from the fact that since at  $x = a$  the convergence is infinitely slow and the sum is finite, each term must vanish when  $x = a$  and is therefore divisible by  $x - a$  or some power of it. If we perform this division on each term we get another series which shows no non-uniform convergence at  $x = a$ , but is merely divergent there. Nevertheless, the sum of the series on the two sides of  $x = a$  represent two different analytic functions, and the nature of the infinity is quite different from the infinity of an analytic function, say, *e.g.*, of

$$\frac{1}{x-a} \quad \text{or} \quad \frac{1}{(x-a)^2}$$

when  $x = a$ . Also, if we multiply each term of the series by  $x - a$  we likewise destroy its non-uniform convergence in the textbook sense; since now when  $x$  only differs from  $a$  by an infinitesimal amount the sum of the series only differs from zero by an infinitesimal amount, and so there is nothing for the textbooks to make a fuss about; but the sum is still an artificial function, though now one with a discontinuity of *slope*, instead of *magnitude*, at  $x = a$ .

In every case of non-uniform convergence the complete behaviour of the series at the point concerned is easily found by reducing the series to an integral. Let us take the textbook example of

$$x^2 + \frac{x^4}{(1+x^2)} + \frac{x^6}{(1+x^2)^2} + \dots \quad (2)$$

when  $x = 0$ ; we call  $x^2$  an infinitesimal, say  $dt$ , and then remember that the term  $\frac{x^2}{(1+x^2)^n}$  must be brought into the form of  $f(n \, dt) \, dt$ , which must be true for any *finite value* of the argument  $n \, dt$ . The term obviously becomes  $e^{-n \, dt} \, dt$ , and so the series becomes—

$$(1 + e^{-dt} + e^{-2dt} + \dots + e^{-ndt} + \dots) \, dt;$$

the value of which is obviously

$$\int_0^{ndt} e^{-t} \, dt \quad \text{or} \quad \int_0^T e^{-t} \, dt,$$

$$\text{i.e. } 1 - e^{-T},$$

<sup>1</sup> In special cases these two functions may be the same function, as in series (2), below, the sum of which is  $1 + x^2$  when  $x$  "is not equal to" 0; but there is still the discontinuity that always marks the junction of two different analytic functions, though in this case it is masked. Really, the function represented by (2) falls from unity to zero and rises to unity again as  $x$  passes through zero; as will be seen from the investigation below.

where  $T$  is the product of the infinitesimal value given to  $x^n$  and the number,  $n$ , of terms of the series taken. The series (2) can thus take any value from zero to unity when  $x^n = 0$ . The value corresponding to  $T$ , or  $n dt$ , equal to zero, I call the *formal* value when  $x = 0$ , and the value corresponding to  $T$  equal to plus infinity I call the *principal* value when  $x = +0$ . Since the series is a function of  $x^n$ , the principal value when  $x = -0$  is, in this case, the same as the principal value when  $x = +0$ .

Surely there is nothing "mysterious or difficult" in thus determining the behaviour of an infinite series at the point of its non-uniform convergence, though the textbooks don't do it.

This phenomenon, in the series (1), has become known as Gibbs's phenomenon, owing to his having pointed it out many decades after it ought to have been known to everyone. And still the attempts of some writers who try to square the existence of Gibbs's phenomenon—which is really that the series (1) can, and does, possess all the values lying between  $-1.8519$  and  $+1.8519$  when  $t = 0$ —with the fact that it is uniquely zero when  $t = 0$ , seem to me both painfully amusing and confusing.

All this confusion I have tried to avoid. Moreover, the views I have adopted don't even depend upon non-uniform convergence at all, as two further examples will show.

First, what is the value of the function

$$\int_0^\infty \frac{\sin xt}{t} dt \dots (3)$$

when  $x = 0$ ? Mr. White would probably say zero, inasmuch as the integrand is zero. But to what student would it be clear that this is so, since the integrand is positive from  $t = 0$  to  $t = \pi/x$ , and is of the order of magnitude of  $x$  throughout this range?

I should say, writing  $xt = u$ , (3) becomes

$$\int_0^{x\infty} \frac{\sin u}{u} du;$$

which is  $\frac{\pi}{2}$  if  $x$  is positive; while when  $x = 0$  the expression is a function of the quantity " $x\infty$ ", which can then assume any value we choose to give it; and consequently (3), when  $x = 0$ , can have any value that

$$\int_0^{x\infty} \frac{\sin u}{u} du$$

can take; *i.e.* any value between  $-1.8519$  and  $+1.8519$ .

Secondly, what is the value of the function defined by

$$\lim_{n=\infty} \frac{a+b}{a+b x^{2n}} \dots (4)$$

when  $x = 1$ ? Mr. White would probably answer unity simply, and if I regarded  $x$  as merely some constant I should agree with him; but looking on (4) *qua function of  $x$*  it seems to me ridiculous to do anything else than say that for  $0 < x < 1$  the function has the value  $(a+b)/a$ ; for  $1 < x < \infty$  the function has the value zero; while when  $x = 1$  the function takes all values between  $(a+b)/a$  and zero; since this is the behaviour of the *function*

$$\frac{a+b}{a+bx^{2n}}$$

when  $n$  tends to infinity.

In this case too, it is also convenient to call unity the *formal* value of the function when  $x = 1$ ,  $(a+b)/a$  the *principal* value when  $x = 1 - 0$ , and zero the *principal* value when  $x = 1 + 0$ . There is no question of non-uniform convergence here: it is merely a matter of adopting a natural or pedantic way of looking at something.

It is not due to any lack of appreciation of the life's work of either Stokes or Gibbs that both their names were deliberately omitted from the brief historical survey at the end of my book.

Finally, I may perhaps say that my book is *purposely* intended to be a slight protest against some modern writers, who, I feel, simply make mathematics boring and repulsive to students by their absurd logical rigour; as if the subject-matter of mathematics were of no interest or value whatever—the only thing of value lying in following their methods of arriving at the results. This is to degrade mathematics into a particularly useless and complicated branch of logic. I have, on the contrary, tried, in the small scope that that book gave me, to impart some interest to my readers in *the behaviour of mathematical functions with explanations*, rather than proofs (which are largely out of place for people who are engaged in learning the subject-matter), of *why* they behave as they do. It is explanations, not proofs, that turn a body of dry facts into an interesting science.

Yours faithfully,

ALBERT EAGLE.

VICTORIA UNIVERSITY OF MANCHESTER.

August 9, 1926.

## STATISTICAL METHODS FOR RESEARCH WORKERS

From R. A. FISHER, Sc.D.

DEAR SIR,—The kindly notice of my book on *Statistical Methods for Research Workers*, which appears on pp. 733–4 of the April number of SCIENCE PROGRESS, contains one sentence which, if uncorrected, might give rise to some misapprehension,

and that on an important point of statistical theory. "E. S. P." writes :

"Again, a long-established method such as the use of the correlation ratio is passed in a few words without adequate description, which is perhaps hardly fair to the student who is given no opportunity of judging its scope for himself."

May I point out that my sin, if I am at fault, is one of commission, not of omission. I warn the student as plainly as I can that (p. 219) "As a descriptive statistic the utility of the correlation ratio is extremely limited." This conclusion (with which I cannot, of course, expect "E. S. P." to agree) was not formed without laborious examinations of the theory and practice of this "long-established" method, as the result of which I *was* able to establish (1922) the true distribution of the sampling errors of this statistic, and so to investigate the pitfalls into which eminent biometricians had repeatedly fallen. The three pages given in my book to the correlation ratio and Blakeman's criterion, are there simply to warn the student against a roundabout process, which has already wasted too much valuable time. I do little more than indicate the main reason for the failure, both of the ratio, and of the criterion, namely, that the sampling distributions are not merely modified by, but are wholly dependent on, the number of arrays, and that this number is left entirely out of account in both cases.

Yours faithfully,

R. A. FISHER.

ROTHAMSTED EXPERIMENTAL  
STATION, HARPENDEN.

June 18, 1926.

## ELEMENTS OF PHYSICAL BIOLOGY

From DR. ALFRED J. LOTKA

DEAR SIR,—The review of *Elements of Physical Biology* which appeared in a recent issue of SCIENCE PROGRESS is based on a fundamental misunderstanding of the method and purpose of that work. As this misunderstanding is calculated to mislead the reader, may I be permitted to offer a correction through your columns?

In the first paragraph my effort is spoken of as a "*mechanical theory of evolution.*" This hardly describes either its intent or its effect. I point out in the introductory chapters that certain mechanical properties or situations are characteristic of the one-sided time trend (evolution) of those systems whose physics are clearly understood. It seems altogether proper that the student of evolution should be made aware, or reminded, of this fact. For, on the one hand, the very concept

of evolution is inevitably linked with the concept of one favoured direction in time ; on the other hand, the examination of the meaning of this favoured direction in time can hardly be approached with competence without reference to mechanics. But it should be clearly noted that the subsequent portions of the book are in themselves independent of the facts and relations set forth in the sections devoted to the physical meaning of irreversibility. The reader who is willing to accept the one-sided direction of the progress of time as axiomatic, and to shut his eyes to the intriguing problem of the significance of his axiom, may skip the sections dealing with this topic, and will not thereby be hampered in his reading of the matter that follows. This is not saying that he will lose nothing by the omission.

The second misunderstanding is contained in the reviewer's sentence, referring to the fundamental system of equations : " A system of equations of this type is *apparently* chosen *owing to the analogy* with ' completely damped ' systems in dynamics." The system of equations in question was chosen, not on account of any analogy, but simply because it is the most natural system to choose, as anyone will see upon reflection. It simply states that the rate of growth of a species is *uniquely* determined by the existing number of that species, and of all other species that enter into relation with it, and, finally, by the state of the system as defined by such conditions as climate, etc., as well as by the character of the several species. (This latter being, in general, susceptible of change with time.) The equations given are simply the same statement expressed in analytical symbols, it being expressly pointed out that the functions  $F$  are single-valued, so that the velocities are uniquely determined. It is this single-valued character of the functions  $F$  that imparts to the system its resemblance to " completely damped " systems of dynamics. The analogy follows from the assumptions, it is not itself the reason for choosing those assumptions. The reader need only for a moment consider the alternative—that the velocities are *not* uniquely determined by the factors named, and it will immediately be seen that this latter assumption does not commend itself, that the one actually made is essentially the natural one to make, and that for this we need no guidance from analogy to physico-chemical systems. That analogy exists, that reaction velocities in physico-chemical systems also are *single-valued* functions of the masses of the reacting substances, the volume and the temperature, are facts of nature, for which the author can hardly be made responsible.

The next misunderstanding is the logical consequence of the first. The reviewer being under the impression that my intent was to propound and seek to establish a mechanical

theory of evolution, he now construes my examples of numerical applications as efforts on my part to prove a theory. As well might one regard the working examples in a textbook of differential equations as an effort to prove the theory of radioactive transformations. Here is a total misunderstanding of my true purpose. What the illustrations given are intended to prove is no *theory* whatsoever, but merely the simple fact that the general *method* of solving the fundamental equations yields, as special cases, the solutions of equations set up independently by others for particular instances.

Regarding the sigmoid curve for population growth obtained by the method illustrated, the reviewer remarks that such a sigmoid curve can be fitted also by a cubic parabola, which latter "is not obviously derivable from [my] fundamental equations." To this I can only reply that if it were so derivable I should throw my fundamental equations on the scrap-heap, for a cubic clearly gives absurd values of  $x$  for large positive or negative values of  $t$ .

As to the discrepancy in the two estimates of the rate of circulation of water in nature, anyone reading the review will probably obtain the impression that this discrepancy is a discovery of the reviewer. Reading the book it will be found that I particularly pointed out this discrepancy. It is a well-established principle that negative results and failures should be recorded, as well as positive ones, and successes.

As for speculations and reasoning by analogy, I have expressly warned against these (see pages 258, 274, 283, 288, 289). Since the reviewer is under the misapprehension that I am seeking to prove a "mechanical" theory—whereas I am merely expounding a method of drawing conclusions by mathematical analysis from certain simple assumptions, the conclusions standing or falling according as these assumptions themselves are or are not applicable—since he is under this misapprehension, probably much of what he has taken for speculation is merely the statement of certain necessary conclusions that hold when, and only when, the underlying assumptions apply. The author cannot be made responsible for application beyond these limits made by others who may disregard the plain statement of the range of applicability. It is expressly stated on page 47 of the work reviewed that my "fundamental equations" are not expected to cover all cases; and it should hardly be necessary to point out that the special case of evolution under conditions of constant parameters  $P$  and  $Q$  must not be confused with the more general scope of the equations interpreted without this restriction.

Yours faithfully,

ALFRED J. LOTKA.

NEW YORK, U.S.A.  
June 15, 1926.

## REVIEWS

### MATHEMATICS

**Plane Curves of the Third Order.** By HENRY SEELY WHITE. [Pp. xii + 168]. (Cambridge, Mass.: The Harvard University Press, and Oxford: The University Press, 1925. Price 12s. 6d. net.)

PROF. WHITE has not very much new to say about plane cubics, but he gives an admirably clear account of the classical theorems. The difficulty about a book of this kind is to discover the class of reader for whom it is intended. Personally, the reviewer feels that there is quite enough about the cubic for the general reader in Salmon or Hilton; the specialist had better read Schroeter (he will have to learn German sooner or later, so why not begin on this?) and, in any case, it would be much better for him to get some general ideas on geometry and curves first, so that he may apply his general theorems to this particular case. What Prof. White does is to assume nothing; he proves theorems only for the special case of the cubic, and he explains about the line at infinity, the number of points in which a straight line meets an algebraic curve and so on, with reference to Todhunter's *Integral Calculus* and to Burnside and Panton, so that the intelligent reader, scanning the index as a preliminary, wonders what on earth these authors contributed to knowledge of this particular subject. Still, the book may prove of use in showing that geometers do know something beyond conics, and that cubics are not absolutely incomprehensible.

F. P. W.

**The Theory of Functions of a Real Variable and the Theory of Fourier's Series.** By E. W. HOBSON. Second Edition, revised throughout and enlarged. Volume II. [Pp. x + 780.] (Cambridge: At the University Press, 1926. Price 50s. net.)

THE second volume of the new edition of Prof. Hobson's *Functions of a Real Variable*, which has been eagerly expected since the publication of Vol. I in 1921, has at length appeared, and it fully justifies our anticipations. It is surely a matter for self-congratulation that we possess such a magnificent account of the whole theory in English, so well presented and so excellently printed. It would be impertinent for a reviewer, without months of continual use of this book, to pretend to criticise it as a whole; we can only give a brief summary of the topics treated.

The first three chapters deal with the theory of convergence sequences and series, numerical in Chapter I, with terms which are functions of one or more variables in Chapters II and III. There follows an account of the Weierstrass theorem on the representation of continuous functions by sequences of polynomials and of Baire's fundamental result on the representation of a function as the limit of a sequence of continuous function. Various part of the theory of integration, which were not examined in Vol. I, are next considered, and here we may note how up-to-date Prof. Hobson is, as there is a discussion of the theory of integration due to Torrelli, which was published in the *Annali di matematica* so recently as 1924. Chapter VI gives the construction of functions showing assigned peculiarities, especially of non-differentiable continuous functions.

Prominence is given to a General Convergence Theorem, which is developed in Chapter VII, with a view to its application to the Theory of Fourier Series and Integrals, and Fourier transforms, which occupies very nearly half of this volume. These chapters afford abundant evidence of the great activity in this direction since the publication of the first edition in 1907, but, as Prof. Hobson remarks in his preface, "there still remains for solution at least one fundamentally important question which has hitherto baffled all attempts at settlement." An entire chapter has been added on the representation of functions by series of normal orthogonal functions.

F. P. W.

**Probabilités Géométriques.** Par R. DELTHEIL. (Traité du Calcul des probabilités et de ses applications, par E. BOREL, t. II, fascicule 2.) [Pp. 123.] (Paris: Gauthier-Villars et Cie, 1926. Price 22 fr.)

WHATEVER may be the philosophical bearing of the theory, there is no doubt that the subject of geometrical probability has given rise to interesting mathematical problems. Following Poincaré, the "elementary probability" upon which calculations are based depends upon an arbitrary positive function, and in general the result will depend upon the choice of this function, though Poincaré himself showed that in certain problems, notably that of the roulette, with very wide conditions on the function, the result is independent thereof. M. Deltheil, in this interesting little book, which forms part of M. Borel's large treatise on Probability, attaches the choice of the arbitrary function to the theory of groups; suppose we have group  $G$  of transformations of the variables  $x_1, x_2, \dots, x_n$ , two domains of this  $n$ -dimensionable space will be said to be equivalent with respect to the group  $G$  if there is at least one operation of the group which transforms one into the other. Then if it is possible to find a differential  $dJ = F(x_1, x_2, \dots, x_n) dx_1 dx_2 \dots dx_n$  of which the integral has the same value for any two equivalent domains, then  $dJ$  is taken as the elementary probability with regard to the group  $G$  for problems of geometrical probability in the space  $(x_1, x_2, \dots, x_n)$ .

With this definition M. Deltheil considers problems relating to points taken at random on a segment of a straight line, in a region of a plane or in a three-dimensional region of space, the group  $G$  being here the group of displacements. He obtains a formula due to Crofton, which gives a means of avoiding the calculation of multiple integrals which the direct treatment would lead to, and he gives a discussion of the well-known problem of Sylvester: given a convex domain in a plane, what is the probability that four points taken at random form a convex quadrilateral? He then goes on to problems concerning straight lines in the plane, beginning with Buffon's needle, which may be said to have begun the subject, discussing an allied problem: given two convex closed curves  $C_1$  and  $C_2$ , what is the probability that a secant of  $C_1$  taken at random cuts the curve  $C_2$ , and finishing the chapter with an account of work due to Crofton (1886) which, says the author, "trop peu comme en France," and, we may add, in England also. The last two chapters give various problems in three dimensions, and on the sphere in  $n$  dimensions. The whole makes an interesting book, well worth reading, even though the subject seems a little artificial.

F. P. W.

**Traité de Mécanique Rationnelle.** Par P. APPELL. Tome V: Éléments de Calcul tensoriel. Applications géométriques et mécanique, avec la collaboration de R. Thiry. [Pp. vi + 198.] (Paris: Gauthier-Villars et Cie, 1926. Price 40 frs.)

THIS book forms the fifth volume of M. Appell's monumental treatise on mechanics, of which the first three have long been the stand-by of every

applied mathematician, and the fourth, which only appeared in 1921, and which treats the fascinating subject of rotating liquids, was a very welcome supplement. It also is intended as the first part of a treatise on Relativity mechanics, but it does not touch at all upon Einstein's theory. It is in fact, as the title says, an account of the tensor calculus, which, once acquired, can be applied to a varied range of subjects, in differential geometry as well as in dynamics. A preliminary chapter recalls the elementary theory of linear and quadratic forms; it is printed in smaller type than most of the book to indicate that it is not meant for the accomplished mathematician. Following on the main part of the book, Chapter II, are applications of the theory to classical mechanics and to Euclidean and Riemann geometry of three and, afterwards, of  $n$  dimensions. An account is also given of Weyl's geometry and of the work of Cartan. A final chapter, again in smaller type, gives a rather unsatisfactory conspectus of Cayley's Absolute Geometry.

The book is a valuable addition to the literature of the subject.

F. P. W.

**The Geometry of René Descartes.** Translated from the French and Latin by D. E. SMITH and M. L. LATHAM. With a Facsimile of the First Edition, 1637. (London and Chicago: The Open Court Publishing Company, 1925. Price 17s. 6d. net.)

THE *Géométrie* of Descartes was published, as is well known, as the last of three *Essais* appended to the *Discours de la Méthode pour bien conduire sa raison et chercher la vérité dans les sciences*, Leyden, 1337. After the first two *Essais*, the *Dioptrique* and the *Météores*, Descartes inserted the following *Avvertissement*: "Jusques icy j'ay tasché de me rendre intelligible à tout le monde, mais pour ce traité je crains, qu'il ne pourra estre leu que par ceux, qui scauent desia ce qui est dans les liures de Géométrie. car d'autant qu'ils contiennent plusieurs vérités fort bien démonstrés, j'ay creu qu'il seroit superflus de les repeter, et n'ay pas laissé pour cela de m'en servir." Even so, such was the obscurity of the work that almost immediately he had to authorise one of his Dutch friends (probably Godefrid of Haestrecht) to write an Introduction to make it more intelligible to geometers (such as Desargues) themselves. This Introduction was discovered as recently as 1894 among some papers of Leibniz at Hanover, and is published in Volume X of the magnificent edition of Descartes by Adam and Tannery. M. Adam speculates on Descartes' reasons for this obscurity, clearly intentional, and comes to the conclusion that, distrustful and suspicious to a degree, he feared the originality of his work would not be recognised if he made it too easy, and that his inventions would be appropriated. Also he took pleasure in seeing, from his retreat in Holland, his rivals in France, Fermat, Etienne Pascal, Roberval in the grip of difficulties which they could not surmount.

The *Géométrie* was omitted from later editions of the *Discours*, but a Latin translation by Franciscus a Schooten, with notes by Florimond De Beaune, appeared in 1649 (another edition 1659), and a separate French edition in 1664. But apparently it has never before been translated into English, and we owe a debt of gratitude to the Open Court Company for this very fine edition, which gives a facsimile of the French of 1637, a competent English translation, and some necessary notes and explanations. The only serious blemish is the lack of a short bibliographical note, which ought not to be difficult to compile—the sentence, in the Preface, "the epoch-making treatise of Descartes has never been printed in our language, or, if so, only in some obscure and long-since-forgotten edition," apart from its journalese, is not good enough for a precise, scientific work.

Perhaps a brief account of this classic of analytical geometry, which everybody has heard of, but which few have read, may not be out of place.

The work is in three books, each of which may be roughly divided into two parts, theory and applications. Descartes begins the first book by a brief indication of his algebraic notation, familiar enough to-day, but a great reform (foreshadowed by Vieta) in getting rid of the cabalistic "cossic" symbols. M. Adam quotes from a letter of Descartes the same equation in the two forms— $1C - 9Q + 13N$  e.g.  $\sqrt{288} - 15$  and  $y^3 - 9yy + 13y - 12\sqrt{2} + 15 = 0$ . But the great innovation is of course the introduction of arithmetic into the operations of geometry—a conscious innovation, for he refers to "le scrupule que faisoient les anciens d'user des termes de l'Arithmétique en la Géométrie" which "causoit beaucoup d'obscurité et d'embaras." (This passage, by the way, is quite wrongly translated in this edition, pp. 20-21.) This bridging of the gap evoked the comment (quoted by M. Adam) from Ciermans, the Jesuit Professor of Mathematics at Louvain, that the title "Géométrie" said too little; it should have been "Mathématiques." To which Descartes replied that then it would have said too much, since mathematics includes mechanics, which he had not treated.

The application in the first book is to the problem of Pappus, which may be stated as follows: To find the locus of a point such that if from it straight lines be drawn making given angles with certain fixed lines in the plane, the product of some of them shall bear a given ratio to the product of the others. Descartes contents himself with enunciating the problem in Latin, sketching the solution, and indicating the equations which arise and the kind of curves which are found according to the various cases. But for this something must be said upon the nature of curves; this is done in Book II, the most important part of the work. But Descartes advised readers, Mydorge, for example, to pass on to Book III, and we may as well follow this advice, and leave Book II to the end.

Book III is practically a treatise on algebra, or, at least, the theory of equations. We may mention some of the topics dealt with. An equation can have as many different roots as its degree, but it often happens that some of these roots are false, i.e. negative. Again, neither the true nor the false roots need be always real; they may be imaginary. This is the first occurrence, according to Cantor, of the antithesis of the words "real" and "imaginary" in this connection, although the idea itself was not new. The left-hand side of the equation can always be divided by a binomial consisting of the unknown diminished by one of the true roots or increased by one of the false. Then comes the well-known "Rule of Signs"—"On connoist aussy de cecy combien il peut y auoir de vrayes racines, & combien de fausses en chasque Equation." Descartes also shows how to increase the roots of a given equation by a given quantity, and thus how to remove the second term; he discusses the cubic equation, comparing his solution with Cardan's. He then gives the construction of a curve, which he calls "parabole du second genre," as the locus of the intersection of a variable parabola and a variable straight line, by means of which a sextic equation can be solved, and he applies it to finding four mean proportionals between two straight lines, and to dividing an angle into five equal parts, notable extensions of those two problems of antiquity, the duplication of the cube and the trisection of an angle.

But to return to Book II, on the nature of curves. Descartes begins with a distinction between what are now called "transcendental" curves and "algebraic" curves—he calls them "mécanique" and "géométrique"—stating that the points of one of the latter "ont necessairement quelque rapport a tous les points d'une ligne droite, qui peut estre exprime par quelque equation, en tous par une mesme." He then classifies curves according to the degree of this equation, but not in the simple, natural way (which apparently is due to Newton): but artificially enough. If the equation contains no terms of degree higher than the second, the curve is of the first class

(containing the circle, the parabola, the hyperbola, and the ellipse); if it contains terms of the third or fourth degree, the curve is of the second class, and so on, going up by twos. He then shows how to construct these curves by points, and goes on to say that, knowing the equation of the curve, it is an easy matter to find its diameters, axes, centre, and so on. He does not stay for this, but passes on to tackle what is in effect the famous "problem of tangents," "le problemes le plus utile et le plus général non seulement que je scache, mais mesme que j'aye jamais désiré de scavoir en Géométrie." What he actually does is to construct the normales. With any point of the diameter of the curve (e.g. a parabola as centre, he describes a circle cutting the curve in two points. If the radius of the circle is diminished, these two points get closer together, and when the circle touches the curve the two roots of the equation giving what we should call the abscissæ of the two points will have equal roots. Later, in his correspondence, Descartes gave up this method for the one now more usual, in which a straight line from a fixed point turns about this point, and from a secant becomes a tangent; this he got from Fermat.

The remaining part of Book II deals in some detail with various cases of the problem of Pappus, and then with the Cartesian ovals, which he had referred to in the *Dioptrique* as being important for the construction of lenses. Finally, we have in a short paragraph a hint of the possibility of the extension of the method to solid geometry—"comment on peut appliquer ce qui a esté dit icy aux courbes qui se descriuent dans une espace à trois dimensions."

And he concludes, "ainsi je pense n'auoir rien omis des elemens, qui sont necessaires pour la connoissance des lignes courbes."

May one finish with a final quotation, the last sentence of the whole work: "j'espere que nos neveux me scauront gré, non seulement des choses que j'ay icy expliquées; mais aussy de celles que j'ay omises volontairement, affin de leur laisser le plaisir de les inuenter." F. P. W.

**The Mathematical Theory of Electricity and Magnetism.** By J. H. JEANS, D.Sc., LL.D., F.R.S. Fifth Edition. [Pp. viii + 652.] (Cambridge: at the University Press, 1925. Price 21s. net.)

THE appearance of a new edition (the fifth) of Jeans' *Electricity and Magnetism* is a notable event in the literature of this subject. Since its first appearance in 1907 the book has held its place as the authoritative textbook on the mathematical theory of electricity and magnetism, and the present edition, which contains an additional chapter on the electrical structure of matter, brings the work up to date.

It is difficult, if not impossible, for a pure physicist to be *au fait* with the whole of the matter contained in this work, demanding for its complete understanding a mathematical equipment quite beyond the average, but one of the remarkable features of the book is that the physicist can pick here or there for parts of the theory which most concern him, and obtain a clear exposition of a particular point, without having studied the book systematically. The same convenience is also available from the mathematical side also, as for example in the section dealing with Spherical Harmonics, Curvilinear Co-ordinates, Confocal Co-ordinates, and so on.

An outstanding difficulty which confronts all teachers and students of electricity is the confusion arising from various uses of the word "induction." In this book we have "induction" in the connection "electrification by induction" (p. 16), and in the cognate conception of coefficients of induction; again we have "magnetic induction" (p. 384), this time denoting a vector with special properties, and, thirdly, we speak of the "induction of currents" (p. 452). Other writers introduce the conception of "electric

induction" in dealing with the theory of polarised dielectrics: a conception which is parallel to Dr. Jeans' introduction of "polarisation" (p. 117). The writer of this review dares to hope that some day Dr. Jeans may take compassion on the labours of lesser men and really straighten up this nomenclature in a concise exposition of the theory of polarised media, which will cover both electrostatic and magnetostatic phenomena without ambiguity, and indeed without quoting Faraday verbatim.

Green's Reciprocation Theorem (p. 92) provides another example of the difficulties of the student: "We need only notice that

$$V_p = \sum \frac{e_q}{PQ}$$

the summation extending over all charges except  $e_p$ ."

"Why," says the student, "surely the potential due to the very near charge  $e_p$  is large in comparison with contributions arising from distant charges  $e_q$  etc.?" The query is natural, if it hasn't been previously explained that the potential integral is absolutely convergent, whereas the intensity integral is at the best semi-convergent. The difficulty is of course closely allied to the one previously referred to, namely, that of a correct understanding of the behaviour of electric quantities in close neighbourhood of charges or charged molecules.

Much of the mathematical technique would be more easy to handle if vector analysis could have been introduced. To expect this is to ask too much, for naturally such a change would mean a complete rewriting of the book—could one dare to hope that such a change might take place eventually? If it does, it is to be expected that the Heaviside rational system of units will be adopted throughout.

The type, lay-out, and binding of the book are excellent and we are grateful to Dr. Jeans for this work, which is representative of the great tradition of Faraday and Clerk-Maxwell.

R. C. RICHARDS.

## PHYSICS.

**Introduction to Physical Science.** By IVOR B. HART. [Pp. xii + 306, with 199 figures.] (Oxford: at the Clarendon Press, 1925. Price 4s. net.

THIS book is intended for schoolboys who will, the author hopes, be attracted to natural philosophy by pretty photographs of other boys doing pin optics or gazing steadfastly at Boyle's Law tubes. The bulk of the book deals with mensuration and mechanics, but there are short sections at the end on heat, sound, light, and magnetism and electricity. To the latter two subjects twenty-eight profusely illustrated pages are allotted. Such treatment of important subjects is apt to lead the young into the most dangerous of all intellectual states, the state of thinking that they understand a subject, and then becoming too self-satisfied to perform the necessary hard work which would enable them to realise their own ignorance.

The first section, on the measurement of lengths, insists on two decimal places continually, and talks about the *correct* length of a line. This is surely a bad introduction to physical science. On the other hand, the hydrostatics section is good and contains many useful exercises.

The paper and type are both excellent.

J. H. S.

**X-rays.** By MAURICE DE BROGLIE. Translated by J. R. CLARKE, M.Sc., F.Inst.P. [Pp. xii + 204, with 37 figures and 7 plates.] (London: Methuen & Co., 1925. Price 12s. 6d. net.)

THIS book is an excellent translation of one of M. de Broglie's contributions to the useful series of monographs produced by the publishers of the *Journal*

*de Physique*, which was amplified from a series of lectures delivered in Paris in the years immediately following the war. The original idea was to provide refresher courses for physicists who had lost touch with their subject during the war period. The monographs are suited to a much wider public, since they give connected accounts of the developments in the various branches of physics, and are therefore very useful to beginners in post-graduate experimental work, as well as providing handy works of reference for all research workers. The unavoidable weakness of the English translations is that they cannot contain an adequate account of the most recent work on the subject. In the present volume this weakness has been to some extent mitigated by the addition of two appendices; the first is on the refraction and total reflection of X-rays, and the second on  $\beta$ -ray and  $\gamma$ -ray spectra. Both subjects are worth fuller treatment than it has been possible to give them in the present volume.

Beginning with an introductory account of Bohr's theory, the book gives a complete account of the interaction of matter and X-rays and of the continuous and line spectra. The chapter on line spectra contains complete tables of wave-lengths as determined by various workers. A chapter on spectrographs and spectrometers follows this, together with a short account of the various forms of X-ray tube. The concluding chapter deals with secondary electrons due to X-rays and  $\gamma$ -rays. A useful bibliography is given at the end of each chapter.

The plates are well up to the standard which is associated with the name of de Broglie. It is to be regretted that the book does not give a fuller description of the technique which produces such fine photographs.

J. H. S.

## CHEMISTRY.

**Three Centuries of Chemistry.** By IRVINE MASSON, M.B.E., D.Sc., F.I.C. [Pp. 191.] (London: Ernest Benn. Price 10s. 6d.)

It is rare that a book on the history of chemistry stimulates so much interest as the above volume by Prof. Masson. Although the reader may not subscribe to all of the views of the author on the development of science in the seventeenth century, he will find that the subject is presented from a new and interesting angle, for in the text is embodied the results of a detailed research into the part played by Francis Bacon and the founding of the Royal Society in preparing the way for modern science. Perhaps the author has overemphasised the Baconian influence on this period, but his attitude is preferable to that of the scientific world of the present day, which has allowed the tercentenary of one of the greatest figures in our history to pass without any adequate notice.

The author has aimed at recreating the atmosphere of the periods which he surveys, rather than the presentation of the dry bones of historical fact. In this object he has been successful, particularly in the glimpses he has given us of the conflict between the atomic theory of Boyle and the older theories of matter which held sway in chemistry from the time of the Arabs until their final overthrow at the hands of Lavoisier.

In the last fifty pages of the book the story is extended to the nineteenth century. The argument is closely reasoned, and the century is adequately surveyed, but there is an obvious discontinuity in the method of treatment after the time of Lavoisier. Here the author has not interwoven into his fabric those intimate biographical details which supply atmosphere to historical development.

Both the general reader and students of the history and methods of science will find much that is of interest in their perusal of this volume.

W. E. G

**Organic Syntheses, Vol. V.** By CARL SHIPP MARVEL, Editor-in-Chief. [Pp. vii + 110.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1925. Price 7s. 6d. net.)

THE fifth volume of this series maintains the high standard set by the previous four. Detailed instructions are given in it for the preparation of thirty-three organic chemicals. This volume includes new and improved directions for the preparation of ethyl oxalate (this preparation had been given in a previous volume). Details for the isolation of two amino acids are also included. The instructions for each preparation are given under the three heads: I. Procedure, II. Notes, III. Other Methods of Preparation. They are clear and sufficiently detailed to enable the experiment to be carried out, and the stated yield obtained, without reference to the original literature. The details for each preparation are checked by independent chemists before being published. A most useful collective index to the five volumes is given at the end of this volume.

J. N. E. D.

**The Use of Solvents in Synthetic Organic Chemistry.** By DONALD W. MACARDLE. [Pp. vii + 217.] (London: Chapman & Hall, 1926. Price 15s. net.)

THE scope of this book may be indicated by giving the titles of the ten chapters: I. General Considerations. II. Inorganic Solvents; Organic Solvents for Inorganic Salts. III. Alcohols as Solvents. IV. The Preparation of Absolute Alcohols. V. Higher Alcohols and Ethers as Solvents. VI. Organic Acids, Esters, Ketones, and Bases as Solvents. VII. Hydrocarbons as Solvents. VIII. Inert Liquids as Solvents; Solid Diluents. IX. Special Means of Inducing Crystallisation. X. Salting Out.

The writer of this book has undertaken a difficult task in collecting together this mass of details and dry facts, and preparing them for presentation in book form. In spite of the subject, the book is interesting to read, and the chemist will find that it contains many fruitful suggestions with regard to practical details of manipulation. Useful sections are given on the preparation of alcohols and the purification of commercial solvents. The book should prove useful to research and works chemists, who will be reminded of such things as the solubility of inorganic salts in organic solvents, and the different solvent power of wet and dry solvents.

The bibliography contains twenty-two pages of references. Frequently, in addition to the original reference, reference is made to one of the chemical abstract journals. In these cases it is noticeable that preference is given to American or German abstracts, the English abstracts being rarely referred to. Similarly, on page twenty-three, where reference is made to a method for the purification of T.N.T., the reference is to an American patent, and no reference is made to other workers on this subject who have shown that the reaction is a chemical one.

The facts contained in the book are rendered more easily accessible by the addition, at the end of the volume, of Indexes of Authors, Subjects, and Solvents.

J. N. E. D.

**The Synthesis of Benzene Derivatives.** By STANLEY C. BATE, B.Sc., F.I.C. [Pp. 229.] (London: Ernest Benn, 1926. Price 21s. net.)

THIS book gives examples of various methods employed for the preparation of derivatives of benzene. While the instructions are not sufficiently detailed to enable the syntheses to be carried out without reference to the original literature, the book gives a valuable summary of the methods

which are employed. As stated in the preface, this is an attempt to fill the gap between the ordinary textbook of preparative organic chemistry and the large detailed manuals of practical organic chemistry.

The book should be useful to students reading for honours in chemistry, and also to research workers. The cost appears to be high, considering the number of pages, and this will make it prohibitive to many students. It might be well to add an author index.

J. N. E. D.

**Certain Aspects of Biochemistry.** By H. H. DALE, F.R.S., J. C. DRUMMOND, D.Sc., L. J. HENDERSON, M.D., and V. A. HILL, F.R.S. [Pp. 313 + viii.] (London: University of London Press. Price 12s. 6d. net.)

THIS volume contains four short series of lectures, delivered at University College, London, during the session 1924-5. Only a few of the present-day problems are dealt with, but they are certainly amongst the most interesting, and each subject is presented by an authority. Dr. Dale, in the first four lectures, treats of the recent advances in the chemistry of the endocrine secretions, including the now famous Insulin. The new conceptions of the oxidative processes of the living cell are reviewed by Prof. Drummond, who devotes a chapter to some fascinating speculations on the significance of phosphates in the cell. An attitude novel to many is presented in a chapter by Prof. A. V. Hill on the physical environment of the cell.

The complicated physical and chemical mechanisms regulating the changes in the blood during the respiratory cycle, revealed by the brilliant work of Henderson and his colleagues, are described in as simple a manner as the intricacy of the subject permits, in three chapters from the pen of Prof. Henderson.

We recommend this book to biochemists and physiologists who would keep in touch with recent developments of biochemistry with a minimum of effort.

P. E.

**Practical Physiological Chemistry.** By SYDNEY W. COLE, M.A. [Pp. xii + 479.] Seventh Edition. (Cambridge: W. Heffer & Sons, 1926. Price 16s. net.)

THIS latest edition, while maintaining the character of all the previous editions, once more establishes the fact that the book is an eminently sound and practical one; full of useful hints and suggestions, it can always be relied upon to describe methods and experiments which will work. Further than this, the author has very wisely incorporated a good deal of theoretical matter in amplification of the purely experimental work, with which the book is primarily concerned; this adds very materially to the value of the work, since the author has in a marked degree the power of clear exposition, which he develops to good purpose in elucidating such subjects as reduction potentials and the theoretical basis and the practical measurement of hydrogen ion concentration. Other features include a chapter on biological oxidations and reductions, a considerably extended account of the chemistry of hæmoglobin and its derivatives, and a new chapter on the analysis of blood. The book may be thoroughly recommended, not only to students of physiological chemistry but to those requiring information on the application of physical chemistry to biological problems in general.

**The States of Aggregation.** By G. TAMMANN, translated from Second German Edition by R. F. MEHL. [Pp. xi + 297.] (London: Constable & Sons. Price 24s. net.)

WHILST the conditions of equilibrium between vapours and liquids were investigated by Andrews and Van der Waals, examination of more complex

systems involving equilibria between varieties of crystalline solids and liquids, as well as the nature of glasses, presented a series of formidable theoretical and experimental difficulties not lightly overcome. It is due chiefly to the work of Tammann and his school at Göttingen and Bridgman at Harvard that our knowledge has been so greatly increased.

Tammann employs the potential functions of Gibbs as the basis for the theoretical conditions of equilibrium. It is somewhat unfortunate that at the present time, especially in America, alternative symbols and functions are being so widely employed. It would be well if some international body could arrive at some unanimity in this matter.

The second edition of this book contains but little new matter, reference to the investigations of Smits, Cohen, and Schreinemakers might well have been included. The translation is marred by a number of misprints especially in well-known names such as Gibb for Gibbs, p. 6, van der Walls for van der Waals, p. 26. In addition, there exist many idiomatic expressions which might well have been employed for the German equivalent such as "arrest point" for a "point of halt," p. 126. With these exceptions, however, the English is clear and the printing good.

E. K. R.

### **La Détermination Colorimétrique de la Concentration des Ions Hydrogène.**

Par I. M. KOLTHOFF, translation into French of the 3rd German edition by E. VELLINGER. [Pp. xiv + 260.] (Paris: Gauthier Villars et Cie. Price 50 frs.)

THE subject of biochemistry, which makes a general appeal, possesses the peculiar faculty of bringing into prominence certain aspects of physical chemistry which might otherwise have been neglected. The great importance attached by the biochemists to hydrogen-ion concentration in biological fluids has led to a treatment of this subject much more extensive and detailed than was given to it by the chemist. In this volume a very full account of the properties of indicators and the correct method of determining the  $P_H$  of solutions and buffer mixtures is given, together with an extremely comprehensive bibliography.

At the present time much attention is being given to the influence of salts on chemical reaction rates and the section on the salt effect on indicators might well be extended. Chiefly due to the inspiration of Euler the amphoteric character of sugars and esters is now generally realised, a property of some importance in connection with the phenomena of mutarotation and hydrolysis; the slight reference on p. 193 scarcely does the matter justice. Biologists will be interested in the appendix on Vlès' method of determining the intra-cellular  $P_H$ .

It is somewhat surprising to find that W. Ostwald's explanation of colour change in indicators as due to an alteration in dispersity neglected. Whilst the Ostwald-Hantzsch view of tautomeric ionisation is probably correct for the majority of indicators, the alteration in dispersity of substances such as Congo rubin, protected gold, and even phenolphthalein with the  $P_H$  has been fully established.

The volume appears to be free from misprints and the printing may be regarded as fair.

E. K. R.

### **The Physical Chemistry of Steel-making Processes.** [Pp. 127, 20 figures, 4 plates.] (London: Gurney & Jackson. Price 8s. 6d.)

A GENERAL Discussion held by the Faraday Society and the Iron and Steel Institute, June 1925. Papers read by Sir Robert Hadfield (chairman),

Dr. A. M'Cance, Mr. T. P. Colclough, Mr. Frank T. Sisco, Prof. J. B. Ferguson, Dr. P. M. Macnair, Mr. J. H. Whitely, Mr. Alexander L. Field, and Mr. W. J. Rees.

The following contributed to the discussion: Prof. Donnan, Prof. Desch, Mr. Harbord, Dr. Hatfield, Dr. Rosenhain, Mr. Sarjant, Mr. Service, Mr. Whitely, Mr. Hallemond, Mr. Jackson, Mr. Herty, and Dr. Haakon Styri.

In several of the papers presented emphasis is laid on the fact that the chemical reactions upon which the manufacture of steel depends are *balanced* actions with definite equilibrium constants, which, moreover, are a function of the temperature. This point is made especially clear in the paper by Dr. M'Cance, in which he contrives to use the most recent of thermodynamic methods, including Nernst's Heat Theorem, to evaluate these constants and relate them to practice. Among other things he shows how the solubility of FeO in molten iron, recently established by Rosenhain, Hanson, and Tritton, makes it necessary for the molten metal to be deoxidised if there are to be no inclusions on cooling; and compares the deoxidising powers of manganese and silicon, as disclosed by the equilibrium equations, with their observed effect in producing sound ingots (Brinell). It is clear from the discussion that Dr. M'Cance's treatment does not completely clear up this matter, since hydrogen is given off by steel on solidifying to an extent which would suggest it is, at least in part, responsible for the production of blow-holes; and it is greatly to be hoped that he will extend his work to cover the solution of gases other than oxygen (for it does not seem possible to distinguish between FeO and O when the solvent is Fe.) At the same time the value of the work already done is illustrated by the light it throws on the important problem of the solubility of FeO in molten iron containing small quantities of dissolved carbon. In answer to a question, Dr. M'Cance pointed out that, in accordance with the equilibrium



the concentration at which FeO becomes saturated depends on the partial pressure of the CO in the gaseous phase as well as on the carbon concentration.

The Faraday Society thus adds one more to the list of its valuable discussions, which not only encourage manufacturers to make full use of scientific aid, but also demonstrate to pure scientists the fascination of the problems which industry presents.

K. SCHOFIELD.

## BOTANY.

**A Laboratory Guide for a Course in General Botany.** By L. BONAR, R. M. HOLMAN, and L. ROUSH. [Fp. xii + 106.] (London: Chapman & Hall, 1925. Price 6s. net.)

THIS book is intended as a guide to practical botany of an elementary character and consists of directions for laboratory work planned to occupy two periods a week, of two to three hours' duration, throughout a session.

A certain number of questions are given at the end of the various sections: an inclusion that appears somewhat out of place in a laboratory manual. About half the text is devoted to anatomy and physiology and the remainder is chiefly occupied with practical work on representative types of the chief groups.

The directions given are mostly on familiar lines, but the physiological aspects, in connection with which the student requires most assistance, is the least helpful part of a work which is not conspicuous either for the freshness of its outlook nor the comprehensiveness of its content.

E. J. S.

**The Families of Flowering Plants. I. Dicotyledons.** By J. HUTCHINSON, F.L.S. [Pp. xiv + 328, with 264 figures, and distribution maps.] (London: Macmillan & Co., 1926. Price 20s. net.)

WHILST the author's statement that the problems of phylogeny are of especial interest to the younger generation of botanists will be questioned by many, yet discussion of these problems is necessarily bound up with any attempt to improve natural classification.

The system here adopted assumes that the presence of a perianth is primitive, thus contrasting with the Englerian system of the *Pflanzenfamilien* in which the absence of a perianth was a feature of the families which were regarded as the more archaic. Also the view is accepted that the spiral arrangement of parts is more primitive than the whorled. With regard to the perianth it is by no means certain that groups with and without floral envelopes may not be equally primitive. Study of the Ranales, which the author admits as primitive, shows that the perianth has probably evolved in various ways either by modification of bracts, as in *Eranthis*, or by modification of both bracts and stamens, as in *Ranunculus*, and the assumption that all families which do not possess a perianth have attained this condition by reduction is an entirely gratuitous one that ignores the probability that some such families at least represent the less specialised condition in which no modification of either bracts or stamens for protection or attraction has taken place. With regard to the assumption of a primitively spiral arrangement of the floral parts we may note that the cohorts which Mr. Hutchinson employs as the starting points in his phylogenetic scheme, viz. the Ranales and Magnoliales, both contain members whose perianths are whorled, and even those in which the spiral organisation seems most pronounced show evidence of having been derived from a condition in which the parts were in whorls of three members each. In the Ranales as a whole the spiral arrangement is an accompaniment of multiplicity of parts associated with a slowly elongating torus.

But though there will be a considerable body of disagreement with Mr. Hutchinson's views as to the value of different characteristics in determining the degree of specialisation of groups, and the contingent conclusions at which he arrives, yet one must pay tribute to his courageous attempt at reconstruction, which, if it but stimulates discussion and interest, may well do more for the progress of taxonomic botany than the mere acceptance of his views could possibly achieve.

The value of the artificial key to the dicotyledonous families, which occupies fifty pages, can only be assessed by repeated use. It is arranged on the indent system, but without any conventional signs to indicate corresponding subdivisions, in consequence of which the divisions of the larger groups are somewhat difficult to follow.

The greater part of the text is occupied with very condensed descriptions of the families themselves, of which a large number are recognised, viz. 264 as compared with the 241 of Engler's *Syllabus*. This tendency towards multiplication of groups has both its dangers and its merits, but the more complete the segregation the greater the necessity that emphasis should be laid on probable and possible affinities.

In each instance the chief genera are cited and the more important commercial products are mentioned. The very limited space available for description of the individual families necessarily precludes the citation of rare exceptions, though the statement that in the Ranunculaceæ stipules are "absent or rudimentary" can scarcely be excused on these grounds, since stipules which are far from rudimentary characterise a number of species of *Ranunculus*.

A helpful feature is the inclusion of an illustration of a representative member of each of the families recognised. Many of these drawings are

excellent, though their value would have been enhanced by some indication of the scale of magnification or reduction. In addition to the numbered figures there are some thirty diagrammatic maps showing the geographical distribution of individual families and genera.

E. J. S.

**Methods of Descriptive Systematic Botany.** By A. S. HITCHCOCK. [Pp. viii + 216.] (London: Chapman & Hall, 1925. Price 12s. 6d. net.)

A book devoted, as is this work, to the technique of descriptive taxonomy is not only a new departure in botanical literature but is a welcome addition to the botanical bookshelf. Here the student who has but vague notions of the Vienna Code of rules of nomenclature will find enlightenment as to their requirements, and in what respects these differ from the requirements of other codes such as the type system code.

The first six chapters deal with the more elementary aspects of the systematist's work, such as the use of Floras, procedure in the identification of species, taxonomic categories, etc. The later chapters are more especially addressed to the advanced worker who is preparing a taxonomic publication. But for the general botanical reader the chapters on Synonyms and Homonyms, Types, Keys and Synopses, and the various codes of nomenclature provide information which is not available in the ordinary works of reference.

These pages not only contain a great deal of useful information but also much good advice based on the author's considerable experience in the study of grasses.

E. J. S.

**Life of Plants.** By SIR FREDERICK KEEBLE. [Pp. xii + 256, with 51 figures.] (Oxford: at the University Press, 1926. 5s. net.)

THIS is not a textbook of Botany disguised under a popular title, but is an account of the life-processes of the plant with structural details reduced to a minimum. In its small compass the adequately equipped reader will find a good picture of the vital activities of plants, though some aspects, such as vegetative propagation, are only cursorily dealt with.

As a popular account of the more elementary facts this exposition has much to recommend it. It is clearly and interestingly written and the author has attained some measure of success in showing that "the knowledge of plants is an illumination of life." The one handicap to its appeal is that familiarity with technical language is assumed to a degree which is unlikely to be possessed by the type of reader for whom the book is presumably intended.

The illustrations are well chosen, and the format exhibits the usual excellence we associate with the Oxford Press.

Works of this kind are calculated to improve general education and remove the stigma of biological ignorance under which no one can be regarded as either well educated or cultured in the true sense.

E. J. S.

## ZOOLOGY.

**Reproduction in the Rabbit.** By JOHN HAMMOND, M.A. [Pp. xxv + 210, with 39 tables and 20 plates.] (Edinburgh and London: Oliver & Boyd, 1925. Price 15s. net.)

THE fourth volume of the series of monographs of biology published by Messrs. Oliver & Boyd, describes the results of many years of investigation

by Mr. Hammond on the reproductive processes of the rabbit. Prof. F. H. A. Marshall has contributed a foreword and a chapter on the *corpus luteum*. The object of the investigation was to obtain a thorough knowledge of the reproductive physiology in one animal which would serve as a basis for an understanding of those processes in the larger farm animals. In the rabbit are combined a number of features which render it particularly favourable for this work: it is small, cheap, easy to mate, and it only ovulates after copulation or sexual excitement.

The central thought which runs through Mr. Hammond's work is the hypothesis of a "generative ferment" which was first suggested by Heape. This hypothesis postulates that there is in the blood-stream a special substance upon which the development and activities of the various components of the reproductive apparatus in both sexes depends. It seems further to be assumed that when one component is active it exercises a predominant claim upon this substance and so temporarily inhibits the activities of the remaining components. Mr. Hammond is of opinion that this substance is not a "ferment" but rather is allied to the vitamins, and that it is converted by the gonads into "gonadin," the hormone of the glands on which the secondary sexual characters depend. This hypothesis certainly seems to subsume a great number of facts that have been discovered regarding the mutual relations of the reproductive organs. Among others may be mentioned the following: (1) hypertrophy of one gonad when the other is removed, and the presence of only one ovary in the fowl where yolk production is so great; (2) hypertrophy of interstitial tissue in the cryptorchid testis, and in parts with restricted blood-supply after partial castration; (3) delay of development of gonad until puberty in spite of the presence of interstitial cells (the latter being considered as the prime movers in reproductive activity by Steinach), the "generative ferment" being supposed to be necessary for the multiplication of the body cells also; (4) stoppage of general growth and suppression of *corpora lutea* during lactation; (5) involution of uterus during lactation, supposed to be due to the mammary glands depriving the ovary of "generative ferment," whence it is unable to exercise its usual influence on the nutrition of the uterus; (6) dependence of the number of ova produced on "the soma and its nutrition, and not on the amount of ovarian tissue present," shown by the results of removal of all ovarian tissue except the half of one ovary; (7) arrest of spermatogenesis in cancer.

An hypothesis which admits of the subsumption of so many facts (and many more will be found in the book) under one general conception is entitled to every respect, but the suspicion arises that perhaps it is *too* good when every new discovery fits into the scheme so easily. In the first place the question suggests itself: Why should a *special* substance, apart from the general nutritive supplies of the body, be postulated? The author refers to this difficulty in one or two places, particularly on p. 199, where he writes: "Atrophy of the reproductive organs in lactating rabbits occurs while large quantities of body fat are still present, showing that it is the absence of a particular substance and not body energy which is the cause." Now this fact merely shows that nutrition is not simply a question of more or less raw material. This indeed is known from the investigation of vitamins, and it is a familiar observation that some men put on fat on a frugal diet, while others can eat three big British meals a day and still remain thin, even though the expenditure of energy is approximately the same in the two cases. But there does not seem as yet to be any very conclusive evidence that there is one special substance provided for the reproductive organs. The suppression of spermatogenesis in cancer and the stoppage of general growth during lactation suggest that this substance is also available for other tissues as well as the gonads. The whole subject is further compli-

cated by the facts which Mr. Hammond brings forward to show a correlation between activity of the reproductive organs, the suprarenal cortex, and lipid metabolism.

The "generative ferment" hypothesis has clearly been a valuable guide to Mr. Hammond in his investigations, and this after all is the chief function of a good scientific hypothesis. The vast array of well-established fact will remain as a permanent contribution to the physiology of reproduction, whatever becomes of the hypothesis. In any case this postulate does not profess to do more than show why one component of the complex reproductive organs wanes while another waxes; it does not explain how one, so to speak, "gets the upper hand" and drains away the major part of the hypothetical substance. Further, must we postulate a separate substance or hormone for each separate function of each component? Clearly too much must not be expected of "gonadin." These are more ultimate problems which await solution.

Mr. Hammond's book will be a lasting witness to his zeal and energy and a source of authoritative information to all interested both in the theoretical and practical aspects of the physiology of reproduction. It has already won appreciative recognition in the breeders' press. This volume, like the others in the series, is well produced and seems to be quite free from typographical errors.

J. H. W.

**Evolution, Genetics, and Eugenics.** By HORATIO HACKETT NEWMAN. (Second Edition). [Pp. xx + 639, with 93 illustrations.] (Chicago: University of Chicago Press, 1925.)

THIS book is a compilation of quotations from illustrious writers on the topics indicated in the title, with short connecting links by Prof. Newman. The subject-matter falls into three chief divisions. The first presents a short account of the history of evolutionary theories up to the present day, followed by a long chapter on the recent anti-evolution campaign in the United States. Perhaps it is impossible for an English reader to appreciate the feelings of Americans on the subject, but it certainly strikes one as strange that so much space should be devoted to it in a scientific book. The whole tone of this part of the book is highly apologetic, and culminates in a chapter called "The Relation of Evolution to Materialism," which asserts, truly enough, that "Evolution is one thing and Materialism quite another," but it does not tell us what the relation of one to the other really is. It suggests that materialism is a bad thing and evolution a good thing which must therefore not be discredited by getting mixed up with the former. It does not seem to be realised that materialism has had scientific consequences as well as theological ones, and hence an opportunity has been missed of helping to remove a deeply rooted misunderstanding. The rest of this book is very largely materialistic in the sense that it pays little or no attention to *mental* evolution.

The second of the main divisions of the subject-matter presents the evidences for evolution and the traditional causal theories, chiefly from the Darwinian point of view. It is curious to note the amount of space devoted to the views of different workers. In the long chapter on the "acquired characters" question, eight full pages are taken up with an account of the highly debatable results obtained by Guyer and Smith with cytolsins; whereas the beautiful and laborious work of Kammerer is dismissed in a short paragraph of eight lines, only his salamander experiments being mentioned.

The rest of the book is devoted to modern genetics and the chromosome theory, concluding with a considerable section (about 76 pages) on eugenics, as a practical corollary. This book will be useful to those who have not the

leisure to read the original authors in full, but it suffers from the defect of repetition inseparable from such collections. It is on the whole broad and fair, and free from the too confident assertions which are usually to be found in books of this kind.

J. H. W.

**General Zoology.** By H. L. WIEMAN. [Pp. ix + 312, with 208 illustrations.] (London: McGraw-Hill Publishing Co., Ltd., 1925. Price 15s. net.)

**Life and Evolution: An Introduction to General Biology.** By S. J. HOLMES, Ph.D. [Pp. v + 449, with 227 illustrations.] (New York: Harcourt, Brace & Company, 1926.)

If the number of books published on it is any index of the popularity of a subject biology must at the present moment be enjoying a boom in the United States of America. The biological books that have appeared in that country lately have certain rather striking features in common: they usually conclude with a chapter on eugenics, pointing out the pressing need for a solution of America's population problems, and, from the point of view of the teaching of biology, they agree in the small amount of space devoted to the morphological side of the subject and the large amount allotted to evolution and heredity. In these respects American books present a contrast with our own, perhaps because eugenics, anti-evolutionary propaganda, and the chromosome theory of heredity are at the moment characteristically American activities, and American authors are especially sensitive to topical interests.

Prof. Wieman's book is a textbook based on his own class teaching and written almost exclusively from the functional point of view, there being no anatomical description of types. The book begins with a chapter on the subdivisions of the subject and the chief zoological theories, followed by one on adaptation. The eight following chapters are devoted to the chief animal functions, and their organs, in the principal groups. A chapter is then devoted to each of the following topics: metabolism, internal secretions, cell division, gametogenesis and ontogenesis; three more follow on evolution and heredity, and the book closes with a chapter on the principal phyla. It will be seen that this is a large field to cover in so small a compass, so that the several subjects can only be treated in a very superficial manner—a defect that would no doubt be compensated for by lectures and laboratory work. The illustrations are excellent.

The book by Prof. Holmes is less a class-book than an introduction to biological problems for the general student. Prof. Holmes has succeeded in making it very readable. His book deals interestingly with some aspects of the subject (among the more usual ones) which are too often omitted in elementary books of this kind, such as will be suggested by the following chapter titles: "The Perpetuation of Life," "The Development of Social Life," and "Regeneration and the Regulation of Organic Form." These subjects are expounded with a wealth of recent information and the controversial topics are treated with admirable freedom from dogmatism or one-sidedness. The last chapter, on "The Eugenic Predicament," brings very vividly before the reader, by means of accounts of families whose ramifications have been followed in great detail, the alarming rate at which criminal and mentally deficient stocks are multiplying; and, in contrast with this, the gradual diminution of the size of the families who receive a university education. On p. 421 the author writes: "With our present death-rate and marriage-rate it requires three or more children per family to keep a stock from decreasing in numbers. The average number of children for the graduates of Harvard from 1860 to 1890 was a little over two per married graduate. . . . With the graduates of women's colleges the record

is still lower. Only a little over 50 per cent. of women graduates marry at all, and those who marry produce, on the average, less than three children per family . . . the average size of the completed family among American men of science is 1.88 children." The author remarks that "It is much easier to decree that the defectives shall produce less children than to induce superior people to produce more." Prof. Holmes urges the "wider diffusion of knowledge of heredity," and reminds his readers that "We are in one respect better situated than the peoples of older civilisations which have crumbled into decay—we have some knowledge of the biological factors which are responsible for racial improvement and racial deterioration. We can diagnose our own ills. And we should, if we are wise enough, be able to supply the remedy."

J. H. W.

**Laboratory Directions in General Zoology.** By WINTERTON C. CURTIS and MARY J. GUTHRIE. [Pp. xxxiii + 194, with 61 figures.] (New York: John Wiley & Sons; London: Chapman & Hall. Price 7s. 6d.)

THE authors state, in their introductory chapter of remarks to instructors, that if too many cases are presented to the student at the outset, "they cannot see the forest for the trees." That is exactly the trouble with the book. It is so full of trivial instructions, which to any normally intelligent student should be unnecessary—instructions dealing with labelling specimens, tabulating results and drawing dissections—that it is difficult to decide which points are, in the opinion of the authors, of critical importance. In fact, it is not clear whether the primary object of the authors is to teach zoology or to instruct students how to keep a notebook tidy. Perhaps the worst point in this connection is that there is no index to assist the student in "looking for the trees." Surely one of the essentials of any textbook of practical zoology is an efficient index.

The actual instructions for dissection are in many cases novel, but not such as might be expected to result from twenty years' teaching experience. For instance, in dissecting the frog, no directions are given for ligaturing the anterior abdominal vein, and it is difficult to see how a satisfactory display of the body contents can be obtained without this. The anterior abdominal vein seems to be ignored completely. On p. 13 the post-caval vein is said to carry blood from the posterior part of the body to the heart. The alternative route from the hind limbs to the heart via the anterior abdominal vein is not mentioned. But apart from such inaccuracies as this there are technical mistakes, such as directing the student to cut through the pelvis of the frog with a *scalpel*. In dissecting the earthworm the usual precaution of marking segment 15, which is always visible externally, before commencing the general dissection is not mentioned.

The text-figures are clear and may be of use to the elementary student. However, those of the development of *Amphioxus* are very much out of date, while those of the development of the frog on p. 157 which apparently are original, would not have been drawn if the authors had been acquainted with the recent classical researches of Spemann and his school.

H. G. C.

**Problems of Bird Migration.** By A. LANDSBOROUGH THOMSON, O.B.E., M.A., D.Sc. (Aberdeen.) [Pp. xv + 350, with maps and diagrams.] [London: H. F. & G. Witherby, 1926. Price 18s. net.]

It is a pleasure to receive yet another bird book written by a biologist, particularly as our author deals with a phenomenon occurring also in other

sections of the animal kingdom. Since migration reaches its greatest development amongst birds, they undoubtedly offer the best field for such a study as the present. Dr. Thomson has handled his intricate subject with remarkable skill and lucidity, and the book will surely remain the standard English work on bird migration, until some of the still insoluble aspects of the topic have been successfully elucidated.

The book is divided into three sections, I. An outline of bird migration, II. Some special studies of bird migration, III. The main problems of bird migration. There is in addition a short introduction outlining migratory movements in the animal kingdom as a whole.

Part II, beginning with an exposition of the advantages of the marking system, as an aid to the study of migration, deals mainly with selected species that have received special attention from various ringing organisations. These include swallow, white stork, lapwing, starling, mallard, pintail, and herring and lesser black-backed gulls. Some of these studies have been previously published in the *Ibis* and *British Birds*. No one in England has had greater experience with the marking method than the author, and his contention that marking can prove exceedingly profitable along certain lines, if properly carried out, is certainly convincing.

Part I is an excellent general account of the various aspects and problems of migration, while Part III discusses numerous theories bearing on them. This last section has been particularly well handled. It is adequate and clear, but one could wish that the author had expressed his own opinions somewhat more emphatically. The full bibliography at the close of each chapter is ample evidence of the vast amount of labour the author has expended on his task.

To recommend every bird student to read this admirable treatise on the most fascinating of all branches of ornithology may be superfluous, but it can be recommended with equal weight to the laboratory biologist, and perhaps particularly to the geneticist and the physiologist, who will see spread before them an almost untapped field for original research.

W. R.

**The Origin of Birds.** By GERHARD HEILMANN. [Pp. 208, with two Plates in Colour and 140 Photographs and Text Figures executed by the Author.] (London: H. F. & G. Witherby, 1926. Price 20s. net.)

This is a book that supplies a long-felt want, and fills a conspicuous gap in ornithological literature. It is a book for which British ornithologists should be particularly grateful, for it is written entirely in English by a Dane, and very well written too. It contains nothing startlingly new, but a good deal that is new, while it also brings together and adequately summarises recent work by many authors in this field. It is a book that every ornithologist should read, no matter what may be his particular field of activity in the world of birds, or how restricted his line of endeavour, for while the work can hardly be called popular, it is written in such a manner as to be comprehensible to the veriest novice, if he really lends his mind to it. It cannot help but broaden his outlook and show him his own subject in a new light.

The numerous illustrations are excellent and admirably reproduced, and far more fully labelled than is usual, a fact for which many readers will be grateful. They are also much more numerous than is indicated on the title page, for some of the figures contain as many as ten or twelve separate drawings. Each section is provided with its own bibliography. The index could have been greatly improved.

The book is divided into four parts. The first gives a full account of those ever-intriguing fossils *Archæopteryx* and *Archæornis*, the latter being dealt with in great detail, the author having studied it at first hand in Berlin,

This is no doubt the best account of its kind extant. One has to agree with Mr. Heilmann that "Archæornis is a reptile in the disguise of a bird." This portion incidentally deals quite fully with the comparative osteology of birds and reptiles.

Part II compares numerous embryonic and adult characters of birds and reptiles, laying particular stress on the many reptilian structures that appear in the avian embryo and disappear again in the adult. Part III takes us farther along the same lines, and compares selected anatomical and biological facts which impress the reader yet more with the ties that weld together the two component parts of the Sauropsida. The fourth and last part is devoted to a hunt for the Pro-avian, a beast neither bird nor reptile, but offspring of the latter and progenitor of the former. That our author takes us as far as the Pseudosuchians, and there begins the hunt, is not a surprise, but his account of our subsequent travels through the unknown is lucid and convincing. We feel that the hunt has been a success, and that we can accept his restoration of the hypothetical Pro-avian as something very near the truth.

W. R.

**British Birds**, Vol. III. By ARCHIBALD THORBURN, F.Z.S. [Pp. 168 + x, with 48 colour plates.] (London: Longmans, Green & Co., 1926. Price 16s. per vol.)

THE present volume of Mr. Thorburn's work covers all the game birds with the exception of some of the waders, which will no doubt be included in the fourth and last volume which is to appear in the autumn. It maintains the same standard of excellence as its predecessors and emphasises the extraordinary cheapness of this beautifully illustrated work. The Ringed Plover on Plate 137 is one of the most charming portraits Mr. Thorburn has ever produced. It is so delightful that one cannot refrain from wishing that the Turnstone and the Kentish Plover, good as they are—particularly the latter—were not there to distract attention.

It seems a pity that a defect of the first edition of Mr. Thorburn's *British Birds* should have been perpetuated in the present one. Nowhere can we find a variant from the formula "Male and female" under the plates in which both sexes of a species have been illustrated. In about 50 per cent. of the cases it should read "Female and male," the female being on the left and the male on the right. Many amateur ornithologists will not be sufficiently versed in British bird lore to detect the error, except in the most familiar instances.

W. R.

## ANTHROPOLOGY.

**American Journal of Physical Anthropology.** Edited by ALEŠ HRDLÍČKA Vol. IX. No. 2. May—June 1926. (Annual Subscription \$6.25.) (Published at Geneva, N.Y., U.S.A., by W. F. HUMPHREY.)

THERE are six original articles, dealing with very varied researches in physical anthropology, in this part of the American journal. A paper by W. E. Castle on the biological and social consequences of race-crossing provides no new data, but is mainly an attack on Mjoen's theory, based on a study of the effects of crossing different races of rabbits, that physical deterioration is the normal resultant. Castle contends that there is no reliable evidence to support such a theory. It is evident that the crossing of white and coloured races has given rise to inferior populations, but a sufficient cause of their decadence is found in the fact that the parents are in almost all cases drawn from the socially and physically inferior classes. In a paper on the endocranial form of the gorilla skull, dealing with Lord Rothschild's collection of forty-five specimens, H. A. Harris describes a method of determining the

cephalic index from radiographic photographs. The presence of the sagittal and transverse ridges on the adult skull makes it impossible to determine the usual index from ectocranial measurements. The method appears to suffer from several defects: the suggested corrections for distortion are of uncertain accuracy, judging from the photographs reproduced the terminals of the measurements are not precise, there are no comparative data, and the method is certainly expensive. Inside calipers designed to measure the endocranial diameters would give lengths more quickly determinable and subject, we should imagine, to a much smaller personal equation. English anthropologists should be particularly grateful to Dr. Hrdlička for his exhaustive inquiries into the history of the Rhodesian skull. In a paper read to the Royal Anthropological Institute last year he gave an account of his personal interviews with all the workmen and officials who had had any connection with it, and he has now presented all the direct evidence that is ever likely to come to light. The geological age of the skull must remain indeterminate; if its form had not been indubitably archaic, a case for its modernity might well have been made out. Several hitherto unpublished measurements are given, but there is a pressing need for a comparative study.

A paper by C. B. Davenport under the somewhat misleading title of *Human Metamorphosis* is yet another contribution to the extensive literature on growth curves for the living subject. Stature, weight, and arm-span are dealt with, and there are some new data for the last character. In *Human Blood Groups: their Inheritance and Racial Significance*, Dr. L. H. Snyder claims to have given a more comprehensive account of the work already published than any previous one. It is claimed that, "In the human race the blood groups occur as fixed bio-chemical conditions, subject to the laws of heredity. As such they provide a method of studying racial origins and relationships." From the fact that the sera of some bloods cause the cells of certain other bloods to agglutinate, it has been possible to divide all human samples into four qualitatively distinct groups, and the classification is apparently accepted by all present-day workers. A study of the statistics of 200 families leads Snyder to confirm the suggestions that the "blood groups are inherited as a series of three multiple allelomorphs, not as two independent pairs of factors, as has long been supposed." The group proportions for various races of man lead to broad distinctions between families of races corresponding with those found for other somatic characters. Large numbers of individuals have been tested—statistics being given for more than 66,000 representing 54 races—but it has yet to be shown that the method can effectively classify local races. The work being done in this field is certainly of great interest and importance to the physical anthropologist. The last paper is a short one by R. J. Terry, presenting new data of the frequency of the supra-condyloid process of the humerus found among various native and white populations in North America.

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## MEDICINE.

**A Manual of Injurious Insects.** By GLENN W. HERRICK. [Pp. xxi + 489, with 458 figures.] (New York: Henry Holt & Company. Price \$4.50.)

A BOOK which will be very useful to many people in warm countries, and indeed elsewhere. It contains twenty-five chapters, which include short notes regarding the physiology of insects and the losses caused by them. Methods for destroying pests are described, and then mentions are made of insects which are injurious to various kinds of fruit and crops. There are no less than 458 figures in the text. The insects, however, which carry

diseases of men and animals are scarcely dealt with at a length sufficient to be useful to medical men or veterinarians. Papers, chiefly American ones, referring to particular points are mentioned.

**Ultra-violet Radiation and Actinotherapy.** By ELEANOR H. RUSSELL, M.D., B.S., Dunelm, and W. KERR RUSSELL, M.D., B.S., Dunelm; with Forewords by SIR OLIVER LODGE, F.R.S., D.Sc., LL.D., and SYDNEY WALTON, C.B.E., M.A., B.Litt. [Pp. iv + 262.] (Edinburgh: E. S. Livingstone. 1925. Price 10s. 6d. net.)

THIS book gives a very good account of the subject from the point of view of therapeutic utility. The authors write throughout the text as having had first-hand experience in the use of the various sources of visible and ultra-violet radiation which are so extensively used at the present time. The descriptions of these various sources are adequate and are supplemented by good illustrations. The different diseases which are treated by means of these radiations increase by leaps and bounds, but the mechanism by which restoration of growth or function is brought about remains very obscure. This is likely to be the case until much more experimental work has been done, for it appears to be true that profound effects are produced in the body by radiation which only penetrates the surface to a very small degree. How this is brought about is at present largely a matter of conjecture, and experimentation proceeds but slowly. The authors give one chapter on the biological effects of ultra-violet radiation, and this but serves to accentuate the scanty nature of the information yet to hand on the nature of the reactions of living things to the radiations in question.

The demand in medicine for books on this subject is well met by the one under review, and we look forward in future editions to a larger space being devoted to the mode of mechanism of the radiations which appear to be so beneficent in many human ills.

S. Russ.

### MISCELLANEOUS.

**Science and the Modern World.** By A. N. WHITEHEAD, F.R.S. [Pp. xi + 296.] (Cambridge: at the University Press, 1926. Price 12s. 6d.)

IN this extremely important book, based upon his Lowell Lectures, Prof. Whitehead has disclosed the edifice of ideas which the foundations laid down in *The Principles of Natural Knowledge* (1919), *The Concept of Nature* (1920), and *The Principle of Relativity* (1922) were intended to support. Those books were, in the main, technical studies, full of interest and value for scientific and philosophical readers sufficiently prepared to follow them, but a little baffling to the philosopher without some mathematics and physics and the scientist without some training in metaphysics. The present work has its patches of difficulty where the novel and subtle ideas with which it abounds are packed too tightly for easy comprehension; but, on the whole, the argument is crystal-clear, and is developed with a breadth of knowledge and appreciation, a boldness in metaphysical conception attended everywhere by sane and level judgment, an architectonic sense, an epigrammatic felicity of expression, an urbanity and a disciplined eloquence which, taken together, must secure for the author a high place among philosophical writers.

Dr. Whitehead's main point is that physical science has, during the last four centuries, won its triumphs by rigid adherence to an abstraction whose essence is to regard the passage of nature as a series of configurations of unchanging material elements, defined by "simple location" in space and time and succeeding one another in accordance with a few universal prin-

ciples; and that this scheme, notwithstanding its enormous successes, started with a wrong view of what is really most concrete in nature, and is, therefore, fundamentally incapable of explaining the world. In fact, its claims, though in principle unlimited, are in practice admitted fully by nobody. "A scientific realism, based on mechanism, is conjoined with an unwavering belief in the world of men and of the higher animals as being composed of self-determining organisms." This inconsistency and the distraction to which it has led are widely expressed in modern thought and literature (*e.g.* in Tennyson) and gave a characteristic colour to nineteenth-century civilisation. The weakness due to the "misplaced concreteness" of the mechanistic theory was bound, sooner or later, to be revealed by its own intrinsic development, just as the development of the Ptolemaic theory brought out the weakness of the geocentric assumption. The emergence of the quantum theory seems to mark the arrival of physics at this point; for the theory of the atom which it imposes "is strongly suggestive of the epicycles of astronomy before Copernicus."

In what way must science reorganise its concepts so as to escape from the present *impasse* and regain harmony with views of the world that claim an equal validity with its own? Prof. Whitehead's answer is, in brief, that it must abandon the postulate of simple location and take seriously the doctrine that past, present, and future are terms implying reference to a specific standpoint; that it must substitute for the idea of "bits of matter" whose inner nature is uninfluenced by their external relations the notion of "events" that are never wholly self-contained but always have aspects that belong to the constitution of other events; that it must replace the cinematographic view of nature by the Bergsonian idea of a continuous passage in the course of which the genuinely concrete elements of the world develop their character; and finally that it must recognise that this character is the character associated with the term *organism*, *i.e.* that the real concretes in nature are structures whose constituents are, in general, modified in nature and behaviour in accordance with the plan of the whole to which they belong.

The development of this theory of "organic mechanism"—which has an obvious kinship with the metaphysics of Alexander and of Lloyd Morgan—involves features of great interest that can here only be glanced at. They include the bold concept of experience, abstracted from cognition, as a pervasive fact in nature; the notion of a "prehensive activity" which is the source of unity of spaces, durations, and perceived objects; a doctrine of evolution which, upon the assumption that all concrete things are organisms, includes the study of the conditions that favour the endurance and development of atoms and molecules as well as of plants and animals; and a discussion of the irrationality expressed in the limitation of actuality among infinite alternatives which leads to the metaphysical discovery of God as "the ultimate limitation" and of "His existence as the ultimate irrationality." The debt to Alexander's great book, *Space, Time, and Deity*, which Dr. Whitehead fully acknowledges, lies largely in the region occupied by these important and difficult topics; but his treatment of them is nevertheless highly original and derives special value from the author's combination, in the highest degree, of knowledge and powers with which few philosophers have had the good fortune to be equipped.

T. P. NUNN.

**The Purpose of Education.** By ST. GEORGE LANE FOX-PITT. (Cambridge University Press. Fifth issue revised, 4s. net.)

A VERY valuable addition to the economic section of this excellent book has made a perusal of this new issue well worth while. A clear understanding of present-day economic problems is vital to the spiritual as well as the

material health of the nation, and is therefore in its essence a problem of education. The writer believes that the ignorance and mental confusion that prevail in economic questions is a real danger to its healthful existence. No one at the present moment can doubt this contention, and every effort at unprejudiced thought and at clarity on the subject is especially welcome.

W. C. B.

**Practical Photo-micrography.** By J. E. BARNARD, F.R.S., F.Inst.P., F.R.M.S. and FRANK V. WELCH, F.R.M.S. [Second Edition. Pp. xii + 316, with xvi plates and 84 text-figures.] (London: Edward Arnold & Co., 1925. Price 18s. net.)

THIS is the new edition of a work which first appeared fourteen years ago. The authors state that during this period no outstanding advance in Photo-microscopy has been made either on the optical or on the mechanical side, and are of opinion that future developments will be in the direction of the efficient utilisation of simple apparatus. They have consequently centred their attention, in the addition to and revision of the work, on detailed accounts of procedure rather than on apparatus: as various textbooks on the subject of the microscope are available, a comparatively shorter space is given to the consideration of this instrument and more to the camera and its construction. The rest of this book is taken up by a description of illuminating condensers, screens, plates, and all the various processes involved in the subject. There is no account of the recent work of the authors which has shown that there are hitherto unsuspected possibilities in the use of ultra-violet light in Photo-micrography. This information will be published in a separate volume.

The work is an admirable and authoritative account of the various processes in use in Photo-micrography.

E. M. C.

**Phototopography. A Practical Manual of Photographic Surveying Methods.** By A. L. HIGGINS, M.Sc., A.R.C.S., A.M.Inst.C.E. [Pp. xv + 130. with 52 illustrations, including 1 folding plate and 8 other plates. (Cambridge University Press, 1926. Price 6s.)]

THIS most recent addition to the literature of surveying contains three introductory sections, headed respectively "Preface," "Introduction," and "Bibliography." These are followed by five "articles" dealing respectively with fundamental principles, the photogrammeter, photographic surveying, photogrammetry, and stereo-phototopography. The book concludes with a section headed "Hints and Observations," and an index.

In the words of the preface "the following pages are presented, not as a treatise on one of the most widely discussed branches of Geodesy, but as a manual in which the essential principles are outlined. . . ."

There is little doubt that there is room for a book in English on this subject, coming between a complete treatise like that of J. A. Flemer, and the scrappy articles in books on general surveying and in the transactions of scientific bodies. The little book by Capt. Deville, published in 1895, is not only somewhat out of date, but also difficult to obtain.

From this point of view, the book is certainly to be welcomed; and, as will be seen from the list of contents, a most comprehensive survey of the subject is attempted. But it is, perhaps, in this very respect that the book is most open to criticism. The author seems to have tried to cover so much ground that it has been necessary to condense, at times—(in the opinion of the present writer)—beyond the limits of clearness. A notable example is in the section on contouring, beginning on p. 86, while one wonders if it

would not have been wiser, in a book of this size, to omit the portion dealing with inclined photographic plates (for instance) altogether. There are some misprints and omissions, including an important one in the formula on p. 28. In this connection, nothing is said about the reason why Haffner's complicated formula should be preferred to the more simple formula of Deville.

There are also cases where condensation has been carried so far that text and illustration do not seem to agree readily, for instance in Fig. 12 one looks in vain for the points  $p$ ,  $q$ , referred to in the text, while the meaning of points  $p$ ,  $q$ , "at the same elevation" (p. 25), does not seem very clear.

These are, however, after all but small faults. The worst of them can easily be remedied, and, while the reviewer must regard it as his duty to point them out, they must not be allowed to obscure the facts that the book fills a gap in the literature of the subject, and that it gives a useful general survey of the methods and problems of photographic surveying from the ground. There is also an account of an original contrivance of the author's for use in plotting from the photographs.

M. T. M. O.

**Formal Logic** (1847). By AUGUSTUS DE MORGAN, edited by A. E. TAYLOR, F.B.A., Professor of Moral Philosophy in the University of Edinburgh. [Pp. xxi + 392.] (London: The Open Court Company, 1926. Price 12s. 6d. net.)

SERIOUS students of Logic will cordially welcome this reprint of a book which is a classic of its kind, and which has been out of print, and difficult to procure, for many years now. Professor Taylor, the editor of the reprint, and Mr. J. M. Child, of the University of Manchester, have devoted the necessary care to present an accurate text free from the errata of the original edition. It is a pity in some ways that Professor Taylor did not utilise the occasion to give a brief account of the work of De Morgan, and an estimate of his place in the history of Logic; but having got the carefully edited reprint it may be ungrateful to complain that Professor Taylor has not given us more.

It is a remarkable fact that some of the most important British contributions to Logic during the last hundred years or so have been made by writers who were not professional teachers of Logic, and who were all associated with University College, London. The names of John Stuart Mill, Augustus de Morgan, and W. Stanley Jevons are familiar to every student of Logic. Mill was primarily an economist and politician, De Morgan was primarily a mathematician, and Jevons an economist. Mill was one of the founders of University College, De Morgan and Jevons were Professors there, of Mathematics and of Political Economy respectively. It would be interesting to know exactly why they all felt drawn to the study of Logic. It is not extravagant to surmise that they felt the need and importance of a due appreciation of evidence in all kinds of scientific and kindred work. And when it is seen that men of science with the same data before them not infrequently take extremely opposite views in the interpretation of those data, one is made to feel that the logical study of evidence and of the general conditions of valid reasoning might profitably receive a larger measure of attention than it does. Such, at all events, was the view held by Lord Kelvin.

The dominant mathematical interests of De Morgan naturally influenced his Logic. This is clear from his emphasis on numerically definite syllogisms, on probability, and his conception of Logic, or certain parts of it, as preparatory to the study of Geometry. Since the days of De Morgan, and largely through his and Boole's influence, the mathematical side of formal Logic has been developed enormously by mathematicians. But, on the

whole, those parts of Logic in which Mill, De Morgan, and Jevons were chiefly interested, namely, the study of the conditions of valid inference, have unfortunately been rather neglected by the present generation. This is mainly due to the influence of certain philosophers, or rather certain teachers of philosophy, who are more interested in speculative theories of knowledge than in the study of "the calculus of inference necessary and probable," to use De Morgan's phrase. Logic, as conceived by these teachers of philosophy, is the study of thought. By extending the conception of Logic in this way it is possible to talk about the same things under different names—Logic, Psychology, Epistemology, Philosophy, etc.—and one is not so tied to the conditions of valid inference. Educationally, however, this is a grave error, and it is to be hoped that the reissue of De Morgan's *Formal Logic* may be regarded as a symptom of a revived interest in Logic as he and his contemporaries understood it.

A. WOLF.

**Mind and Matter.** By C. E. M. JOAD. [Pp. 170.] (London: Nisbet & Co., 1926. Price 4s. 6d. net.)

THIS little book is described at once as "an introduction to the study of metaphysics" and as "the philosophical introduction to modern science." Both these claims appear to be rather extravagant, but then one need not expect much from a mere "introduction." The actual aim of the little volume is "to outline and to examine some of the most important theories current to-day with regard to (1) the nature of mind, (2) the nature of matter, and (3) the relationship between them." Mr. Joad handles these problems with the lightest possible touch, and many a general reader will no doubt be duly grateful to him for this popular exposition of difficult, and therefore unpopular, themes. Unfortunately for Mr. Joad, his treatment of the problems in this book seems inevitably to challenge comparison with the treatment of the same problems in Dr. Broad's *Mind and its Place in Nature* and with Mr. Bernard Shaw's philosophy of the Life-Force in *Man and Superman* and *Back to Methuselah*. There are very few books that will bear comparison with the philosophical acumen of the one or with the literary art of the other. And Mr. Joad cannot be disappointed if his little book suffers from a comparison which he certainly did not intend to provoke.

A. WOLF.

**International Critical Tables of Numerical Data, Physics, Chemistry, and Technology.** Prepared under the Auspices of the International Research Council and the National Academy of Sciences, by the National Research Council of the United States of America. Editor-in-Chief EDWARD W. WASHBURN, Ph.D. [Pp. xix. + 415], in five volumes. (London and New York: McGraw-Hill Book Company, 1926. Price per set \$60, payable at the rate of \$12 per volume as the volumes are published.)

THIS book has been produced by a considerable body of men of science acting as Corresponding Editors or on Advisory Committees under the auspices of the National Research Council of the United States. The Preface of the Board of Editors states that the International Union of Pure and Applied Chemistry, at a meeting held in London in June 1919, approved the compilation of the tables and assigned the task to the United States of America. The American National Research Council then accepted the executive task and appointed a Board of Editors. Early in 1922 this Board appointed Corresponding Editors in different countries. The general

plan of preparation of the tables was first to divide the subject-matter into some three hundred different sections; and then experts were sought out to undertake the task of critical compilation of the several sections.

On the back of the volume it is described as Volume I, but no indication of this is to be found on the title-page. The contents of the volume consist of National and Local Systems of Weights and Measures; Symbols, Basic, Constants, Conversion Data, Dimensions, Definitions; Chemical Elements and Atoms, to begin with. Then a section deals with Laboratory Technique; another with the Arrangement of Chemical Substances; another with Radioactivity, and, lastly, one on Astronomical and Geodetic Data, and on Aerodynamics. The quarto pages are divided into two columns, the printing is fine and very clear; and the volume will certainly be in demand in most laboratories connected with the subjects it deals with, and also, to a less extent, in biological institutions. The Preface and some other matters are rendered in English, French, German, and Italian.

## BOOKS RECEIVED

*(Publishers are requested to notify prices)*

Les Fonctions Quasi Analytiques. Leçons Professées au Collège de France. Par T. Carleman, Professeur à l'Université de Stockholm. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 116.) Price 30 frs.

A Course of Differential Geometry. By the late John Edward Campbell, F.R.S., Fellow of Hertford College, Oxford. Prepared for the Press with the Assistance of E. B. Elliott, M.A., F.R.S., Emeritus Professor. Oxford: at the Clarendon Press, 1926. (Pp. xv + 261.) Price 21s. net.

Reziproken Tafel. Aller Ganzen Zahlen von 1 Bis 10000. Ausgabe F. von Noordhoff's Tafeln von Dr. M. van Haaften., Chefmathematiker der Hollandsche Societeit van Levensverzekeringen in Amsterdam. Groningen: P. Noordhoff, 1926. (Pp. xxiii + 50.) Price 2.40 frs.

A very well and largely-printed table of Reciprocals of integral numbers from 1 to 10000, placed out on 50 pages in groups of five numbers each, the reciprocals being carried to seven places.

Analytical Functions of a Complex Variable. By David Raymond Curtiss, Professor of Mathematics, North-Western University. Chicago: Published for the Mathematical Association of America by the Open Court Publishing Company. (Pp. ix + 173.) Price 10s. net.

Mechanics and Applied Mathematics. Statics-Dynamics-Hydrostatics. By W. D. Hills, B.Sc., Senior Applied Mathematics and Mechanics Master, Dartford Grammar School, Kent. Part II. Applied Mathematics. London: University of London Press: 10 Warwick Lane, E.C.4, 1926. (Pp. xi + 248.) Price 5s. net.

Practical Geometry. Based on the various Geometry Books by Godfrey and Siddons. By A. W. Siddons, M.A., Senior Mathematical Master at Harrow School and R. T. Hughes, M.A., Assistant Master at Harrow School. Cambridge: at the University Press, 1926. (Pp. ix + 264.) Price 4s. net.

Theoretical Geometry. Based on the various Geometry Books by Godfrey and Siddons. By A. W. Siddons, M.A., Senior Mathematical Master at Harrow School, and R. T. Hughes, M.A., Assistant Master at Harrow School. Cambridge: at the University Press, 1926. (Pp. xiv + 173.) Price 3s. net.

Fourfold Geometry. Being the Elementary Geometry of the Four-Dimensional World. By David Beveridge Mair. London: Methuen & Co., 36 Essex Street. (Pp. viii + 182, with 61 diagrams.) Price 8s. 6d. net.

The Wonder and the Glory of the Stars. By George Forbes, M.A., F.R.S. London: Ernest Benn, Bouverie House, Fleet Street, 1926. (Pp. 221, with 16 plates, and 20 figures.) Price 8s. 6d. net.

- The Elements of Astronomy. A Non-mathematical Textbook for Use as an Introduction to the Subject in Colleges, Universities, etc., and for the General Reader. By Edward Arthur Fath, Professor of Astronomy in Carlton College. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. viii + 307, with 191 figures in the text.) Price 15s. net.
- A Manual of Radioactivity. By George Hevesy, Ph.D., and Fritz Paneth, Ph.D. Translated by Robert W. Lawson, D.Sc., F.Inst.P. London: Oxford University Press, 1926. (Pp. xix + 252).
- Geschichte der Physik. By Edmund Hoppe. Verlag von Friedr. Vieweg & Sohn. Akt.-Ges. in Braunschweig, 1926. (Pp. viii + 536.) Price 30 R.M. Bound 33 R.M.
- Readable Relativity. A Book for Non-specialists. By Clement V. Durrell, M.A. Senior Mathematical Master at Winchester College, London: G. Bell & Sons, 1926. (Pp. vii + 146.) Price 3s. 6d.
- Reflections on the Structure of the Atom. By Florence Langworthy. London: Watts & Co., Johnson's Court, Fleet Street, E.C.4. (Pp. xi + 260.) Price 12s. 6d. net.
- Photochemical Reactions in Liquids and Gases. A General Discussion held by the Faraday Society, October 1925. London: The Faraday Society, 90 Great Russell Street, W.C.1. (Pp. 437-658.) Price 15s. 6d. net.
- Reichenbach's Letters on Od and Magnetism (1852). Published for the first time in English, with extracts from his other works, so as to make a complete presentation of the Odic Theory. Translated Text, Introduction, with Biography of Baron Carl von Reichenbach, Notes, and Supplements by F. D. O'Byrne, B.A., Sole Official Interpreter to the International Congress of Radiology. London, 1925. London: Hutchinson & Co., Paternoster Row, E.C., 1926. (Pp. lxxii + 119.) Price 7s. 6d. net.
- Atomzertrümmerung. Verwandlung der Elemente durch Bestrahlung mit A-Teilchen, von Hans Pettersson and Gerhard Kirsch. Mit 61 Figuren im Text und 1 Tafel. Leipzig: Akademische Verlagsgesellschaft. M.B.H., 1926. (Pp. viii + 327.) Price 15 marks.
- The New Heat Theorem. Its Foundations in Theory and Experiment. By W. Nernst, Professor at the University of Berlin. Translated from the Second Edition by Guy Barr, B.A., D.Sc. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvi + 281.) Price 12s. 6d.
- Chemistry and Recent Progress in Medicine. By Julius Stieglitz. Chairman of the Department of Chemistry; Professor of Chemistry in the University of Chicago. Baltimore: Williams & Wilkins Company, 1926. (Pp. viii + 62.) Price 7s. net.
- Problems in Organic Chemistry. By H. W. Underwood, Jr., Ph.D., Instructor in Organic Chemistry, Massachusetts Institute of Technology. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xii + 233.) Price 10s. net.
- Chemistry of the Proteins and its Economic Application. By Dorothy Jordan Lloyd, M.A., D.Sc., F.I.C. Biochemist on the Staff of the British Leather Manufacturers' Research Association. Introduction by Sir Frederick Gowland Hopkins, M.B., D.Sc., F.R.C.P., F.R.S., Sir William Dunn Professor of Biochemistry, University of Cambridge. London: J. & A. Churchill, 7 Great Marlborough Street, 1926. (Pp. xii + 279, with 50 illustrations. Price 10s. 6d. net.

- Industrial Stoichiometry.** Chemical Calculations of Manufacturing Processes. By Warren K. Lewis and Arthur H. Radasch. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xi + 174.) Price 12s. 6d. net.
- Thermodynamics for Students of Chemistry.** By C. N. Hinshelwood, M.A., Fellow and Tutor of Trinity College, Oxford. London: Methuen & Co., 36 Essex Street, W.C. (Pp. v + 185, with 11 diagrams.) Price 6s. net.
- Variations in the Composition of Milk.** By J. F. Tocher, D.Sc., F.I.C., Public Analyst, Official Agricultural Analyst, Consulting Chemist to the Highland and Agricultural Society of Scotland, Lecturer on Statistics in the University of Aberdeen. Edinburgh: Published by His Majesty's Stationery Office, 1925. (Pp. 195.) Price £1 1s. net.
- A Textbook of Organic Chemistry: Historical, Structural, and Economic.** By John Read, M.A., Ph.D., B.Sc., Professor of Chemistry and Director of the Chemical Research Laboratory in the United College of St. Salfahr and St. Leonard in the University of St. Andrews. London: G. Bell & Sons, 1926. (Pp. xii + 679.) Price 12s. 6d. net.
- A Textbook of Inorganic Chemistry.** By Dr. Fritz Ephraim, Professor of Chemistry in the University of Berne. English Edition by P. C. L. Thorne, M.A., M.Sc., Lecturer in Chemistry, Sir John Cass Technical Institute, London. London: Gurney & Jackson, 33 Paternoster Row, E.C.4, and Edinburgh: Tweeddale Court, 1926. (Pp. xii + 805.) Price 28s. net.
- The Chemistry and Examination of Edible Oils and Fats: Their Substitutes and Adulterants.** By G. D. Elsdon, B.Sc., F.I.C., Lancashire County Analyst. London: Ernest Benn, Bouverie House, Fleet Street, 1926. (Pp. xix + 521, with 12 illustrations.) Price 45s. net.
- An Introduction to Chemistry.** By C. G. Vernon, M.A., B.Sc. London: George G. Harrap & Co. (Pp. 276, with 7 figures.) Price 4s. 6d. net.
- An Introduction to Surface Chemistry.** By Eric Keightley Rideal, Humphrey Owen Jones Lecturer in Physical Chemistry, Cambridge University. Cambridge: at the University Press, 1926. (Pp. vii + 336.) Price 18s. net.
- Tables of Physical and Chemical Constants and some Mathematical Functions.** By G. W. C. Kaye, O.B.E., M.A., D.Sc., F.Inst.P., Superintendent of the Physics Department, the National Physical Laboratory, and T. H. Laby, M.A., Sc.D., F.Inst.P., Professor of Natural Philosophy, University of Melbourne. Fifth Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. vi + 161.) Price 14s. net.
- Tables of Physical and Chemical Constants and some Mathematical Functions.** By G. W. C. Kaye, O.B.E., M.A., D.Sc., F.Inst.P., Superintendent of the Physics Department, the National Physical Laboratory, and T. H. Laby, M.A., Sc.D., F.Inst.P., Professor of Natural Philosophy, University of Melbourne. Fifth Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. vi + 161.) Price 14s. net.
- Colloid and Capillary Chemistry.** By Herbert Freundlich, Ph.D., Professor at the Kaiser Wilhelm Institute for Physical Chemistry, Berlin. Translated from the Third German Edition by H. Stafford Hatfield, B.Sc., Ph.D. London: Methuen Co., 36 Essex Street, W.C. (Pp. xv + 883, with 157 diagrams.) Price 50s. net.

- Ice Ages Recent and Ancient. By A. P. Coleman, M.A., Ph.D., F.R.S., Emeritus Professor of Geology, University of Toronto. London: Macmillan & Co., 1926. (Pp. xiii + 296, with 51 illustrations and 8 maps.) Price 17s. net.
- Kalkfrage Bodenreaktion und Pflanzenwachstum. Von O. Arrhenius. Stockholm. Mit 40 Abbildungen und 1 Tafel. Leipzig: Akademische Verlagsgesellschaft. M.B.M., 1926. (Pp. vii + 148.) Price 8 marks.
- Index Kewensis Plantarum Phanerogamerum. Supplementum Sextum Nomina et Synonyma Omnium Generum et Specierum ab Initio Anni MDCCCCXVI Usque ad Finem Anni MDCCCCXX Nonnulla Etiam Antea Edita Complectens. Ductu et Consilio A. W. Hill Confecerunt Herbarii Horti Regii Botanici Kewensis Curatores. Oxonii: e Preto Clarendoniano, 1926.
- Root Development of Field Crops. By John E. Weaver, Professor of Plant Ecology, University of Nebraska. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xii + 291, with 113 figures.) Price 15s. net.
- The Pediculate Fishes of the Suborder Geratioidea. By C. Tate Regan, M.A., F.R.S., Keeper of Zoology in the British Museum (Natural History). The Danish "Dana"—Expeditions 1920-22 in the North Atlantic and the Gulf of Panama. Leader: Professor Johs. Schmidt, Ph.D., D.Sc., Geographical Reports edited by the "Dana"—Committee. Published at the Cost of the Rask-Orsted Fund. Copenhagen: Gyldendalske Boghandel. London: Wheldon & Wesley, 1926. (Pp. 45, with 3 plates and 17 figures in the text.) Price 15s. net.
- Animal Ecology. By A. S. Pearse, Professor of Zoology, University of Wisconsin. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. ix + 417.) Price 20s. net.
- Eugenics. By A. M. Carr-Saunders. London: Williams & Norgate. (Pp. vii + 256.) Price 2s. 6d. net.
- Notes on the Game Birds of Kenya and Uganda. (Including the Sandgrouse, Pigeons, Snipe, Bustards, Geese and Ducks.) By Sir Frederick J. Jackson, K.C.M.G., C.B., M.B.O.U., F.Z.S. London: Williams & Norgate, 18 Henrietta Street, Covent Garden, W.C.2, 1926. (Pp. xiii + 258 with 13 coloured plates.) Price 25s. net.
- Tierpsychologie vom standpunkte des Biologen. Von Dr. Friedrich Hempelmann. A. O. Professor der Zoologie und Vergl. Anatomie an der Universität Leipzig. Mit 124 Figuren im Text und 1 Tafel. Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1926. (Pp. viii + 676.) Price 36 marks.
- Dr. H. G. Bronns Klassen und Ordnungen des Tier-Reichs. Wissenschaftlich dargestellt in Wort und Bild. Dritter Band. Mollusca (Weichtiere) 2 Buch: Pulmonata. Bearbeitet von Dr. H. Simroth, fontgeführt von Dr. H. Hoffman in Jena. 148 Lieferung. Mit 81 Abbildungen im Text. Seite 833 bis 912. 11.20 marks. 149 Lieferung. Mit 74 Abbildungen im Text. Seite 913 bis 964, 1925. 8 Marks. Fünfter Band Gliederfüssler: Arthropoda. I Abteling: Myriapoda, 2 Buch: Diplopoda. Bearbeitet von Dr. K. W. Verhoeff in Pasing. 1 Lieferung. Mit 3 Abbildungen im Text. Seite 1 bis 128, 16 marks. 2 Lieferung. Mit 23 Abbildungen im Text und 5 Tafeln. Seite 129 bis 288, 20 marks, 1926. Leipzig: Akademische Berlagsgesellschaft. M.B.H.
- Biology "Science for All" Series. By O. H. Latter, M.A., Senior Science Master, Charterhouse. London: John Murray, Albemarle Street, W.1. (Pp. vii + 197.) Price 3s. 6d. net.

- An Introduction to Experimental Embryology. By G. R. de Beer, M.A., B.Sc., F.L.S., Fellow of Merton College, Oxford: at the Clarendon Press, 1926. (Pp. 148.) Price 7s. 6d. net.
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# SCIENCE PROGRESS

## RECENT ADVANCES IN SCIENCE

**PHYSICS.** By L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

*The X-Ray Examination of Long-chain Compounds.*—A very comprehensive dissertation, of great interest to all interested in research on X-ray spectra, long-chain compounds and surface phenomena in general, is published in the *Annales de Physique* for July–August 1926, by M. Trillat, who has carried out much work in M. de Broglie's laboratory. It is now well established, through the work of many investigators, that numerous long-chain organic compounds, such as fatty acids, alcohols, ketones, etc., when melted or pressed on a strip of glass, set in such a manner that they exhibit under X-ray examination a series of regular, equidistant planes a relatively great distance apart. In other words, substances whose condition may be intermediate between the amorphous and crystalline states exhibit the phenomena of X-ray reflection in the same way as purely crystalline bodies. Thus the rotating crystal method enables us to obtain photographs of X-ray spectra which are characteristic of these substances.

In order to consider the work in the above article, it is necessary briefly to review the work carried out previously by other workers. Müller (*Trans. Chem. Soc.*, vol. 123, p. 2043, 1923), in the first paper on the X-ray examination of long-chain compounds, dealt with the saturated fatty acids containing from ten to twenty atoms of carbon. In these experiments, and those of Müller and Shearer (*Trans. Chem. Soc.*, vol. 123, p. 3156, 1923), a small quantity of the fatty acid was melted and spread on a small strip of glass, which was mounted on the table of an X-ray spectrometer and allowed to rotate slowly about a vertical axis, whilst a beam of monochromatic X-rays fell upon it. The acid formed a series of planes of molecules parallel to the surface of the glass, with the result that for definite angles of reflection, depending on the wavelength employed, lines were produced on a photographic plate placed in a suitable position. These lines, which were well defined, correspond to successive orders of reflection from

the same system of parallel planes. The displacements of the lines varied regularly with the number of  $\text{CH}_2$  groups in the long-chain compound, and for the series of fatty acids investigated, it was found that the distance between consecutive planes was directly proportional to the number of carbon atoms in the chain. This law was confirmed by spectrographic X-ray examination of numerous other compounds by Saville and Shearer (*J.C.S.*, vol. 127, p. 591, 1925), and by Müller and Saville (*ibid.*, p. 600). The metallic salts (soaps) of certain fatty acids were studied by Piper (*Proc. Phys. Soc.*, Aug. 1923), who also found a regular increase in the spacing with increase in the number of carbon atoms in the chain. If the molecules are perpendicular to the film, then the distance between the consecutive planes represents the length of a molecule, but in general the molecules are inclined to the surface, and the measured spacing is less than the true length of the molecule; this was clearly shown by Müller's examination of a large crystal of stearic acid (18 carbon atoms) which was a monoclinic prism, and in which the carbon chain made an angle of  $59^\circ$  with the base. In addition to the well-defined line spectra previously mentioned, a certain number of faint lines were also observed, and these appeared to correspond to planes whose spacing was not a function of the number of carbon atoms in the chain. Presumably they arose from the fact that the elementary cell was a long prism whose cross section remained constant, but whose length varied with the number of carbon atoms.

Trillat's experiments were carried out by the simultaneous use of the rotating crystal and the powder transmission methods of X-ray examination, and he first investigated the effect of the glass surface on the orientation of the compounds under examination. The object of these experiments was to determine the conditions under which the parallel layers were produced, and what modifications were possible. Palmitic acid was used throughout, as it was obtainable in a state of great purity. To produce satisfactory films of acid, a few drops of an alcohol or benzene solution of the acid were placed on a glass strip, and the solvent allowed to evaporate. Now, it is known that the adherence of a fatty compound to a glass surface is due almost entirely to the layer of molecules in immediate contact with the glass, and it would therefore be expected that a thin film of water would prevent the acid from sticking. According to Devaux (*Journ. de Phys.*, Sept. 1923), faces are formed on the fatty acid which may or may not be wetted by water. Thus, when a film of water is placed between the glass and the substance, the acid surface turned towards the glass must be one that is wetted, and the molecules, instead

of forming chains perpendicular to the surface of the glass, may take up a totally different orientation. In fact, only spectra corresponding to very small spacings were observed by Trillat under these conditions, so that the chains must have set with their axes parallel to the surface of the glass. After showing the effect of moisture on the spectra, the next step consisted of finding the effects of acidity or alkalinity of the glass surface. It was found that when the surface was slightly acid, the fatty acid wetted the surface with difficulty, and the spectra consisted only of the faint lines due to narrow spacings. On the other hand, when the surface was slightly alkaline, the acid wetted the surface with ease, and good spectra consisting of sharp lines were obtained. This was, presumably, because the alkaline surface exerted a powerful attraction on the acid molecules from one side, so that chains perpendicular to the surface were readily established. Indeed, probably a film of soap was formed between the glass and the palmitic acid; this view was certainly confirmed by a study of the layers of soap formed when the fatty acid was mounted on metal strips. The orientation of the molecules did not take place instantaneously, for when a drop of palmitic acid was placed on a cold, dry strip of glass, it set immediately, but the X-ray spectrum showed no trace of regular orientation. If, on the other hand, the glass strip was first heated and the acid allowed to set slowly, sharp spectra were obtained. By using films of palmitic acid formed by the slow evaporation of alcoholic solutions, it was possible to observe spectra at various stages of the setting of the acid film. It was found that even when the film presented a gelatinous appearance, the spectra showed several lines due to successive orders of reflection from equidistant planes. Moreover, although these lines at first were faint and diffuse, they became more and more sharp as crystallisation proceeded, and it seemed that the molecules at the surface of the glass constituted a framework for the production of a crystal face. When the film was very thin,  $1\text{ }\mu$  to  $0.02\text{ mm.}$ , the spectra only exhibited lines corresponding to large spacings, *i.e.* to the length of the chain, but as the film increased in thickness, the faint lines appeared and continued to increase in intensity, whilst the former lines dwindled and eventually disappeared when the thickness of the film was about  $1\text{ mm.}$  This suggested that, whilst the lowest layers were completely orientated, the directing effect of the glass became less and less pronounced in the layers further removed from its surface. Further experiments were made on films of palmitic acid deposited on crystalline surfaces, such as mica, quartz, and gypsum, on amorphous surfaces, such as cellulose acetate, and ebonite, and on metals. The experiments with

amorphous and crystalline surfaces permitted the conclusion that the amorphous or crystalline nature of the surface had no effect on the orientation of the molecules, and the only factor which played a part was the acidity or alkalinity of the surface.

In the case of the experiments with metal surfaces, the latter were carefully cleaned and polished, and a very thin film of palmitic acid was deposited by evaporation of an alcoholic solution. The metals Al, Fe, Ni, Cu, Zn, Mo, Pd, Sn, Sb, Pt, Au, Pb, and Bi were used, the films being examined in such a manner that no characteristic X-rays of the metal were generated. The spectra obtained, with the exception of those from films on the metals Al, Pd, Pt, and Au, presented a complex appearance, but each spectrum could be analysed into two distinct sets of lines, one set peculiar to palmitic acid and the other peculiar to the given metal employed. The latter set of lines was not due to the metal itself or to a special modification of palmitic acid, but was due to chemical action between the fatty acid and the metal support, resulting in the formation of a thin layer of soap, in which the molecules were orientated in a definite manner as in the film of acid. This explanation was amply borne out by later experiments, and it agreed well with the results of the experiments on the alkalinity of the glass strip mentioned above. The spacings for the soaps were obtained from the spectra, and as examples we may take the values for copper and zinc palmitate which were  $43.3 \text{ \AA}$  and  $40.1 \text{ \AA}$  respectively; these results may be compared with the values  $43.3 \text{ \AA}$  and  $40.0 \text{ \AA}$  obtained by Müller and Shearer (Solvay Congress, 1925). It is remarkable that such small quantities of acid reacting with the metal surface should produce such definite effects, and one realises at once the value of such a sensitive method for the study of surface reactions, and Trillat actually used the method to determine the constitution of the lead soaps of arachidic, daturic, and margaric acids. The method renders unnecessary the separate preparation of the soap.

There is a striking difference between the spectra obtained from the pure acid and the spectra from one of its soaps. The pure acid gives rise to intense odd order and weak even order spectra, whereas soap gives rise to spectra whose intensities decrease with increase in order. This may be explained (*cf.* de Broglie and Trillat, *C.R.*, p. 1485, 1925; Bragg, *Nature*, Feb. 21, 1925; Shearer, *P.R.S.*, vol. 108, p. 655, 1925), if we suppose that a fatty acid consists of a homogeneous medium of  $\text{CH}_2$  groups, which is intersected by  $\text{COOH}$  planes of high electron density and by the planes of low electron density which bisect the adjoining  $\text{CH}_2$  groups. These planes are,

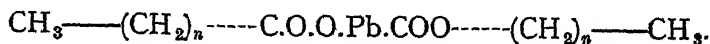
therefore, distributed alternately and separated by the length of a chain, which is consequently one-half the spacing distance. It may be shown that, from the point of view of X-ray reflections the planes of low electron density may be replaced by plane, of high electron density, providing that a phase difference of  $\pi$  is introduced when reflection occurs, so that the even order spectra are weakened and the odd order spectra enhanced. This view requires that each stratum of a fatty acid must consist of two layers of molecules whose  $\text{CH}_3$  termini are in opposition, and this view is borne out by the experimental evidence. Now, in the case of the soaps, we must imagine that the plane of high electron density passes through a group of atoms comprising the  $\text{COO}$  atoms and the metal atom, and the electron density in this plane will now in general be so high that the effect of the planes passing through the  $\text{CH}_3$  groups will be very small, and the decrease in intensity of the even order spectra will be small. However, some slight decrease should be observable, and, in fact, was definitely found in the case of the soaps which gave the most intense spectra.

We have already seen that these experimental methods may be used in the study of surface actions. Now, the work of Langmuir, Hardy, Adam, Harkins, Devaux, and Marcelin has proved that long-chain compounds, spread over the surface of water, arrange themselves so that their molecules are perpendicular to the surface, the  $\text{COOH}$  groups forming their extremities being, as it were, attached to the surface of the water. Trillat (*C.R.*, Jan. 1926) therefore used the reflection and transmission methods of X-ray examination to study the formation of thin films of palmitic and stearic acids on water; the transmission experiments, of long duration, were made with a quartz tube designed by Dauvillier. The results with the two acids were identical and showed that their molecules did not orientate in the same manner on water and on glass; for the transmission photographs of powdered palmitic acid showed only rings corresponding to the narrow spacings which produced the faint lines in the previous work, whereas the transmission photograph of a drop of acid, which had formed and set slowly on a water surface, showed in addition a series of rings corresponding to the normal large spacing,  $36.1 \text{ \AA}$ . Again, the reflection spectra from the surface of this drop which had been in contact with the water, showed the faint lines corresponding to a spacing of about  $4 \text{ \AA}$ , and two very feeble lines corresponding to the first and third orders of reflection from planes with large spacing. The interpretation of these results was that the molecules in immediate contact with the water were arranged as on a glass surface, but not quite

perpendicular to the surface. Many succeeding layers were then orientated parallel to the surface, and these were finally surmounted by a layer of molecules whose arrangement was arbitrary. In other words, the molecules in immediate contact with the water formed a thin layer somewhat as depicted by Langmuir, but above this was a comparatively thick layer of flat crystals.

Trillat (*loc. cit.*) further made a valuable addition to our knowledge of long-chain compounds, by an investigation of fatty acids, containing as many as 32 atoms of carbon, which were prepared by Gascard and Damoy. The greatest spacing measured was 74.0 Å, a considerable advance on the greatest spacing measured by Müller, viz. that of brassidic acid, 59.9 Å. The important point about these measurements was that at first they seemed to contradict the law enunciated by Müller and Shearer, but on dividing the fatty acids into two series, containing odd and even numbers of carbon atoms respectively, it was seen that the law held accurately for each series, but the increase in spacing due to the addition of a CH<sub>2</sub> group was different in the two cases. The two straight lines, representing the variation of spacing with number of carbon atoms, drawn through the experimental values of Trillat and of Saville and Shearer (*J.C.S.*, vol. 127, p. 591, 1925), intersect at a point corresponding to an acid containing 7 carbon atoms. It is interesting that the curves which represent the melting points of the odd and even acids also intersect in this region. It was impossible to detect any similar variation in the spacings which produced the faint lines, and which appeared to be the same for both series of acids. Measurements on the diacids, although much less numerous, exhibited the same difference between odd and even acids.

It was clearly of interest to examine the soaps in order to see if there was any corresponding difference in the spectra of soaps of odd and even acids. The lead and tin soaps of all the acids containing from 2 to 32 carbon atoms were prepared and examined by the simple method outlined above. In spite of the large spacings which were examined, the spectra all consisted of very intense, fine lines. Moreover, all the experimental values for lead soaps were accurately represented by a single straight line. This was because each soap contained an even number of carbon atoms, and the spacings observed corresponded to chemical formulæ of the type



A new and ingenious application of X-rays was further made by Trillat, in that he used the reflection method to observe

the course of chemical reactions, by observing the changes which took place in the X-ray spectra of oleic, linoleic, and linolenic acids, when these compounds were allowed to dry in air. Films of these unsaturated fatty acids were deposited on strips of polished lead and the X-ray spectra were determined at suitable intervals. In the case of oleic acid, the spectrum obtained immediately after deposition of the acid was very intense and corresponded to a spacing of 28.9 Å, but after some hours, an additional set of lines corresponding to a spacing of 37.5 Å was observed, and the film of acid at the same time showed an increase in thickness. Finally, after many hours, when the film appeared bright and elastic, no spectrum was to be observed. Similar effects were noted with the two remaining acids, but usually they were produced or accelerated by slight heating of the films. The results, then, indicated an increase in the spacing of the three lead salts with time, and the interpretation was that this increase in all cases was caused by the fixation of the same quantity of oxygen in the double bonds, rather than by polymerisation, which would have given rise to different effects in the three cases. The introduction of a molecule of oxygen into the double bonds thus caused an increase in length of the lead salt by an amount which was approximately 6 to 7 Å in each case.

It is well-known that ordinary crystals do not permit the examination of X-rays of great wave-length, but the new acids and soaps examined by Trillat will serve admirably for this purpose. Siegbahn (*Arkiv. för Matematik*, B18, No. 24, 1924) previously used palmitic acid films on glass to measure wave-lengths of the order of 17 Å, the  $L\alpha$  and  $L\beta$  rays of Cu, Ni, Co and Fe. The long-chain compounds used by Trillat, however, would permit the examination of wave-lengths up to 92 Å, and Dauvillier (*C.R.*, p. 1083, 1926) has measured the K spectrum of copper directly, using a film of melissic acid on lead.

Finally, Trillat (*C.R.*, p. 843, 1925) also examined various lubricants. W. H. Bragg showed that the cause of lubrication was the formation of definite layers able to slide over one another, which the lubricant could acquire merely under the action of pressure. Trillat rubbed his lubricants between two strips of glass or metal, and, finally, the two strips were separated by sliding. The preparations were then examined by the reflection method, and, in the majority of cases, definite spectra due to the orientation of the molecules were observed, so that definite layers of molecules were formed by friction. It is clear that long-chain compounds will play an ever-increasing rôle in practical X-ray work.

*Recent Work on Magnetism.*—Among the recent papers on

magnetism there are at least two which are worthy of notice. Firstly, on the theoretical side we have a paper by Stoner (*Proc. Leeds Phil. Soc.*, Jan. 1926), who discusses the different values, which are obtained by various methods, for the magnetic moment per atom of ferromagnetic substances. For instance, the values deduced from low temperature saturation intensities differ considerably from those obtained from the variation in susceptibility with temperature above the Curie point, and, moreover, these values bear no apparent relation to the values of the moments of the ions of ferromagnetic metals found by measurements on salts and solutions. The values of the gram molecular moments are usually expressed as multiples  $p$  of the Weiss unit magneton ( $1123.5 \text{ erg gauss}^{-1}$  per gram molecule), and as an example of the above differences we may consider some values of  $p$  for nickel. From the table compiled by Stoner, we see that from low temperature measurements on Ni—Fe alloys the value of  $p$  is 3.0, whilst the value from measurements on nickel above the Curie point is 8.0 (Ni  $\beta_1$ ). It will be remembered that Weiss (*C.R.*, vol. 180, p. 358, 1925) attempted to account for the nickel results by supposing that there existed magnetic carriers with moments of 3 and 8 magnetons, whose relative number changed gradually with increase in temperature; this explanation, however, cannot be regarded as satisfactory. On the classical theory such differences would arise if all the magnetic carriers had not the same moment, and on the quantum theory differences would certainly arise, because only discrete orientations may occur in a magnetic field, and the atomic moments would not in general be quintuple multiples of the Weiss unit—1 Bohr magneton = 5 Weiss magnetons—except at low temperatures. Now, Stoner shows that the experimental differences can be accounted for on the basis of the quantum theory, simply by supposing that the atoms in the crystals are associated in groups, and that the magnetic properties are due to ions within these groups which have the same moments as the ions in solid salts and in paramagnetic solutions. Thus in the case quoted above the nickel group is supposed to consist of five atoms, consisting of one  $\text{Ni}^{++}$ , one  $\text{Ni}^+$  and three neutral nickel ions. At low temperatures the Bohr magnetons associated with these ions are respectively two, one, and zero, whence  $p$  should be equal to 3.0 Weiss magnetons. At high temperatures, where several discrete orientations are possible, the Weiss magnetons associated with these ions are respectively sixteen, nine, and zero, so that theoretically  $p$  should be equal to 8.2 Weiss magnetons. Clearly these numbers agree extremely well with the experimental values, and it will be noted that Stoner's suggestion avoids the necessity of assuming a

change in the constitution of a ferromagnetic substance with temperature.

On the experimental side we have a valuable paper by Sucksmith and Potter (*P.R.S.*, vol. 112, p. 157, 1926), who have measured the specific heat of the ferromagnetic substances nickel and Heusler alloy up to temperatures well above their critical points. They successfully employed the Nernst-Eucken method of measurement up to temperatures greater than  $400^{\circ}\text{C.}$ , by placing their heated specimens inside an evacuated vessel enclosed in an electric furnace which was maintained at a known temperature  $T$ . The specimen was then heated electrically through a small increment of temperature  $\delta T$ . In addition, they made simultaneous measurements of the intensity of magnetisation of the specimen by the ballistic method, in order to discover any possible relation between the specific heat and the intensity of magnetisation. Now, according to the Weiss theory of ferromagnetism, the specific heat of a ferromagnetic substance should rise to a maximum at the critical temperature, and then decrease discontinuously, owing to the sudden disappearance of the magnetic energy. Many experiments on this point were carried out by Weiss and his collaborators (*cf. Journ. de Phys.*, vol. 7, p. 249, 1908; *Arch. Sci. Phys. Nat.*, vol. 27, pp. 352 and 453, 1909; *ibid.*, vol. 39, p. 451, 1915; *ibid.*, vol. 42, p. 378, 1916, and vol. 43, pp. 22, 113, 199, 1917), by means of the method of mixtures, but these experiments were certainly not sufficiently accurate to obtain conclusive information on this point. However, they concluded that in the case of nickel, at any rate, there was good agreement between experiment and theory. Sucksmith and Potter, whose experiments were carried out with a much higher degree of accuracy, obtained results which were definitely not in agreement with the Weiss theory, for at the critical temperature of nickel the observed decrease in the value of the specific heat was only 60 per cent. of that obtained by Weiss, and there was no sudden discontinuity, the decrease in specific heat taking place over a considerable range of temperature. They therefore suggested that, whilst changes in specific heat and intensity of magnetisation are closely connected, the relation between them is not so intimate as that suggested by Weiss, and, moreover, they formed the opinion that the critical point indicates a certain stage in a transition which takes place over a range of temperature of perhaps some hundred degrees, and which is not complete at the critical temperature.

*The Transmutation of Mercury.*—Such a considerable amount of interest has been taken in experiments on the transmutation of mercury into gold, that perhaps it will not be out of place

to refer to some recent experiments which may be taken to settle the question. Miethe in 1924 described the production of irregular amounts of gold in a Janicke mercury vapour lamp, operated at atmospheric pressure with a current of 12.5 amperes and running for 20 to 200 hours. Nagoaka similarly obtained gold by allowing sparks to pass between a tungsten pole and a mercury surface under transformer oil. The most recent experiments, made with the object of confirming or refuting these results, were carried out by M. W. Garrett (*P.R.S.*, vol. 112, p. 391, 1926), who passed condensed spark discharges at 15,000 volts between tungsten electrodes immersed in an emulsion of mercury droplets in transformer oil, between aluminium electrodes under the surface of distilled water carrying mercury in suspension, and between an iron pole and a mercury surface in an atmosphere of hydrogen. Only negative results were obtained in these experiments. The most conclusive experiment, however, was that in which Garrett caused an interrupted direct current arc to run for several days and nights between pure mercury electrodes in an atmosphere of hydrogen in a quartz tube. Only a small amount of mercury was used, and it was analysed without distillation on completion of the experiment. No gold was found, and it was suggested that the gold obtained by other workers was derived from the materials of their electrodes and vessels.

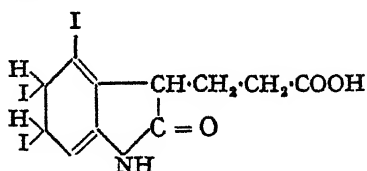
**BIOCHEMISTRY.** By R. KEITH CANNAN, M.Sc., University College, London.

*Hormones—The Purification of Insulin.*—The name of J. J. Abel will always be associated with the pursuit of the chemical characteristics of the active principles of the endocrine glands, and it has been, therefore, with a special interest that biochemists have learnt that he has addressed himself to the problem of the chemical nature of insulin. Dr. Abel and his collaborators have, indeed, made the claim recently that they have succeeded in isolating insulin in the crystalline state. In a paper published with E. M. K. Geiling (*J. Pharm. Exp. Thera.*, 1925, 25, 423) will be found a description of a fractionation of an active commercial preparation in which the chief novelties are the use of phenol as a solvent and of pyridine as a precipitant for the active principle. A product was thereby obtained some three to five times as active as the original material. The authors draw particular attention to the sulphur content of this product and place emphasis on their observation that treatment with sodium carbonate effects some change which results in the sulphur being readily removed

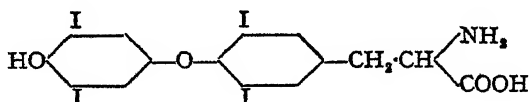
as hydrogen sulphide by subsequent treatment with dilute acids. They find that the physiological activity runs parallel with the amount of this labile form of sulphur in the insulin preparation and consider that the activity of the hormone may be connected with this characteristic. In a later paper (Abel, *Proc. Nat. Acad. Sciences*, 1926, **12**, 132) further purification is described, involving repeated precipitation by brucine and by pyridine, yielding ultimately a crystalline product. This crystalline material melts sharply at  $233^{\circ}\text{C}$ ., gives the biuret, Pauly, Millon, ninhydrin, and "labile sulphur" reactions, and, from some preliminary and scarcely satisfactory animal experiments, appears to be about ten times as active as the original commercial material. The work is obviously incomplete, and criticism may well await the report of the preparation of the product on a sufficient scale to permit accurate chemical analysis and biological assay. The possibility of insulin being a sulphur compound is certainly intriguing in view of the important place which we are at present inclined to give this element in tissue respiration. Meanwhile one great difficulty which has held up the elucidation of the rôle which insulin plays in carbohydrate metabolism seems to have been removed by the conclusion of C. H. Best, J. P. Hoet, and H. P. Marks (*Proc. Roy. Soc.*, 1926, **100B**, 32) that a great part of the glucose which disappears from the blood of the normal animal under the action of insulin is, in spite of frequent conclusions to the contrary, built up into glycogen. Best, H. H. Dale, and Marks (*Proc. Roy. Soc.*, 1926, **100B**, 55) have been able to show that in the eviscerated animal the whole of the missing glucose can be accounted for either as glycogen or as increased carbon-dioxide production. Thus the futile search for a hidden form of carbohydrate—a search which has engaged the attention of many—would seem to be rendered unnecessary.

*Thyroxine*.—Pride of place in any discussion of the chemistry of hormones to-day is due to the brilliant work of C. R. Harington on the active principle of the thyroid gland. By this work thyroxine becomes the second hormone—adrenalin being, of course, the first—which has been isolated in the pure state, whose chemical structure has been established and whose synthesis, but for one step, has been effected in the laboratory. Some ten years ago, E. C. Kendall (*Journ. Biol. Chem.*, 1919, **39**, 125) isolated thyroxine from thyroid glands in the crystalline condition, the yield being 11 grm. from a ton of fresh tissue. Harington (*Biochem. J.*, 1926, **20**, 293) has effected such improvements in the method as to increase the yield to 270 grm. from the same amount of raw material. The improved method has been found to be efficient on an industrial

scale. Kendall (*Journ. Biol. Chem.*, 1919, **40**, 265) had followed his success by the claim that synthesis confirmed analysis in giving to thyroxine the structure



No experimental details were, however, vouchsafed of the synthesis, and, in their absence, chemical improbabilities patent in the above structure denied general acceptance by chemists. The second paper by Harington (*Biochem. J.*, 1926, **20**, 300) justifies this scepticism by an alternative for which the evidence seems overwhelming. It would appear that thyroxine is to be represented as  $C_{15}H_{11}O_4NI_4$  and not (Kendall) as  $C_4H_{10}O_2NI_2$ . Harington's argument merits brief repetition. By reduction of the crystalline material of natural origin the author obtained a substance—desiodothyroxine—which differed from thyroxine only in the absence of four iodine atoms. This substance gave evidence of containing two benzene rings, united probably by an oxygen atom, a three-carbon side-chain, a phenolic hydroxyl, and an  $\alpha$ -amino-acid group. "At this stage," says the author, "it was decided to attempt to meet degradation by synthesis." The attempt was completely successful. Two independent methods gave a product chemically indistinguishable from desiodothyroxine. The final step of introducing the four iodine atoms remains to be achieved, but there can be little doubt that all the evidence points to the structure



It has been necessary to contrast the views of Harington and of Kendall, because the latter has built upon his conclusions wide generalisations as to the catalytic function of thyroxine in metabolism (Chandler Lecture, Columbia Univ., 1925, New York). Since these depend upon studies of synthetic substances having structures allied to that which Kendall attributes to thyroxine it must be agreed that, whatever merit they may have in themselves, they contribute nothing to the understanding of the rôle of thyroxine. By the same token the extension of the argument to adrenalin and to bios loses significance.

The improved method of preparation of thyroxine makes

more attractive the study of the physiological function of this hormone. Boothby and Sandiford (*Physiol. Rev.*, 1924, **4**, 69) have already carried out much suggestive work with the material prepared by the method of Kendall. They found that a direct relation existed between the amount of thyroxine in the circulation and the increase in the metabolism above the basal metabolic rate. One milligram raised the basal rate by 2.8 per cent. Moreover, since in myxoedema the rate is about 40 per cent. below the normal, and since the rate of decay of thyroxine in the circulation was found to be of the order of 0.4 mg. per day, it follows that 14 mg. should bring the metabolic rate of the subject to normal, and a further daily dose of 0.4 mg. should keep it at this level. These conclusions were very satisfactorily confirmed by experiment. Overdosage with thyroxine gave a clinical picture similar to hyperthyroidism and distinct from exophthalmic goitre. To explain this the authors make the interesting suggestion that in the latter condition there is not only hypersecretion by the thyroid but defective synthesis, so that modified substances, having special physiological effects, are thrown into the circulation. When we remember that Plummer claimed that massive doses of iodine reduced the metabolic rate in exophthalmic goitre, we wonder if the study of the physiological behaviour of desiodothyroxine would not provide results of some interest.

*The Œstrus Hormone.*—It has long been known that transplantation of the ovary prevented the atrophy of the uterus and the changes in the secondary sexual characters which normally follow ovariectomy. The Œstrus cycle was, therefore, naturally regarded as under the chemical control of a hormone acting through the blood stream. It was Iscovesco (*C.R. Soc. Biol.*, 1912, **73**, 104) who first demonstrated that certain fractions of the lipid extract of the ovary would induce the objective evidences of "heat" when injected into normal animals. O. Fellner (*Arch. Gynäk.*, 1913, **c**, 641) made a valuable extension of these observations when he reported that alcoholic extracts of the ovary or placenta produced hyperplasia of the uterus and vagina of spayed rabbits. The work was carried further by E. Hermann (*Monat. Geburtsch. u. Gynäk.*, 1915, **41**, 1), S. Frankel and M. Fonda (*Biochem. Z.*, 1923, **141**, 379), Giesy (*Thesis*, Columbia Univ., New York, 1920), R. T. Frank and Gustavson (*Journ. Amer. Med. Assoc.*, 1925, 1720), and, in an extensive series of papers, by E. Allen and E. E. Doisy and their collaborators. Hermann obtained a product from the fractionation of the alcoholic extract of the ovary which he claimed to be chemically pure. Frankel and Fonda made a similar claim for a thick viscous oil which they prepared and to which they gave the formula  $C_{32}H_{52}O_2$ .

They regarded the substance as closely related to cholesterol, and were satisfied of the existence in the molecule of two unsaturated groups, one hydroxyl and one carbonyl group. These claims to a pure product cannot be sustained, but, on the other hand, there does seem some unanimity on certain important properties of the hormone. All are agreed upon its lipoid-like solubilities, its thermostability, resistance to saponification by alkali, or lipase, and there is some evidence that it is slowly oxidised in the air. The preparations of Giesy were free from nitrogen and phosphorus and those of Gustavson and of Doisy (*Journ. Biol. Chem.*, 1924, **61**, 711) from cholesterol, fatty acids, and esters. It is not easy to compare the activities of the products reported from different laboratories because, until recently, no standard method of assay had been adopted. The method of Allen and Doisy (*Journ. Amer. Med. Assoc.*, 1923, **81**, 819) seems to have earned general approval and should assist comparative studies. They adopt as standard that dose which is necessary in the spayed rat to induce enough growth in the genital tract to cause formation of a cornified layer in the vaginal epithelium in forty-eight hours. A positive test is indicated when cornified epithelium replaces the leucocytes characteristic of vaginal smears of the untreated spayed animal. The best preparations from liquor folliculi gave a rat unit per 0.03 mg. (J. A. Ralls, C. N. Jordan, and E. E. Doisy (*Journ. Biol. Chem.*, 1926, **69**, 357)). Parkes and Bellerby (*Journ. Physiol.*, 1926, **61**, 1) and Dickens, Dodds, and Wright (*Biochem. J.*, 1925, **19**, 853) have obtained products of similar purity. Since the latter have shown that the dose varies with the time which has elapsed since ovariectomy, strict comparison is not justified. Lipschutz, Vesujakov, Tuisk, and Adamberg (*C.R. Soc. Biol.*, 1926, **94**, 738) have very recently claimed to have so far purified the product obtained by the method of Allen and Doisy as to get a positive reaction in the mouse in a dose of 1/50,000 mg. If this be substantiated, such a product outstrips in activity all others reported in the literature. They obtained 5 mg. from half a litre of liquor folliculi.

No general unanimity prevails on the question of the seat of synthesis of the hormone. Frankel and Hermann both found it in the *corpus luteum*, but Allen, Pratt, and Doisy (*Journ. Amer. Med. Assoc.*, 1925, **85**, 399, and **86**, 1964) failed to confirm this, and Johnston and Gould (*Surg. Gynec. and Obst.*, 1926, Feb., 236) suggest that the error was due to insufficient care in the collection of the tissues. The former workers have made a survey of the distribution between placenta, ovary, and follicle in different animals. Parkes (*Proc. Roy. Soc.*, 1926, **100B**, 173) finds, in studies on the effect of X-rays upon oestrus, evidence that the interfollicular tissue of the ovary

is the chief factory of the hormone. Though there have been suggestions to the contrary, it would appear that the hormone so far studied is only one of the internal secretions of the female genitalia. The inter-relations of œstrus and the secondary sexual characters indicate that only the first step has yet been taken in elucidating the endocrine functions of the female reproductive glands.

*Secretin*.—Since the classical work of Bayliss and Starling, it has been held that the stimulus to the external secretion of the pancreas was the appearance in the blood of secretin. This was supposed to exist in the duodenal mucosa as prosecretin and to be converted into the active substance and pass into the blood under the action of the acid chyle passing down from the stomach. J. Mellanby has recently suggested certain important modifications of this view. In the first place J. Mellanby and St. J. Huggestt (*Journ. Physiol.*, 1925, **3**, 698) believe that secretin exists preformed in the duodenal mucous membrane. They are able to extract it with a variety of neutral solvents as well as by dilute acid. Secretin is water soluble, thermostable, and allied to the secondary proteoses in that it is precipitated by complete saturation with ammonium sulphate and rapidly destroyed by pepsin and by trypsin. In a later paper (Mellanby, *Journ. Physiol.*, 1926, **61**, 419) the stimulus to the secretion of the hormone is sought and found in the bile. The active constituent is said to be cholic acid, but its activity is modified by other constituents of the bile, by the state of digestion of the animal and by the reaction of the duodenal contents. In brief, it is believed that secretin is absorbed into the blood-stream along with the bile acids. The development of this hypothesis will be awaited with interest.

**PHYSICAL CHEMISTRY.** By R. K. SCHOFIELD, M.A., Ph.D., University, Durham.

*Molecular Orientation at Interfaces*.—At a recent meeting the Faraday Society held a "General Discussion on Physical Phenomena at Interfaces with Special Reference to Molecular Orientation." The present occasion is thus a suitable one for taking stock of the position in this important and rapidly advancing field of physical chemistry.

Physicists of a generation ago set themselves the task of explaining in terms of the Dynamical Theory as many as possible of the general properties which matter possesses irrespective of its chemical nature. To this end they thought in terms of a "generalised" molecule which, very naturally, was spherical in form, and was surrounded by a symmetrical field

of force. While this molecular model held the field the possibility of molecular orientation could not be discussed. In recent years, however, the establishment of the Dynamical Theory by Perrin, and the new knowledge which has sprung from the discovery of X-rays and the radiations from radiative bodies, have done much towards removing the artificial barrier which had grown up during the nineteenth century between the molecule, as pictured by physicists, and that as pictured by chemists.

Hardy (*Roy. Soc. Proc.*, 1912, A 86, 610) was the first to consider the consequences of supposing molecules to be surrounded by an unsymmetrical field of force. He showed from quite general considerations that such a molecule, if situated in the interfacial region where one fluid phase shades into another, will have minimum potential energy when orientated in one particular direction. Thermal agitation will, however, be continually tending to upset the regular arrangement which would otherwise obtain. Thus the actual degree of orientation will depend on the relative importance of these opposing influences. Hardy did not immediately follow up the matter, and it was left to Langmuir (*Met. Chem. Eng.*, 1916, 15, 468, and *J.A.C.S.*, 1917, 39, 1848) to demonstrate the importance of this new idea.

The strength of the orientating influence will depend on two factors: (a) the degree of asymmetry of the field due to the molecule, and (b) the abruptness of the transition of one phase into the other of the interface.

(a) The remarkable agreement between the arrangement of the atoms in the molecules of crystals as revealed by X-rays and that argued from their chemical properties has shown that its chemical formula is a reliable indication of the actual shape of a molecule. This, coupled with the realisation that the constituent atoms themselves consist of a distribution of unit electric charges, demonstrates that the electromagnetic field near the molecule must vary greatly from point to point. At distances from a molecule great compared with its linear dimensions the field may approximate to one of spherical symmetry. It is necessary, therefore, to obtain an estimate of the distance at which the field due to a molecule ceases to be effective in order to obtain some idea of the relative importance of the highly localised field close to the molecule and the more symmetrical field without. Laplace built up his theory of capillarity on the idea that the effective "sphere of influence" of a molecule is large in comparison with its volume. Were this the case we should not anticipate any considerable orientating influence at an interface. It was shown by Maxwell, however, (cf. Edser, *4th Report on Colloid Chemistry*, H.M. Stationery

Office, 1922) that, if the force between two molecules be supposed to vary inversely as some power of their distance apart, that power cannot be less than 8. This means that the field at the surface of such a molecule would be  $2^8 = 256$  times as great as at a distance from it equal to the molecular radius. Hence the part of the field round a molecule which is chiefly responsible for the molecular cohesion in liquids and solids is that very close to the molecule. Further, we should expect the effective field to vary over the surface of the molecule according to the chemical nature of the atom or atomic group in the immediate vicinity. Thus we conclude that a molecule which has different atomic groups at either end will be surrounded by an unsymmetrical field.

(b) Maxwell's computation has also an important bearing on the thickness of the transition layer at an interface. For whereas on the Laplacian view this is great in comparison with molecular dimensions, one phase shading gradually into the other; Edser has shown (*loc. cit.*) that, assuming an 8th power law, 95 per cent. of the surface free energy is associated with a single layer of molecules. Hence we have good reason to believe that most of the change from a phase to another at an interface occurs in a layer a few ÅU in thickness.

The necessity for considering the asymmetry of molecular fields was impressed upon Hardy by his study of the conditions under which oils will spread on water. A drop of an insoluble hydrocarbon oil (such as medical paraffin) when placed on water forms a lens which has no tendency to spread, and has no effect upon the tension of the surrounding water surface. If, on the other hand, an oil in which a group such as  $-OH$ ,  $-COOH$ ,  $-Cl$ ,  $CN$ ,  $COOCH_3$ , etc., has been substituted be used, a thin film spreads out from the lens, and when it covers the whole surface, reduces the surface tension. This Hardy accounted for by supposing these groups, which he termed "active," to be surrounded by a field in consequence of which they are more strongly attracted to the water than are the hydrocarbon chains. Further, the failure of a pure hydrocarbon oil to spread must be interpreted as showing that hydrocarbon chains adhere to each other more readily than they do to water. Thus Hardy's observations on spreading (which have been extended by Harkins and his co-workers) would lead us to anticipate that molecules of insoluble substituted hydrocarbons would have a minimum potential energy on a water surface, when arranged with their active groups in contact with the water, and their hydrocarbon chains packed side by side. It should be noted, however, that such an arrangement is probable only if the transition at the water an interface is relatively abrupt, and that its actual existence is

conditional upon the inability of thermal agitation to break it up.

The credit for establishing the existence of such orientation in these films (which Rayleigh and Devaux had already regarded as unimolecular) is due to Langmuir (*loc. cit.*). The extended researches of Adam have shown that the insoluble fatty acids  $C_{14} - C_{18}$  all form such films (which Adam terms "condensed") when placed in pure water at room temperature (*Roy. Soc. Proc.*, 1922, A 101, 452). These films have a lateral compressibility of the same order as that of the oil in bulk, and often show marked rigidity. They have been compared by Shearer to the layers in the crystals of these substances, which, following Sir William Bragg, he has examined under X-rays (*Roy. Soc. Proc.*, 1925, A 108, 655). He suggests that "any attempt at orientation is essentially an attempt at crystallisation: the more we learn as to the relative orientation of molecules in the crystal the more expert shall we become at predicting under what conditions orientation will occur, and when it occurs what its nature will be." Shearer's latest measurements on the spacing between the hydrocarbon chains in crystal layers lead to a value  $20.7 \text{ \AA}$  for the area occupied by a vertically orientated chain, a value in excellent agreement with that,  $20.4 \text{ \AA}$ , recently given by Adam from his measurements on "condensed" films. Gorter and Grendel have obtained films with proteins such as hæmoglobin and casein, which indicate that the molecules lie flat on the surface, covering a superficial area of some  $2,000 \text{ \AA}^2$ , and being only  $7 \text{ \AA}$  in thickness. This conclusion is only justifiable if the films are really of the "condensed" type, upon which point there is insufficient evidence at the present time.

Besides fluid and rigid "condensed" films three other types of films are known: "liquid expanded," "vapour expanded," and "gaseous" (Adam, *Chem. Reviews*, 1926). Schofield and Rideal (*Roy. Soc. Proc.*, A, 1925, 109, 57; 1926, 110, 167) have critically examined the data for these films. From the quantitative correspondence between their behaviour and that of the analogous states of three-dimensional matter they conclude that these films lower the surface tension by exerting an outward force over the surface, and that this force is kinetic in origin (*cf.* SCIENCE PROGRESS, June 1926). In the "liquid expanded" film the disruptive force of thermal agitation has increased the molecular area "at zero compression" from  $20.4 \text{ \AA}^2$  to about  $48 \text{ \AA}^2$ . The cohesion exhibited by these films shows, however, that portions, at least, of the molecules are still in contact, and Adam has suggested that in these circumstances the hydrocarbon chains are "coiled down" in a spiral, and hence occupy a larger area. With the "vapour

expanded " and gaseous films the disruption has gone still further. If, however, we assume the essential correctness of the kinetic theory of surface-tension lowering, we obtain evidence that under high surface pressures molecules such as those of a fatty acid when in collision are vertically orientated since the two-dimensional Amagat equation leads to a limiting area  $24 \text{ \AA}^2$ . Since such molecules adhere mainly by their hydrocarbon chains they may become considerably inclined as the heads are driven apart by molecular bombardment. Moreover, following Langmuir, it is generally believed that the chains tend to lie flat on the water surface when the molecules are sparsely distributed as in a " vapour expanded " film.

Langmuir urged the essential continuity between the films of insoluble fats and those formed by their soluble homologues at the surface of dilute solutions in accordance with Gibbs's adsorption equation; but his arguments were insufficient to prove the point. The work of Adam on the insoluble films and of Schofield and Rideal on adsorbed films on dilute solutions have, however, amply confirmed this view. In the opinion of Adam the observed continuity " constitutes a proof that the adsorbed films are one molecule thick and also forms perhaps the best verification of Gibbs's equation which has yet been given."

Going further, Langmuir and, independently, Harkins, Darris, and Clark (*J.A.C.S.*, 1917, **39**, 541) put forward the view that the surfaces of pure liquids are covered by a unimolecular film of molecules, orientated with their active groups towards the liquid. To substantiate this view they postulate that the total energy  $\sigma - Td\sigma/dT$  associated with a sq. cm. of surface (a quantity which is nearly independent of the temperature) is determined solely by the nature of those portions of the molecules which form the free surface. Were this the case, we should expect the total surface energies of all  $\alpha$  substituted aliphatic hydrocarbons to be the same, since the more " active " groups would be drawn inwards, and the surface consist in each case of the  $\text{CH}_3$  - groups at the other ends of the chains. Though roughly the case, this is not strictly true. Moreover, Sugden (*J.C.S.*, 1924, **125**, 1167) has shown quite definitely from his study of mono and *p*.di-substituted benzene derivatives that the value of the total surface energy is influenced by *all* the groups in the molecule. From this we must conclude, either that the molecules in the outermost layer are not completely orientated as Langmuir and Harkins suppose, or that, if they are, the total surface energy is not determined solely by the nature of the groups of atoms that form the surface. Sugden expresses the opinion " that the surface properties of pure liquids are best accounted for by the theory

of a random distribution of surface molecules." While there is little doubt that the views put forward by Langmuir and Harkins were not sufficiently carefully considered, yet from the general standpoint advanced by Hardy a *tendency* to orientate is undoubtedly to be expected here as well as in the case of films. Furthermore, the values of the potential differences across the surfaces of organic liquids, as measured by Franklin (*Zeit. Phys. Chem.*, 1924, **109**, 34, **111**, 190, and 1925, **116**, 485) seem hardly explicable unless we suppose such molecules to have an electric moment, and to be in some measure orientated in the surface layer.

There is a difficulty, however, in dealing with pure liquids which does not arise in the case of films, for here molecules are continually leaving the liquid, and entering the vapour, and the same number are returning to the surface. From the number of vapour molecules which, on the kinetic theory, strike unit area in unit time it can readily be shown that the mean life of a molecule on the surface of a liquid of which the vapour pressure is 20 mm. will be of the order of  $10^{-7}$  secs. at room temperature. Sugden calculates that in Brownian movement a molecule will take about  $10^{-9}$  secs. to turn through  $180^\circ$ , and equates this to the time necessary for an orientated film to form. In this case orientation would cease to appreciate when a liquid has any considerable vapour pressure. It should be noted, however, that the essence of Hardy's theory is that orientation occurs when the tendency to come to a position of minimum potential energy is strong enough to *prevent* the molecule from rotating freely in the course of Brownian movement. Thus it would appear that if orientation occurs it must do so in a time less than  $10^{-9}$  sec., and probably considerably less.

The work of Einstein, Born, and Debye supports the conclusions already referred to as to the relatively small range of molecular attraction, and leads to the conclusion that molecular field falls to one half its value at the surface of the molecule in a distance of  $0.3 \times 10^{-8}$  cm. Thus we should expect that the first layer of adsorbed material would be held very much more firmly to a solid surface than successive layers. We should also expect orientation as on liquid surfaces, the molecules being held by those groups which the more readily enter into combination with the surface atoms. Usually so-called active groups are towards the solid surface, for these are most unsaturated. The work of Hardy (*Proc. Roy. Soc.*, 1922, A **100**, 150 ; 1923, A **101**, 487 ; 1925, **137**, 1207 ; *J.C.S.*, 1925, **127**, 1207) on the effect of organic substances on the coefficient of friction between solid surfaces has shown that molecules are more strongly adsorbed by

their polar than by their non-polar groups. Thus OH and COOH are adsorbed more strongly than  $\text{CH}_3$  and compounds containing the former will displace hydrocarbons from the surface. The lubricating power increases with the tenacity with which the polar groups are held on the surface.

Evidence of molecular orientation at solid surfaces is available from the behaviour of crystalline liquids at plane surfaces (*Flüssige Kristalle*, 1904; Freidel, *Ann. Phys.*, 1922, 17-18, 304) and in the mode of crystallisation of long-chain acids on the surface of mica (*J.C.S.*, 1923, 123, 2043, 3152, 3156). Also, Constable and Palmer (*Proc. Roy. Soc.*, 1925, 107, A 255) have shown from measurements that the velocity of decomposition of alcohols is constant. This they explain as due to the terminal group,  $\text{CH}_2\text{OH}$ , being adsorbed by the surface in all cases.

Bartell and Miller (*J. Phys. Chem.*, 1924, 28, 992) have obtained definite evidence that, with organic polar compounds, orientation on a carbon surface occurs from the solution. They conclude that the polar groups are extended into the water phase and that the hydrocarbon chain is directly in contact with the surface. A similar orientation is found when these substances are adsorbed from solution on to a mercury surface as evidenced by the interfacial potential difference they set up (Frumkin, *loc. cit.*).

When orientation is accompanied by lateral adhesion, as is the case when aliphatic alcohols are absorbed from very dilute solution by charcoal, Garner and Knight (unpublished) have found that Langmuir's equation  $x = abp/(1 + ap)$  ceases to hold. The adsorption isotherms are similar to those for the corresponding compounds at a water-air interface.

Finally, the evidence goes to show that orientation of the outer layer of molecules on a solid surface appears when this layer is part of a crystal. The recent discovery of the existence of "patches" on many surfaces which have markedly different catalytic properties suggests that the surface molecules of the solids concerned are not arranged in an orderly fashion.

**GEOLOGY.** By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow. *Sedimentary Rocks*.—Dr. G. W. Robinson has performed a useful service in writing of soil geology from the view-point of the modern agricultural chemist ("Pedology as a Branch of Geology," *Geol. Mag.*, lxi, 1924, pp. 444-55). From this standpoint the soil is regarded as a colloidal system in which the essential constituents are colloidal clay and colloidal organic matter. The former is a material of indefinite composition containing silicic acid and the hydrated oxides of

aluminium and iron. In association with organic matter in the same physical condition it forms a gel coating on the surfaces of the more or less decomposed mineral particles of the soil. The colloidal matter, organic and inorganic, is conceived as a matrix surrounding the mineral particles, facilitating their aggregation into compound particles or crumbs. The term *pedology* for soil science must, however, be deprecated, on account of its resemblance to certain medical terms.

An excellent introduction to the petrography of coal has been written by R. Potonié, the son of H. Potonié of coal investigation fame (*Einführung in die allgemeine Kohlenpetrographie*, Berlin, 1924, 285 pp.). The book deals with the origin and classification of coal, its petrographic constituents and microscopic structures. The book is thoroughly up-to-date in its matter and method, and describes very fully the technique of Jeffrey, Lomax, and others, in the preparation of coal for investigation.

In his paper on the origin of boghead coals, R. Thiessen (*Prof. Paper*, 132-I, *U.S. Geol. Surv.*, 1925, pp. 121-38) rehabilitates the theory of the algal origin of these rocks by calling attention to the discovery of richly oil-bearing algae in salt lagoons in South Australia and in Turkestan. These plants form a scum which is blown at times upon the land, and there consolidates to masses of a rubber-like substance called Australian caoutchouc or *coorongite*. In thin section this material shows identical appearances with those of boghead coal. Accordingly, coorongite is regarded as the peat stage of boghead coal. The oily composition of these algae obviously removes one of the great difficulties of the algal theory of the origin of boghead coals.

C. B. Lipman has carried through a critical and experimental study of Drew's bacterial hypothesis of the precipitation of calcium carbonate in the sea (*Carn. Inst. Washington, Publ. No. 340*, 1925, pp. 181-91), and has come to conclusions diametrically opposed to Drew's. Drew ascribed the phenomena to a specific organism (*Pseudomonas calcis*), but he worked with media rich in nitrogenous compounds and organic acids, partly in combination with calcium salts. Lipman's results show that no fewer than 32 species of bacteria isolated by him can precipitate calcium carbonate from sea-water if certain salts are added to the medium, but in no case did he obtain precipitation from pure sea-water. Lipman concludes that great calcareous deposits are merely reworked remains of animals and plants, and that calcium carbonate precipitation results from a number of appropriate chemical and biochemical reactions which do not require the participation of bacteria.

In a discussion of the Chalk as a possible chemical deposit,

Prof. W. A. Tarr (*Geol. Mag.*, lxii, 1925, pp. 252-64) states that it consists dominantly of three types of material: (1) amorphous, or extremely fine-grained material; (2) spheres; (3) organic remains. The first is believed to represent chemically-precipitated calcite and aragonite; the second a chemically-formed oolite; and the third the normal organic constituents. Recent studies show that the surface waters of the ocean in certain localities are saturated with calcium carbonate, and variations in the amount of carbon dioxide mean either solution or precipitation of this material. It is believed that the physical conditions of the Cretaceous sea favoured precipitation, in which process bacteria may have been a contributing factor.

In a most valuable and comprehensive memoir (*Mem. Geol. Surv.*, "Special Reports on the Mineral Resources of Great Britain," xxix, 1925, pp. 139), Mr. A. F. Hallimond discusses the petrography and chemistry of the bedded iron ores of England and Wales. These ores are classified as follows: *Ferrous Iron Ores*, including (I) chamositic mudstones and chamosite-siderite mudstones, oolitic or compact, *e.g.* the Cleveland ironstone; (II) siderite mudstones, non-chamositic and compact, *e.g.* the Coal Measure ironstones; (III) sideritic limestones, *e.g.* the Marlstones; and *Ferric Iron Ores*, including (IV) ferric chamosite oolites with free oxides of iron, *e.g.* the North Wales pisolitic ore; (V) limonite oolites, *e.g.* the Frodingham ironstone; (VI) primary hæmatites, *e.g.* the Crinoidal Hæmatite of South Wales; and (VII) Glauconitic rocks, not of commercial value as ores. The mineralogy and petrology of these groups are fully studied. Such rocks as the Cleveland ironstone, formerly thought to be due to the replacement of oolitic limestone by iron-bearing solutions, are now shown to be of direct marine deposition as loose, sea-floor aggregates of ooliths, shell-fragments, rolled pieces of consolidated ironstones, and other detritus; and the ferrous material is shown to be a precipitate from sea-water.

From a study of some occurrences of spherulitic siderite and other carbonates in sediments, Dr. E. Spencer (*Q.J.G.S.*, lxxxix, pt. 4, 1925, pp. 667-705) concludes that the formation of these structures has depended in some way on the clayey or fine-grained nature of the sediments, the presence of carbonaceous matter, and probably the absence of shelly carbonate of lime. The spherulites have formed from iron carbonate solutions held by adsorption within the gradually-settling and consolidating sediments.

Prof. F. F. Grout has attempted to correlate the chemical composition and texture of clays by experiment on twelve Minnesota examples (*Bull. Geol. Soc. Amer.*, 36, 1925, pp. 393-

416). These were separated mechanically into fractions of different grain sizes, which were then analysed. Silica was found to be at a maximum in the fine sand grade of most clays. Alumina, iron oxides, and potash, with minimum amounts in fine sand grades, are at their highest in fine clays. Other oxides show less regularity in their variations.

In a paper on iron-stained sands and clays, G. R. McCarthy (*Journ. Geol.*, **34**, 1926, pp. 352-60) points out that the distribution of colour in a sediment is as important in determining the general colour as the total amount of colouring matter present. It is shown that quartz will become iron-stained only in the absence of more active adsorbents; that orthoclase acquires iron-staining more readily than quartz; and that while  $\text{Al}(\text{OH})_3$  is a good adsorbent of iron, pure kaolin will adsorb but little, unless activated by some substance such as an alkali carbonate. In clays the iron content up to about 5 per cent.  $\text{Fe}_2\text{O}_3$  is a linear function of the alkali content, and an equation is given.

The study of the petrology of the sediments of the British geological column continues apace, and valuable additions to stratigraphical petrography are made in a number of papers contributed to the *Proceedings of the Geologists' Association* (A. Stuart, "Petrology of the Dune Sands of South Wales," *Proc. Geol. Assoc.*, **35**, 1924, pp. 316-31; I. S. Double, "Petrography of the Later Tertiary Deposits of the East of England," *ibid.*, pp. 332-58; S. W. Wooldridge and D. M. C. Gill, "The Reading Beds of Lane End, Bucks, and their Bearing on some Unsolved Problems of London Geology," *ibid.*, **36**, 1925, pp. 146-73; E. Neaverson, "Petrography of the Upper Kimmeridge Clay and Portland Sand in Dorset, Wilts, Oxfordshire, and Bucks," *ibid.*, 240-56; M. P. Latter, "The Petrography of the Portland Sand of Dorset," *ibid.*, **37**, 1926, pp. 73-91).

The conditions of deposition of the Stockdale Shales of the Lake District have been investigated by Prof. J. E. Marr (*Q.J.G.S.*, lxxxii, pt. 2, 1925, pp. 113-36). These sediments consist of dark graptolitic muds, followed upwards in order by blue, green, and, lastly, red muds. Benthonic organisms are practically absent from the grey to black shales, rare in the green, more frequent in the blue and red bands; they are always dwarfed. It is argued that the paucity and small size of the benthonic organisms is the result of poisonous conditions during deposition of the sediments. The rocks as a whole were deposited in still waters away from the coastal belt. An analogy is drawn between the pyritic muds of the Black Sea, deposited in the stagnant poisoned water below the 100-fathom line, and the black shales of the Stockdale horizon. "The deposit of fine mud, abundant plankton, the supply of

decomposing organic matter maintained by the algæ and other organisms sinking to the muddy floor, and the consequent development of  $\text{SH}_2$  shown by the abundance of iron sulphide in the beds, are similar in each case."

Other recent studies of assemblages of sediments, and of their conditions of deposition, are those by the late Prof. J. Barrell ("The Nature and Environment of the Lower Cambrian Sediments of the Southern Appalachians," *Amer. Journ. Sci.*, ix, 1925, 1-20), and by E. Norin ("The Lithological Character of the Permian Sediments of the Angara Series in Central Shansi, North China," *Geol. Fören. Stockholms Förh.*, 46, 1924, pp. 19-55).

*Igneous Rocks.*—Prof. K. H. Scheumann has written a full discussion and summary of non-German work of recent years on the classification and nomenclature of igneous rocks ("Ausländische Systematik, Klassifikation, und Nomenklatur der Magmasteine," I, *Fortsch. d. Min., Krist., u. Petr.*, 10, 1925, pp. 187-310), which may be read with much profit by all petrologists interested in this side of the science.

Prof. H. G. Backlund contributes an interesting summary of his work on the igneous activity associated with the Andean tectonic cycles of the cordillera of Mendoza, Argentina (*Geol. Mag.*, lxiii, 1926, pp. 410-22). The igneous activity began with isolated spilitic extrusions, and a correlated plutonic phase of soda-granite, etc., associated with geosynclinal subsidence in Triassic times. Subsequently there were five minor cycles, characterised by granodiorites and andesites with subordinate dacite. These rocks are correlated with, and are characteristic of, corresponding orogeneses. Basic alkaline rocks also occurred sparsely at the end of each cycle. The major tectonic cycle, including the whole of the above phases, concluded with basaltic eruptions and gabbroid intrusions. The post-orogenic evolution following each cycle, and especially the last one, took place with extrusion of plateau basalts accompanied by a general uplift. This paper is a most important contribution to the rapidly growing theory of the connection of igneous activity with tectonics.

In connection with this topic brief mention may be made of a synopsis of the writer's Presidential Address to the Geological Society of Glasgow (*Geol. Mag.*, lxiii, 1926, pp. 284-6), which dealt with the connection between Igneous Action and Earth Movement in Scotland and Scandinavia. An attempt is made in this Address to correlate the sequence of igneous and tectonic events with the thermal cycle of Joly and Holmes.

Prof. W. Eitel ("Physikalisch-chemische Mineralogie und Petrologie," *Wissensch. Forschungsber. Naturwiss.*, Reihe xiii, 1925, 174 pp.) has produced a most valuable digest of the

literature of physico-chemical mineralogy and petrology for the last decade. The book is divided into a general part and a special part; the former dealing with the advances in the application of physico-chemical theory to rocks and minerals; the latter with individual minerals. The book would be invaluable if only for its wealth of references. A further volume on the development of special physico-chemical petrography is promised.

T. L. Tanton (*Journ. Geol.*, xxxiii, 1925, pp. 629-41) has described a quartz-porphyry on the north shore of Lake Superior in which there are abundant globules of glass in a matrix consisting chiefly of glass. Analysis shows that there is a slight difference (chiefly in  $\text{SiO}_2$ ) between the composition of the globules and that of the matrix; and there is evidence that the globules were liquid at the same time as the surrounding rock was liquid. These facts are put forward as a proof of the occurrence of limited liquid immiscibility in silicate magmas.

From a study of the same material G. W. Bain (*Amer. Journ. Sci.*, 11, 1926, 74-88) rejects the immiscibility theory, and regards the black porphyry orbs as autoliths or cognate inclusions formed during an early period of consolidation of the magma. Dr. N. L. Bowen (*Journ. Geol.*, xxxiv, 1926, pp. 71-3) rejects both the immiscibility and the xenoliths theory, and regards the globules as remnants of the dark, unaltered porphyry left after the rock had been affected by solutions which reddened it.

By the discussion of variation diagrams in which composition of the rock is plotted against distance from the outer edge of the intrusive mass, G. W. Bain (*Journ. Geol.*, xxxiii, 1925, pp. 509-25) comes to the conclusion that the Sudbury norite has assimilated quartzite, arkose, etc., to an amount of rock equivalent to two-thirds of its original volume, and possibly as much as two or three times that amount. This view is severely criticised both by Dr. T. C. Phemister (*ibid.*, pp. 819-24) and by Dr. N. L. Bowen (*ibid.*, pp. 825-9). The latter regards the "added" siliceous material as merely the late acid residuum due to the normal process of differentiation.

R. C. Emmons describes cases of mafic xenoliths in granites of the Canadian Shield which are illustrative and confirmatory of Bowen's views of magmatic reaction (*Journ. Geol.*, 34, 1926, 422-8). In many examples there has been profound reaction between the granite magma and large inclusions, which has resulted in the advancement of the crystallisation of a part of the magma to the pegmatite stage. Pegmatite is associated and increases in amount with the relative abundance of inclusions.

In a description of the teschenite sill of Charlestown, Fife (*Geol. Mag.*, lxiii, 1926, 343-7), Dr. F. Walker has pointed out the mineralogical and chemical identity of certain grey veins in this rock, with the type analcite-syenite of Mauchline, Ayrshire. From this he draws the conclusion that the differences between the teschenite and crinanite suites have been over-emphasised as analcite-syenite is now shown to be a felsic differentiate of teschenite as well as crinanite. He also marshals some evidence to show that, on the whole, the analcitic suite in the east of Scotland is of earlier date (probably Lower Carboniferous) than that of the west.

With the aid of an extensive Survey collection of the dyke rocks of Galloway, 500 of which were collected in Wigtownshire alone, Dr. H. H. Read describes the petrography and distribution of the mica-lamprophyres of that county (*Geol. Mag.*, lxiii, 1926, 422-9). They are mostly kersantites, and with other types, form the dyke phase of the Galloway province of Lower Old Red Sandstone age. The most important result of their study is that the group is localised within a definite belt of country, which runs with a north-east to south-west trend parallel to the strike of the country rocks, from a point on the western shore of Wigtown Bay near Kirkcinner, through Port William in Luce Bay, to the tip of the Mull of Galloway.

Dr. P. Eskola begins a series of memoirs on the petrology of Eastern Fennoscandia with a paper entitled "The Mineral Development of Basic Rocks in the Karelian Formations" (*Fennia*, 45, No. 19, 1925, 93 pp.). The basic rocks are partly albite-clinopyroxene rocks (spilites) and partly greenstones derived by low-grade metamorphism from the spilites. The paper closes with a very full discussion of albitisation and the genesis of spilitic rocks. In albite-clinopyroxene mixtures the eutectic point must lie close to the albite end of the series, and the pyroxene should therefore crystallise first from nearly all possible mixtures. But, as the ophitic relation holds both in the albite-clinopyroxene rocks and in the albite-hornblende rocks which are described, the original felspar must have crystallised first, and must therefore have been of calcic composition. Hence the conclusion is drawn that the albite is due to late magmatic reaction between these early crystals and a soda-rich residuum, with the loss of lime. Eskola holds that the original magma differed but little from ordinary basalt.

An important petrographical memoir by J. Barthou entitled "Chronologie et Description des roches ignées du désert arabe" (*Mém. à l'Inst. d'Égypte, Caire*, tom. v, 1922, pp. xxvii + 262) has come belatedly to hand. It contains

descriptions of six igneous series in the "Arabian Desert," which means the region between the Nile and the Red Sea. The first four series, two plutonic and two volcanic, are of Palæozoic ages, and seem to the writer to represent the plutonic and volcanic phases of two granodiorite-andesite kindreds. The fifth series is constituted by the basalts of Sinai, which are regarded as of Cenomanian age; and the sixth is an interesting alkaline series (Wadi Natasch) consisting of nepheline-syenite, tinguaitite, phonolite, trachyte, mugearite, and basalt, forming domes, necks, and flows, and of Upper Cretaceous age. The series concludes with a brief description of a "Mediterranean basalt" of Miocene age from east of Cairo.

A deep boring for coal to a depth of over 1,200 feet through the Deccan traps at Bhusawal, Bombay Presidency, was unsuccessful, owing to the unexpected downfaulting of the lavas; but the cores provided Dr. L. L. Fermor with an unexampled opportunity for the petrographic study of a great section of the Deccan basalts, of which full advantage has been taken (*Rec. Geol. Surv. India*, lviii, pt. 2, 1925, pp. 93-240). Twenty-nine distinct flows were encountered in the boring, of an average thickness of 40 feet. All the lavas were of basaltic composition; they differed in respect to the presence or absence of labradorite phenocrysts and olivine crystals, the uniform or non-uniform distribution of these minerals when present, the character of the iron ore minerals, the shapes and contents of the vesicles, the presence or absence of palagonitisation, and the presence or absence of chlorophæite and chabazite. On these grounds the lavas are divided into seven groups referable to two magma types. Other topics discussed in this important memoir are the causes of fluidity and viscosity in lavas, the origin of plateau basalts (which are regarded as derived from an infraplutonic shell of basaltic composition but eclogitic phase); and there is a full discussion of the origin of the secondary minerals present, including the zeolites and "pala-gonite."

The Mt. Girnar "laccolith" of Kathiawar, India, consisting of olivine-gabbro, diorite, monzonite, syenite, nepheline-syenite, and granophyre, and intruded under a thick cover of Late Cretaceous or Early Eocene plateau basalts, is described by K. K. Mathur, V. S. Dubey, and N. L. Sharma (*Journ. Geol.*, 34, 1926, 289-307). Diorite and monzonite occur in the centre of the area, surrounded by olivine-gabbro. Granophyre is intruded into the adjacent basaltic hills in the form of large dykes of arcuate outcrop. The whole aspect of the mass, as revealed by the map and section, is of a ring complex of the same nature as those of Mull, Ardnamurchan, Glen Coe, and the Oslo region, although this view has not been considered by the authors.

The petrography of the rocks from the Girnar and Osham Hills, Kathiawar, has been described by M. S. Krishnan (*Rec. Geol. Surv. India*, lviii, pt. 4, 1926, 384-424). Rocks of the gabbro clan appear to be predominant, followed by alkali-syenites, especially nepheline-syenite, and by acid types such as quartz-porphry, granophyre, and rhyolite. A somewhat unconvincing silica variation diagram links these diverse rock types together as an alkaline province of the Atlantic type.

Prof. L. D. Stamp describes the interesting igneous complex, probably of late Mesozoic age, which intrudes the presumably Pre-Cambrian slates of Green Island, on the Amherst coast of Burma (*Geol. Mag.*, lxiii, 1926, 399-410). The igneous rocks show a complete series of types from ordinary granite, through contaminated granite, intrusion-gneisses, lit-par-lit aplitic intrusions, to aplites and muscovite-pegmatites. Remarkable light-coloured reaction borders, several inches wide, intervene between xenoliths of slate and the igneous rock, pointing to an exchange of material between the magma and the enclosures. All the rocks are mylonised to some extent, probably as a result of late Mesozoic folding in the Indo-Malayan orogeny.

The type locality of *dunite*, Mt. Dun, near Nelson, New Zealand, is popularly described by Dr. P. Marshall, in a lecture on the geology of Nelson (*Cawthron Lectures*, vol. 2, 1925, pp. 1-25, Nelson, N.Z.).

J. A. Bartrum has furnished a summary of the main facts concerning the distribution of igneous rocks in the North Auckland area of New Zealand, with a preliminary description of a Cainozoic series outcropping between Whangarei and Bay of Islands (*Verh. van het Geol.-Mijnb. Gen. v. Nederland en Kolonien. Geol. Ser. D.* viii, 1925, pp. 1-16). The latter is illustrated by no fewer than 32 new chemical analyses by Mr. F. T. Seelye. In order of age they include rhyolites and dacites, andesites of various types, and finally olivine-andesites and olivine-basalts, accompanied by a small intrusion of teschenite. The olivine-basalts and teschenite are treated as co-magmatic with the foregoing rocks, but in the writer's opinion this is by no means certain. The same series of rocks is dealt with in *Bull. No. 27 (N.S.) of the Geol. Surv. of New Zealand*, "The Geology of the Whangarei-Bay of Islands Subdivision," 1925, by H. T. Ferrar and others.

The eroded volcanic cone of Mayor Island, Bay of Plenty, New Zealand, with its wide caldera enlarged by downbreaks, and modified by later eruptions, has recently been described by Dr. J. A. Thomson (*N.Z. Journ. Sci. and Tech.*, 4, 1926, 210-4).

The volcanic rocks of Christmas Island, described by Dr. W. Campbell Smith (*Q.J.G.S.*, lxxxii, pt. 1, 1926, 44-66), belong to two periods of eruptions—Eocene and Miocene. The older series includes alkali-trachytes, trachybasalts, olivine-basalts with normative nepheline and limburgitic basalts, with probably also nepheline-basalt, and limburgite. In the Miocene series there is a recurrence of two types of basalt which occur in the lower series, with limburgites and palagonite-tuffs. The alkali-trachytes were the earliest products of Eocene vulcanicity, and they were followed by trachybasalts; whilst the volcanic activity ceased in the Miocene with the eruption of limburgites.

Jan Mayen, an island between Iceland and Greenland, exhibits vulcanicity of recent date, in which the lavas are almost exactly parallel to those of Christmas Island (*Trans. Roy. Soc. Edin.*, liv, pt. 3, 1926, pp. 747-65). The rocks (described by the writer) are trachyte, trachyandesite, trachybasalt, and ankaramite (ultramafic olivine-basalt). The suggested sequence of types is alkali-trachyte, trachybasalt, ankaramite. As the latter rock is the holocrystalline equivalent of limburgite, the sequence is almost exactly the same as in Christmas Island. It is suggested that the initial magma was of trachybasaltic composition, and that differentiation caused it to become stratified with the accumulation of olivine and augite crystals with remelting (*i.e.* ankaramite) at low levels, and a trachytic sub-magma at high levels. Freezing from above downwards led to the above sequence of eruption, as trachyte magma would cease to be eruptible at an early stage. Both Jan Mayen and Christmas Island provide excellent examples of a petrological kindred, the *trachybasaltic*, which is increasingly recognised as being characteristic of oceanic islands.

The chemical composition of the peculiar plutonic rock *okaite*, consisting essentially of melilite, haüyn, and biotite, and related to alnöite among the lamprophyres, is studied by J. Stansfield (*Amer. Journ. Sci.*, xi, 1926, 396-8). The importance of sodium sulphate and sodium sulphide in this rock is pointed out. The view that reaction with calcium carbonate has been responsible for the development of melilite-rich rocks is upheld. On the contrary, C. S. Ross believes that the formation of the nepheline-haüyn-alnöite of Winnett, Montana (*ibid.*, pp. 218-27), was due to a normal reaction process with interstitial liquid, reinforced by an additional supply of mineralisers and alkalis from a deep-seated source.

The rarity of the peculiar glassy alkalic lamprophyres known as *verite* invests with interest C. S. Ross's description of a new occurrence from Colorado (*Amer. Journ. Sci.*, 12, 1926,

217-29). It appears as a dyke from 10 to 20 feet in width, with a length of about 10 miles. The rock is predominantly glassy, but has also a fine-grained facies. Locally there is a coarse-grained phase which is described as an analcite-soda-syenite of lamprophyric habit.

**METEOROLOGY.** By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

IN SCIENCE PROGRESS, vol. xx, pp. 391-2, reference was made to the question as to whether we have any evidence that the intensity of the stream of radiation falling upon the outer limit of the earth's atmosphere is variable, *i.e.* whether the so-called solar "constant" is really a variable quantity. It was pointed out that the difficulty of determining the solar "constant" accurately lies not so much in the measurement of the intensity of the stream of radiation received at the bottom of the atmosphere by means of the pyrheliometer, but in the correction necessary to allow for the scattering and absorption of the radiation by the earth's atmosphere, arrived at with the aid of the bolometer.

In the *Monthly Weather Review* of May 1926 (vol. 54, No. 5) Dr. Abbot, of the Smithsonian Institution, points out that if observations are made over a number of years under closely similar conditions of atmospheric transparency and temperature, and with the same amount of precipitable water in the atmosphere, it is very unlikely that these influences will cause apparent variations in the value of the solar "constant" in individual years. The determinations of the observed intensity of the solar radiation and of the solar constant made at Mount Wilson, California, from 1910 to 1920, furnish a long series of carefully standardised measurements from which Abbot obtains groups of observations made under closely similar atmospheric conditions, the month of July alone being considered. All the observations made in July except those for the years 1912 and 1913<sup>1</sup> were divided into four groups. In the first of these the atmospheric transparency was the highest,<sup>2</sup> and the amount of precipitable water, as determined by the bolometer, was least. In the second group there was more water vapour and the air was less transparent, and so on, the fourth group consisting of cases where the amount of water was exceptionally large, or where the transparency, if falling within one of the first three groups, was accompanied by precipitable water

<sup>1</sup> These are omitted because the eruption of Mt. Katmai caused abnormally low transparency of the atmosphere on account of volcanic dust.

<sup>2</sup> The transparency was measured by the intensity of radiation recorded by the bolometer corrected for the mass of air traversed by the sun's rays.

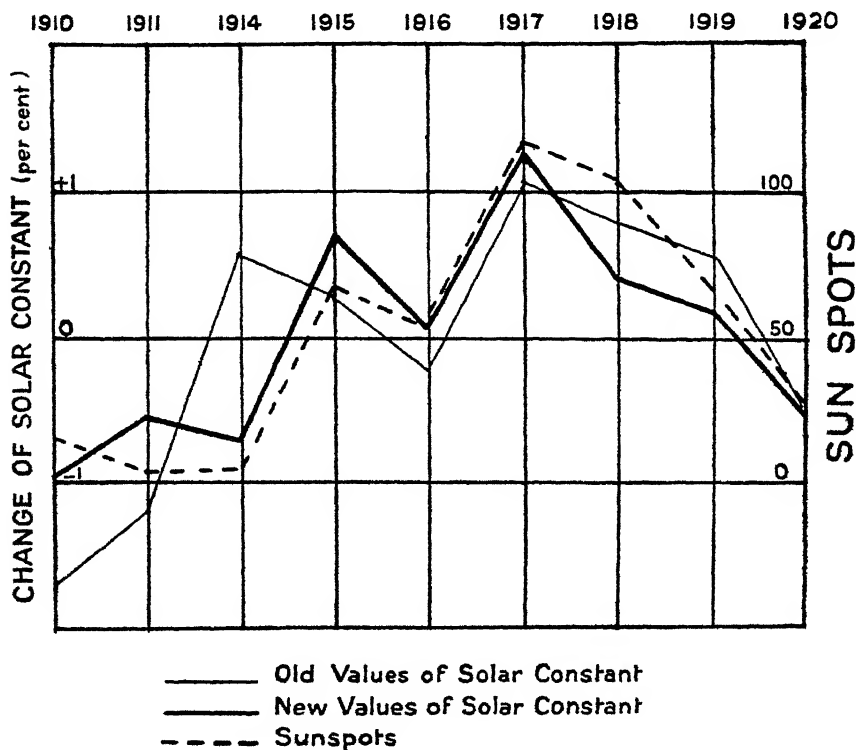
which did not fall in with this group. This fourth group was rejected altogether. The next stage was to determine for each case, in the first three groups, the radiation that would have been observed had the mass of air traversed been equal to a certain standard mass, and also to determine the solar constant, in the normal manner, by the bolometer. A mean value of these two quantities was obtained for each group, and the percentage departures of the means for individual Julys from the group means were calculated. It was observed that the radiation measured by the pyrliometer, as well as the solar "constant," was nearly always high or low in all three groups in a particular July, and further, that the group means in the first two groups were in excellent agreement. In the third group, however, the radiation and solar "constant" both averaged a little higher. These facts suggest that variations in the solar constant occur which cannot be attributed to imperfections in the methods of correcting for absorption and scattering, or to radiation received from the sky immediately surrounding the sun, but that determinations made on days of excessive moisture and haziness were very slightly too high, probably on account of radiation from the sky. Finally, single mean percentage departures for each July were obtained, and these were compared with the number of sunspots for each month. The figures obtained were as follows :

Year.	Percentage Departures		Sunspot numbers
	Radiation observed for standard mass of air traversed	Solar "Constant."	
1910 . . . .	- 0.95	- 1.70	14
1911 . . . .	- 0.60	- 1.17	3
1914 . . . .	- 0.72	+ 0.60	5
1915 . . . .	+ 0.69	+ 0.30	71
1916 . . . .	+ 0.02	- 0.22	53
1917 . . . .	+ 1.26	+ 1.11	117
1918 . . . .	+ 0.44	+ 0.73	105
1919 . . . .	+ 0.13	+ 0.55	64
1920 . . . .	- 0.52	- 0.49	26

These figures are perhaps the strongest evidence yet obtained that the sun radiates more heat at times of numerous sunspots. In the accompanying illustration the values of the solar constant just given are plotted together with the values that had been obtained previously without rejecting days of abnormal moisture or transparency, and the number of sunspots are also shown.

The month of August was then treated in the same way, with similar results ; in the illustration it is seen that in 1914

there is a decided discrepancy between the old and new determinations, and this appeared also in the similar figures that were obtained for August. This result has not yet been fully investigated. It should be mentioned that not only did the means for the observed radiation and the solar "constant" vary together for individual months, but the values for individual days were usually high or low at the same time, from which genuine short period variations in the solar "constant" are deduced. The new method of dealing statistically with



pyrheliometrical observations is shortly to be applied to those for Harqua Hala (Arizona) and Montezuma (Chile), and publication of the results of this further inquiry will doubtless be carried out in due course.

*On the Relation between Barometric Pressure and Gas Pressure in Mines* (Henry Harries, *Monthly Notices, Royal Astronomical Society, Geophysical Supplement*, vol. 1, No. 7).

Mr. Harries first turned his attention to this subject over fifty years ago, when the idea that fire-damp causes explosions in mines when the barometer is low and falling rapidly was

generally accepted.<sup>1</sup> He examined the weather charts of the Meteorological Office for 1870-80 on all occasions of serious explosions, and found that these nearly always occurred within areas of high barometer. This result led him to carry the investigation further, and he was able in 1890 to secure two sets of observations made with water-gauges recording the pressure of gas imprisoned in hermetically sealed underground chambers in two mines 150 miles apart. These chambers were in different geological strata.

In one pit, which we may call Pit A, the gauge pierced the air-tight stopping of a gas charged "goaf," or waste area, at a depth of 1,500 ft. below the pit's mouth and 1,300 ft. below mean sea-level. In the other pit, which we may call Pit B,<sup>2</sup> the gauge was inserted in an air-tight stopping 711 ft. below the pit's mouth, and 386 ft. below mean sea-level. Barometric curves were available for both pits. On plotting graphs showing the pressure of the imprisoned air in these two distant cavities, a remarkably close agreement was observed, although their depths differed by over 900 ft. This agreement suggested that the gauges were actuated by a natural force dominating an extensive area at one time, so that the movements of the gas within the earth are general and not irregular local manifestations. A close agreement was also noted between the characteristics of the curves obtained from hourly readings of the pressure-gauge and barometer in Pit A, and those found by Corbett in another mine seven years earlier.<sup>3</sup> In each case there was a close association between the barometric and gauge fluctuations, the gauges being affected even by small barometric changes. In the absence of information about the behaviour of free gas in open spaces at Pit A and Pit B, Mr. Harries had recourse to Corbett's discussion of this subject, and I will here repeat those portions of Mr. Harries' account of this discussion, and of his own further development of the theory, which are essential to a proper understanding of his final solution of the problem.<sup>4</sup>

At the Maudlin stoppings in Seaham Colliery there were under observation two barometers, three thermometers, and

<sup>1</sup> Scott and Galloway, *Proc. Royal Soc.*, 20, 292-305.

<sup>2</sup> This pit was the Reservoir Pit of the Moura Colliery Co., Ashby-de-la-Zouch.

<sup>3</sup> On Water-gauge, Barometer, and other Observations taken at Seaham Colliery during the time the Maudlin Seam was sealed up. *Proc. North of England Inst. of Mining and Mechanical Engineers*, 32, 1883.

<sup>4</sup> This solution was sufficiently complete to enable him for many years to issue warnings to collieries of the occurrence of atmospheric conditions likely to lead to explosions unless precautions be taken, with a degree of success which the Home Office statistics relating to the loss of life through explosions in collieries proved to be considerable.

four water-gauges, and about a mile distant, in the Hutton Seam, 170 ft. deeper, a worked-out area in which gas accumulated was set apart for ascertaining when the gas escaped, along a gallery about 50 yards long, from the goaf to the return air course. Every hour the observer entered the gallery and noted the distance from the waste at which he first observed gas in his testing lamp.

Corbett selected for his discussion three outstanding instances, and made four comparisons covering each instance. The third comparison was between No. 3 water-gauge in the return air course and the gas-check in the gallery; and the fourth comparison between No. 3 barometer and the gas-check.

The third comparison revealed a surprising coincidence. When the water column was ascending (air pressure exceeding that of the gas), its arrival at  $-0.6$  inch denoted the disappearance of the gas from the distant gallery; and when, after it had reached its highest point in the tube, the water, under the influence of increasing gas pressure, sank to  $-0.6$  inch, the gas from the open goaf would reappear in the gallery, and remain there or pass into the open workings so long as the water in the tube remained below  $-0.6$  inch. Out of eighteen instances fifteen gave the critical point  $-0.6$  inch, the other three  $-0.5$  inch, a negligible difference, probably due to variation in the observing interval. This comparison renders it obvious that the force which produces the increasing pressure on the imprisoned gas in one seam is exerting its influence at the same moment upon the contents of the open goaf in another and distant seam. The fourth comparison resulted in the discovery that gas was pouring out of the goaf into the gallery when the barometer was still rising, the mercury not beginning to sink until from 6 to 28 hours *after* the commencement of the gas escapes. Our information thus shows perfect correlation between

(1) The behaviour of distantly situated volumes of gas in the earth's crust.

(2) A *rising* barometer and *increasing* pressure of gas in a sealed-up chamber.

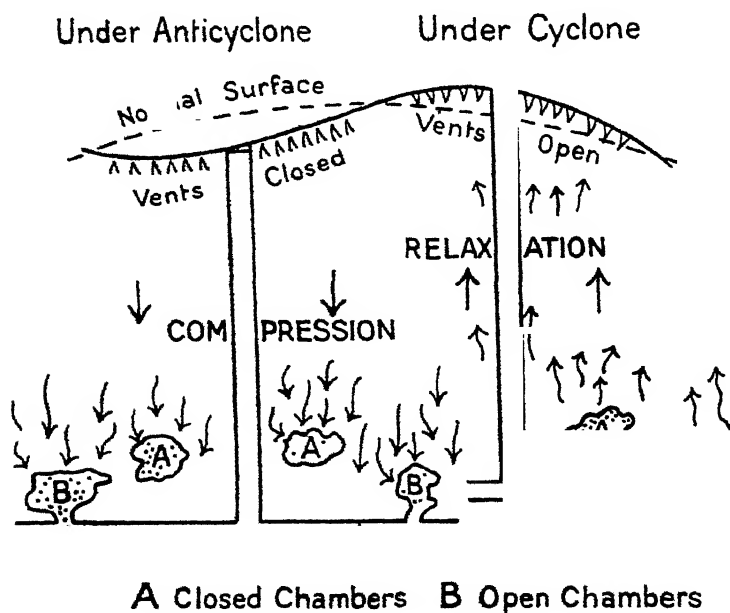
(3) The gas pressure in a sealed-up chamber and the escape of gas in an open chamber.

(4) A *rising* barometer and the *escape of gas*.<sup>1</sup>

Reviewing the evidence, we see that the simultaneous fluctuations of the two water-gauges show that volumes of gas in *closed chambers* at great depths below ground, and over

<sup>1</sup> The Seaham records and reports received from other mines show that frequently the gas disappears when the barometer is very low.

wide areas, vary simultaneously in density ; definite phases of these variations are associated with certain well-defined stages in the march of the barometer, *i.e.* in the weight of the atmosphere on the earth's surface. The Seaham observations carry us another step forward, proving beyond question that gas accumulating in open chambers underground begins to invade the neighbouring air-ways when the barometer is *rising and attains a high level*, an apparent contradiction of the well-known law that gas expands in sympathy with decreasing pressure and contracts with increasing pressure. But when No. 3 gauge showed a continued increase of pressure on im-



prisoned gas in one locality, and it reached a certain point, — 0.6 inch on the scale, it proved to be a correct indication that the open goaf in a distant district was being over-charged with gas. It was the *excess of pressure in the interior* that was forcing it into the testing gallery, *against the increasing pressure of the air outside*, and gas produced in these circumstances escapes in far greater volumes than are possible under our very gradual falls of the barometer.

How does this pressure originate? The air is not in contact with the gas in faults, pockets, and other cavities in the strata, yet it is obvious that the atmospheric changes play a very important part in bringing about conditions which force the gas in different directions. The accompanying figure

represents the directions in which the gas streams flow within the earth's crust under the two types of atmospheric pressure distribution—anticyclonic and cyclonic. As the result of the compression of the strata under high-pressure conditions, the earth's surface assumes a slightly concave formation, so that the vents or stomata of the outer skin close, and the cracks, cleats, cleavages, etc., in the strata widen, of course, very slightly, but sufficiently to enable the gas streams to follow a general downward course along all available channels, invading open mine workings as well as over-charging closed caverns and chambers. As the high pressure passes away, the weight of the superincumbent atmosphere diminishes, the barometer falls, and the earth's surface swells and becomes slightly convex above the normal level; the vents reopen, and the gas streams reversing their direction mount upward, drawn towards the region of least pressure. During this low-pressure stage there have been notable instances of the complete cessation of gas escaping into the mine galleries.

Weather maps are now published daily in most civilised countries, and in their construction the readings of the barometer are utilised to represent, by means of isobars, or lines of equal pressure, the distribution of air weight, and from these it is a comparatively simple problem to determine the weight of the atmosphere over any specified area.

Records covering many decades show that the British Isles annually experience a barometric range exceeding two inches, the greatest range having occurred in the year 1886, when 31.1 inches was reached in January and 27½ inches in December. During very disturbed conditions there is frequently, sometimes in less than a day, a range of more than an inch when a cyclone replaces an anticyclone, or the latter the former.

An inch of mercury is equivalent to 13½ inches of water, which is comparable with the height of the tide in mid-sea. Deformations of the crust due to ordinary fluctuations of barometric pressure may therefore be comparable locally with the tidal deformation.

Altogether our considerations justify the conclusion that under the immense changes of weight indicated by the barometric variations, the earth's crust does undergo a wave-like variation of level, and it is the *depression of the level under high atmospheric pressure* which *compresses* magazines of gas, and, as we have seen, drives the gas out into mine workings against the pressure of the free air. That there is visible movement of the strata is well known to miners.

Mr. Harries concludes with an account of experiences of miners and others which support his theory, one of which may be quoted.

Baldwin Latham, in his paper on the "Influence of Barometric Pressure on the Discharge of Waters from Springs," read at the British Association Meeting, 1881, states that :

"He had set up gauges in the Bourne flow, near Croydon, and selecting periods when there was no rain to vitiate the results, he found that whenever there was a rapid fall in the barometer there was a corresponding increase in the volume of water flowing, and with a rise of the barometer there was a diminution in the flow. The gaugings of deep wells also confirmed these observations; for when there was a large amount of water held by capillarity in the strata above the water line, at that period of the year when the wells became sensitive and the flow from the strata was sluggish, that a fall in the barometer coincided with a rise in the water-line, and that under conditions of high barometric pressure the water-line was lowered. Percolating gauges also gave similar evidence. . . ."

Widespread belief in the dogma that gas escapes in mines with low atmospheric pressure has in the past made it very difficult for Mr. Harries to secure acceptance of his discovery, the more so in that he was unable to disclose all the information which led to this discovery. His results now appear to be generally accepted by those most competent to judge as to their correctness, as is shown, for instance, by some opinions quoted by him in an explanatory paper recently published (Edinburgh 1926) entitled "Colliery Warnings, 1881-1918."

*On the Solution of Problems of Atmospheric Motion by means of Model Experiments.* A paper by C. G. Rossby with the above title appears in the *Monthly Weather Review* for June 1926 (vol. 54, No. 6).

The author points out that for the solution of most problems in Aerodynamics experimental methods have had to be used for many years, because the fundamental equations of classical hydrodynamics present too great difficulties when integration has to be performed. In this paper the application of Bairstow's conception of "dynamic similarity"<sup>1</sup> to the experimental method as applied to meteorology is attempted. Thus, let us suppose that the movements of the atmosphere in a space A are required, the dimensions of this space being unsuitable for the direct application of experimental methods, and that another space A<sup>1</sup> can be found which has suitable dimensions. We require to know what these dimensions must be in order to secure dynamical similarity between A and A<sup>1</sup>, that is to say, that, corresponding to any of the instantaneous

<sup>1</sup> L. Bairstow, *Applied Aerodynamics*, New York, 1920.

configurations of stream-lines and isobaric surfaces in  $A^1$ , there shall be similar configurations in  $A$ .

The sequence of corresponding states of motion generally runs at different rates in the two spaces ; let the ratio between corresponding times in  $A^1$  and  $A$  be called  $T$ . The equations of motion in the two spaces are shown to lead to the following necessary condition for dynamic similarity :

$$\frac{V^1 D^1}{\nu^1} = \frac{V D}{\nu} \dots \dots (1)$$

where  $V^1$  represents a certain characteristic velocity in space  $A^1$ ,  $V$  the corresponding velocity in  $A$ ,  $D$  and  $D^1$  represent characteristic lengths, and  $\nu^1$  and  $\nu$  are the kinematic coefficients of viscosity,  $\left(\frac{\mu^1}{\rho^1} \text{ and } \frac{\mu}{\rho} \text{ respectively}\right)$ .

Equation (1) shows that a condition for dynamical similarity is that the two systems shall have the same " Reynold's number." It follows that if we know the physical properties of the two fluids, the time scale can be fixed if the ratio  $L$  of the linear dimensions of  $A^1$  to those of  $A$  is known.

If now, in addition to considering internal forces (pressure-gradient and friction) we take into account the force of gravity, which comes into atmospheric motions, and examine the equations for motion round a vertical axis, a new condition for dynamical similarity is arrived at, namely, that

$$L^3 = T^2 \dots \dots (2)$$

$$\text{or } \frac{U_1^3}{D} = \frac{U^3}{D}$$

If, further, the deviating force of the earth's rotation is also taken into account, this is shown to introduce as a further condition that  $\Omega^1 = \frac{1}{T} \Omega$ , but this does not add any important new restriction in the choice of scales and fluids, since  $\Omega$  can be made experimentally to vary over a wide range.

Unfortunately, in the application of this method to meteorological problems a more serious difficulty arises, which has not yet been considered. Atmospheric movements are generally accompanied by thermal changes, and the conception of dynamical similarity clearly cannot be extended to these. Even in the case of purely adiabatic movements the compressibility of the atmosphere introduces a new condition necessary for dynamical similarity. If, however, we confine ourselves to the larger atmospheric movements, in which the motion is

practically horizontal, then compressibility can be neglected. In this case the conditions (1) and (2) alone have to be satisfied and the construction of model experiments for the study of atmospheric circulation becomes theoretically possible, although the choice of the dimensions of the experiment and of the nature of the fluid to be used is very limited even by the conditions (1) and (2) alone. Fortunately a further simplifying assumption can be made in the case of atmospheric movements on a very large scale. Condition (1) shows that if we magnify the linear dimensions of a system, the Reynold's number will change in the same way as if we had kept the dimensions constant, but had diminished the coefficient of viscosity. For such large-scale movements, therefore, the atmosphere can be regarded as a non-viscous and incompressible fluid. There remains, however, a difficulty of a practical kind which appears to be impossible to overcome so as to allow the small scale experiment to be carried out quite satisfactorily—namely, that we cannot use in the small scale experiment a fluid of such low viscosity that the behaviour of the fluids in  $A^1$  and  $A$  can be compared quantitatively. Also in atmospheric movements on a large scale the horizontal dimensions that have normally to be considered are far larger than the vertical dimensions, owing to the small height of the atmosphere compared with the extent of the Earth's surface, and if liquids of different densities are used to imitate air masses of different temperature, the movements of these liquid layers will be much influenced by surface tensions and irregularities at the bottom of the model vessel. If it is assumed that vertical velocities and accelerations can be neglected in the dynamical equations—and they are certainly of secondary importance in most large-scale atmospheric movements—this difficulty disappears, and the condition for dynamical similarity becomes

$$l = \frac{L^2}{T^2} \dots \dots (3)$$

Denoting two characteristic horizontal and vertical distances in  $A^1$  by  $D^1$  and  $h^1$ , corresponding to  $D$  and  $h$  in  $A$ , and a characteristic velocity by  $V^1$ , corresponding to  $V$ , we obtain

$$\frac{\frac{h}{D}}{\frac{h^1}{D^1}} = \frac{\frac{dV}{dt}}{\frac{dV^1}{dt^1}}$$

which denotes that the horizontal accelerations of the model movements in  $A$  will be magnified at the same rate as the vertical dimensions of the model are exaggerated in  $A^1$ .

The author next considers some applications of (3). Where the deviating force of the Earth's rotation must be taken into account (3) becomes

$$l = L^2 \dots \dots (4)$$

which may be interpreted as follows :

" If we take any complete atmospheric system, a cyclone surrounded by homogeneous air at rest, for instance, and magnify the horizontal dimensions  $L$  times, the vertical dimensions  $l$  times, then the original and the new system are dynamically similar, provided  $l = L^2$ . In two dynamically similar atmospheric systems the ratio between corresponding horizontal velocities is equal to the square root of the ratio between corresponding vertical dimensions."

The concluding part of the paper shows how the construction of a model vessel may be carried out.

#### **PREHISTORIC ARCHÆOLOGY.** By J. REID MOIR, F.R.A.I.

A NOTABLE discovery has been made recently by Miss D. A. E. Garrod at Gibraltar. During the latter part of 1925, and the early months of the present year, she has carried out excavations in a rock shelter there, and has found a quantity of quartzite implements of Mousterian type associated with the bones of deer, goat, boar, rabbit, horse, and ox. These relics occurred in no less than five superimposed strata lying upon a raised beach upon the surface of which was discovered the carpal bone of an elephant. In the fourth stratum, composed of a very hard travertine—containing large blocks of limestone, and requiring the use of dynamite to dislodge it—were discovered the frontal and parietal bones of an immature human skull of Neanderthal type. As will be remembered, there was discovered, in 1848, during blasting operations carried out at Forbes Quarry, situated about 150 yards east of the site of Miss Garrod's excavations, part of the skull, and face bones, of a very primitive Neanderthal woman which is now preserved in the Museum of the Royal College of Surgeons in London.

From 1863 to 1868 Captain Broom conducted explorations in some of the Gibraltar caves, and recovered a number of bones of animals, of Pleistocene age, together with a series of humanly flaked flints, and quartz pebbles. Later, in 1910, Dr. W. L. H. Duckworth examined the site of Forbes Quarry and found there a quantity of Mousterian implements, but in neither of these excavations were any definite remains of Neanderthal man discovered. The site where Miss Garrod is working is opposite a now abandoned signal-station known as the Devil's Tower—on the north side of the Rock, and was

first noticed by Prof. Henri Breuil, who found there traces of the Mousterian culture. It is evident that a spring of water present at this post was the reason why these ancient people selected it as a camping-ground, but, in view of its northerly aspect, it is supposed that they occupied it only in the summer.

With the animal bones mentioned were discovered a large number of remains of edible shell-fish, together with fragments of the carapace of tortoise. The human skull fragments unearthed must evidently be referred to a child. While the marked bony ridge over the eyes, such as is so prominent a feature in the skulls of adults of the Neanderthal race, is not present, yet the head form of this prehistoric infant differed very widely from those of children of modern man. This difference is chiefly in the extreme flattening of the whole of the skull, and in the form of the frontal bone. The recently discovered remains are being studied by Mr. L. H. Dudley Buxton, who, in company with Miss Garrod, will no doubt soon publish a detailed account of them and the associated relics. Fortunately there have already been found other examples of young individuals of the Neanderthal type, so no doubt can arise as to the race to which Miss Garrod's find is to be referred. The stone implements found at the Devil's Tower are of definite Mousterian type, and so, once more, evidence is forthcoming of the association of these specimens with the bones of Neanderthal man. It would seem that the Gibraltar child was not given a ceremonial burial, such as was afforded to the individual of La Chapelle aux Saints in France, a fact of interest and some importance. Miss Garrod is continuing her excavations during this winter, and it is to be hoped that she may make further discoveries of equal value to archæologists.

**PLANT PHYSIOLOGY.** By R. C. KNIGHT, D.Sc., Imperial College of Science and Technology and East Malling Research Station (Plant Physiology Committee).

*Potassium.*—It is impossible, within the limits of such a résumé as the present, to deal even cursorily with the volume of research which is being reported concerning the absorption and function of all the elements entering a plant through its roots. The work on nitrogen alone is sufficiently extensive to be treated separately, but recently there have appeared some papers which discuss special aspects of the function of some mineral nutrients, and on account of the suggestive nature of the conclusions this work is worthy of special attention. The function of potassium has been investigated in relation to

several types of plants, and Wallace has found ("Pot Experiments on the Manuring of Fruit Trees," *Long Ashton Res. Sta. Ann. Rep.*, 1921, 42-57; 1922, 11-26; 1923, 43-57; 1924, 12-24) that the effect of potash on the foliage of several varieties of fruit plants is very marked. If the nutrient solution supplied is poor in potash, the leaves very early in the season tend to develop the brown dry patches associated with "leaf-scorch" which causes premature leaf-fall. The interesting fact is revealed that the important factor influencing the "scorching" is the ratio of potash to nitrogen. High potash is not a preventive if nitrogen is also available in large quantities.

Mann and Wallace ("The Effects of Leaching with Cold Water on the Foliage of the Apple," *Journ. Pom. and Hort. Sci.*, 1925, 4, 146-161) have extended the study of the function of potash in the foliage by showing that leaching with water will remove a large proportion of the potash normally present in leaves. Some varieties lose potash in this manner much more easily than others, and some correlation was exhibited between the ease with which potash could thus be removed and the susceptibility of the variety under natural conditions to defoliation, following the development and progressive enlargement of brown spots on the leaves. Incidentally, this trouble is more marked in wet than in dry seasons. Analyses of leaves in different conditions in nature showed that whilst the ash of healthy leaves of the variety of Cox's Orange Pippin contained 36.5 per cent.  $K_2O$ , the ash of discoloured leaves contained only 22 per cent., even before breakdown of the tissues occurred. The authors consider that this loss of potash took place by leaching and not by the removal to other parts of the plant.

Mann ("The Physiology of the Nutrition of Fruit Trees. I. Some Effects of Potassium and Calcium Starvation," *Long Ashton Res. Sta. Ann. Rep.*, 1924, 30-45) also found that potash deficiency resulted in the development of leaf-scorch and a decrease in the size of the leaves. He further investigated some other physiological aspects of his plants, and found that gooseberry-leaves deficient in potash had a lower water content and were less resistant to water-loss than normal leaves. These two phenomena may, of course, be interrelated. Apple leaves deficient in potash transpired less than normal leaves in dull light, but their response to bright sunlight was much greater than that of the normal leaves, and thus the transpiration rate under these conditions was above normal. In the course of the same investigation it was shown that calcium starvation of gooseberry-leaves resulted in an effect exactly opposite to that produced by lack of potash, both in leaf-water content and resistance to desiccation.

A further example of the influence on foliage of the balance of mineral substances is afforded by the study of chlorosis. Wallace and Mann ("Investigations on Chlorosis of Fruit Trees," *Journ. Pom. and Hort. Sci.*, 1926, 5, 115-23) found on analysis of chlorotic and normal leaves that in chlorotic leaves the calcium content is lower and the potassium (especially) and the sodium contents are higher than in normal leaves, even when the two types of leaf are taken from the same tree. Both calcium and potassium contents frequently differed in the two types of leaves by 100 per cent. The plants examined were from soils containing a high proportion of carbonates. No explanation of the phenomena is yet offered, but it was found that chlorosis on such soils could be checked by the substitution of sod for cultivation. Wallace ("An Experiment on the Winter Killing of Vegetable Crops in Market Gardens," *Journ. Pom. and Hort. Sci.*, 1926, 5, 205-9) found that the susceptibility to injury by cold exhibited by such plants as lettuce, beans, and onions, could be checked by an increase in the potash content of the manure supplied.

The work on citrus-trees in America must be read in conjunction with the results obtained with fruit-trees in this country. Reed and Haas ("Studies on the Effects of Sodium, Potassium, and Calcium on Young Orange Trees," *Univ. of Calif. Agr. Exp. Sta. Tech. Paper* 11, 1923, 1-32) have emphasised the fact that different species may react very differently to the same nutrient solution. A change of concentration or composition of the latter is not by any means necessarily reflected as even a comparable change in the composition of the plant. For example, a change of calcium content in the nutrient solution supplied may result in a change of the sodium content of the plant. In the case of orange-trees growth is very poor when the sodium in the nutrient is increased at the expense of calcium, and there follow a mottling of the leaves and early defoliation. Orange-trees normally have a very high percentage of potassium in their leaves, and if no potassium is supplied to the roots, the plants may function fairly well for as long as seventeen months, presumably using the potassium already present in the tissues. Potassium starvation was observed by Reed and Haas to be accompanied by the bronzing of leaves, and it may be suggested that the mechanism may partake of the nature of that causing spotting in the experiments of Wallace and Mann. Potassium starvation results in an increase in the absorption of calcium, whilst calcium starvation is accompanied by accumulation of potassium.

In a further communication (*Univ. of Calif. Agr. Exp. Sta. Tech. Paper* 17, 1924, 1-75) Reed and Haas record the fact that solutions deficient in calcium and containing sodium

bicarbonate will not support growth of orange-trees. Top growth is stunted and the roots eventually die, whilst the leaves curl, sometimes become chlorotic, and fall prematurely. Excess potassium has a similar effect, but it becomes evident much more slowly. Throughout these experiments it was found that there was a general tendency for calcium and potassium to be antagonistic ; a high percentage of one of these in the ash of the leaves was accompanied by a low percentage of the other.

Some light may be thrown on the function of potassium by a study of its distribution in plants. Dowding (" Regional and Seasonal Distribution of Potassium in Plant Tissues," *Ann. Bot.*, 1925, **39**, 459-74) found an accumulation in primary meristems and in regions where active secondary growth occurred, whilst Priestley and Tupper Carey (" Physiological Studies in Plant Anatomy," IV. *New Phytol.* 1922, **21**, 210-29) found that the distribution of potassium in willow roots " showed a sharp restriction to the vacuolated region behind the meristematic apex." Reed and Haas found that plants grown with a nutrient solution containing 0.05 per cent. potassium might show over 40 per cent. potassium in the ash of their leaves, whilst in the potash-starved plants the ash of the leaves contained less than 2 per cent potassium. The corresponding figures for roots were 20 per cent. and 6 per cent., and, in fact, roots and rootlets were the last organs to lose their potash when none was supplied to the culture.

Smith and Butler (" Relation of Potassium to Growth in Plants," *Ann. Bot.*, 1921, **35**, 189-225) found that potassium was required by the plant more particularly in the seedling stage. Working with wheat and maize, they showed that if the plants are starved of potassium its distribution in the different organs is relatively the same as when the plants contain the normal quantity. In this respect wheat and maize behave differently from the orange-trees of Reed and Haas. The difference may, perhaps, be regarded as resulting from the different habit and form of the plant. It is natural that a large woody perennial should be slower in changing its internal equilibria than the quick-growing annuals. In the work of Smith and Butler the weight of dry matter formed per unit weight of potassium absorbed was markedly higher when the potassium available in the nutrient solution was limited. Thus the plants appear to absorb, under normal conditions, more potassium than is required, and in this respect behave similarly to orange-trees which store in their leaves potassium in excess of their requirements.

*Aluminium.*—The constant association of acidity in soils with the presence of soluble aluminium compounds has led

to considerable work upon the interaction of these two factors and their respective influence upon growth. Hardy ("The Rôle of Aluminium in Soil Infertility and Toxicity," *Journ. Agr. Sci.*, 1926, **16**, 616-31) has reviewed the progress of our knowledge of the function of aluminium in the soil and plant, and has shown that it is now possible to construct a fairly comprehensive and coherent picture of the part played by this substance. Hardy deals at length with the work of Magistad ("The Aluminium Content of the Soil Solution and its Relation to Plant Reaction and to Plant Growth," *Soil Sci.*, 1925, **20**, 181), who investigated the physico-chemical and physiological aspects of the subject.

Magistad, using aluminium sulphate, demonstrated that soluble aluminium exists in only very minute quantities in solutions at reactions between  $pH$  4.7 and  $pH$  7.8; but Hardy points out that this statement can hardly be regarded as of general application, since the nature and concentration of other ions in solution are very likely to influence the range of insolubility. Indeed, Line has shown ("Aluminium and Acid Soils," *Journ. Agr. Sci.*, 1926, **16**, 335-64) that whereas the precipitation of aluminium as hydroxide from a solution of sulphate begins at about  $pH$  4.0, precipitation as phosphate begins in more acid conditions. However, there is obviously a considerable reaction range within which soluble aluminium exists only in very small quantities—0.006 per cent. in Line's experiments. Magistad further examined soil extracts and found that here also soluble aluminium did not occur within a wide reaction range, practically identical with that for simple solutions. Within the range indicated aluminium exists in colloidal dispersion and not in true solution, and from these facts Magistad concluded that aluminium should not exert toxic effects on plants unless supplied in media with reactions outside the range  $pH$  4.7  $p = H$  8.5. Line reached much the same conclusion. Both these workers also point out that in many cases the ill effects upon plants in water culture which have been attributed to aluminium are probably nothing more than phosphate starvation following on the precipitation of aluminium phosphate.

Magistad found by sand culture experiments that the toxicity of aluminium was specific, lucerne and clover being unaffected by aluminium at  $pH$  5.0, whilst barley and maize were distinctly injured. At reactions more nearly neutral than  $pH$  5.0 aluminium had no effect, whilst in more acid media acidity as well as aluminium exerted an influence. Line is of the opinion that barley is not influenced by aluminium, but that reduction of growth occurs as the result of the acidity of the soil which is a condition necessary to the solution

aluminium. Magistad thinks that highly alkaline soils may produce aluminium toxicity as a result of the presence of aluminate ions.

The close relationship existing between acidity and the presence of soluble aluminium, and the effect upon both of the addition of lime or phosphates, has naturally led to some confusion regarding their respective toxicities; but it appears possible now to distinguish these.

The part played by aluminium in the plant is still obscure, though its toxicity often takes the form of predisposing the plant to diseases such as root rots. It appears that aluminium may be absorbed by plants in the form of complex metallic anions or as organo-compounds and it is also translocated in these forms. In this respect it differs from potassium, which is considered to be absorbed and translocated in very simple forms. Probably only the simpler aluminous ions which occur in strongly acid or alkaline soils exert any toxic effect, and the presence of such ions within the plant is naturally influenced by the reaction conditions.

The work which has been reviewed exhibits one general feature which is worthy of emphasis. Stress is laid not merely upon the quantities of nutrient substances which are present in the make-up of a plant, but more especially upon the *relative* quantities of a variety of substances. It is obvious, of course, that the actual quantity of any constituent must have its influence upon growth, but it is necessary to visualise the plant as a complex whole, the component parts of which are closely interrelated so that a change in one component may disturb the equilibria throughout the organism resulting in fundamental changes in growth forms. Research on plant nutrition is now being directed towards problems of balance and correlation instead of being content with actual quantitative measures.

**AGRICULTURE: ANIMAL NUTRITION.** By HERBERT ERNEST WOODMAN, Ph.D., D.Sc., School of Agriculture, Cambridge.

*The Nutritive Value of Pasture.*—The present decade has witnessed a significant revival of interest in one of the fundamental problems of agriculture, namely, the nutrition of grazing animals. Important contributions to our knowledge of this subject have been made as a result of investigations which have been carried out at different centres for agricultural research throughout Great Britain, notably at Aberystwyth, Aberdeen, Cambridge, and Leeds.

The early work on the nutritive value of grass was for the most part carried out under meadow, and not pasture,

conditions. In other words, the herbage was allowed to grow unchecked to the stage of maturity suitable for hay, at which stage it was cut, weighed, and submitted to chemical analysis. It was pointed out many years ago (W. Somerville, *Journ. Bd. of Agric.*, Supp. vol. xvii, No. 10, 1911) that such tests did not yield results which could be applied to pastures, where grazing by animals encourages the continuous production of new growth of herbage, instead of permitting grasses to attain maturity, and where, moreover, the free growth of the finer grasses, clover and other dwarf plants, is possible. Somerville therefore attacked the problem of securing quantitative comparisons of the nutritive properties of different grasslands by ascertaining the live weight increases which could be produced in sheep grazing such pastures and was thereby enabled to draw important conclusions as to the effect of different systems of manurial treatment on the nutritive value of the herbage.

Mention should also be made of the pioneer work of S. F. Armstrong (*Journ. Agric. Sci.*, ii, 283, 1907), who made a series of interesting observations in respect of the botanical and chemical composition of the herbage of pastures, and concluded that the bulk of the herbage on the best grazing lands, both old and recent in the English Midlands, consisted of white clover and rye grass, whereas on the inferior grasslands in the same district, bent grass constituted the main form of herbage. Investigations into the causes of the high nutritive value and fertility of the fattening pastures of the Romney Marsh and other pastures in the S.E. of England (A. D. Hall and E. J. Russell, *Journ. Agric. Sci.*, iv, 339, 1912) led to the recognition of two main independent factors which governed the value of pasture land for nutritional purposes, viz. floral type and habit of growth. In more recent work (F. Honcamp, B. Stau, and H. Müllner, *Versuchs-Stat*, lxxxvii, 315, 1915), on the composition and digestibility of some of the important meadow grasses, it was demonstrated that good grasses, when grown for hay under similar conditions, possess approximately equal feeding values.

An account of the most recent work in this domain of nutritional science is given below:

*Pasture Investigations at Aberystwyth.*—A notable contribution to the subject was made by the publication, during the year 1924, of a collection of five papers from the Welsh Plant Breeding Station (*Aberystwyth Bulletin*, Series H., No. 3, 1920–23). It had been recognised by Prof. Stapledon and his co-workers that the detailed study of the pasture attributes of the individual herbage grasses was of fundamental importance in the elucidation of the grassland problem, and

the publication cited above embodies the results of an extensive inquiry into the persistency, yielding capacity, and chemical composition of some of the important grass species when grown in pure culture and subjected to the influence of different systems of cutting.

In the first article (R. G. Stapledon), the results obtained over a four-year period (1920-23) in respect of yield, and the influences affecting yield, in relation to different species, nationalities and strains of grasses are summarised and discussed. These yield data are supplemented in the second article (T. W. Fagan and H. Trefor Jones) by chemical evidence based largely on seasonal cuts. In the third article (R. D. Williams), Red Clover is dealt with on lines similar to the grasses, and this is followed by a short note on Subterranean Clover (R. D. Williams and W. Davies). In the concluding article, the evidence brought forward in the preceding papers is discussed in relation to the grazing animal (R. G. Stapledon, T. W. Fagan, and R. D. Williams).

Considerations of space prevent the writer from dealing with the results of these investigations in as great a detail as their importance merits. The following passages, however, taken from the concluding article, serve to indicate the significant bearing of the conclusions on the practice of the grazier.

"It has been shown, firstly, that frequent cutting makes for a leafy herbage, and secondly, that leaf at practically all stages through the grazing season has a higher nutritive value than stem. Thus heavy grazing, which undoubtedly makes for a leafy sward at any particular time, *ipso facto* makes for a nutritious sward. It has been shown also that the nutritive value of both stem and leaf frequently cut is greater than that less frequently cut, and further that the percentage dry matter is less in frequently cut than in infrequently cut herbage. It is thus apparent that the ration afforded at any particular time on any particular field is a variable of at least four dimensions : (1) The botanical composition of the sward ; (2) the ratio leaf to stem ; (3) the nutritive value of both leaf and stem ; (4) the moisture content of leaf and stem ; and, further, that every one of these characteristics of the ration is influenced by systems of cutting, and therefore, although perhaps to a less marked extent, must inevitably be influenced by the grazing animal itself, and therefore by the system of control adopted by the farmer."

"It has been pointed out that excessive grazing early in the spring almost certainly reacts adversely on subsequent productivity, and on the persistency of the more bulky grasses. The chemical evidence, however, suggests that the nutritive value of the herbage on a field at any particular time varies

more or less inversely with the maturity of that herbage. Thus the grazier concerned with fattening beasts on pasture has an interesting and difficult problem to solve. If he starts grazing too soon, he risks an almost immediate shortage of herbage, when the animals will take too long to collect their ration, and he also risks doing lasting harm to his field. If, on the other hand, he starts too late, he risks the herbage keeping too far ahead of his animals all through the season; it will always be a little too mature and not at maximum nutritive value. The obvious compromise would appear to be to start grazing rather lightly and some little time before heading stage, and increase the head of stock during heading and flowering stages, every endeavour then being made to prevent the sward becoming benty and mature."

Further publications on this subject from the Welsh Plant Breeding Station will be awaited with great interest.

*Pasture Investigations at Aberdeen.*—During the past year a series of papers has been published from the Rowett Research Institute at Aberdeen, dealing with the mineral content of pastures and its effect on herbivora. It is now well recognised that a deficiency, or an ill-balance, of the inorganic constituents of an animal's diet may produce grave symptoms of malnutrition. Hence it is of primary importance to secure as much information as possible about the nature and amount of the mineral fraction of pasture grass, which may form the bulk, or even the whole, of an animal's diet.

Chemical analyses of numerous samples of pasture herbage from various areas in the British Isles have been made (W. Godden, *Journ. Agric. Sci.*, xvi, 78, 1926), and it has been shown that, in general, the herbage of the hill pastures in Great Britain is markedly poorer than that of the cultivated pastures in respect of silica-free ash and the individual ash constituents, with the exception of sodium. Despite the marked differences in the mineral content of the different types of pasture, there appeared to be very little variation in calorific value. Where sheep were permitted free choice in grazing, they consumed, by preference, the herbage containing the higher percentage of mineral constituents. In a further paper (W. Godden, *Journ. Agric. Sci.*, xvi, 98, 1926), the same writer has brought forward analytical data which indicate that the application of artificial fertilisers to grassland may result in considerable modifications in the mineral content of the herbage. The calcium and potassium constituents were found to display the biggest variations as a result of the application of fertilisers to pasture land, whereas the alterations in the phosphate content were not so pronounced.

Coupled with any marked increase in the calcium content, there was generally to be found an increase in the percentage of nitrogen.

Definite seasonal variations in the mineral content of pasture herbage have been detected in pastures examined at intervals between the months of May and October (E. M. Cruickshank, *Journ. Agric. Sci.*, xvi, 89, 1926). Such variation was most clearly illustrated by the calcium content, which rose in amount as the season advanced and then fell steadily in the later stages. Similar behaviour was displayed to a less marked extent by the phosphorus, sodium, and nitrogen constituents. On the other hand, however, the chlorine constituent did not suffer any marked diminution in amount during the late part of the season. It was further shown that the range of seasonal variation of the mineral content was definitely greater in good types of pasture than in those of inferior quality. The period at which the maximum mineral content was reached varied in different pastures, and it is suggested that the nature of the grazing may exert a considerable influence on the progress of the changes.

In a further publication (A. Elliot and W. Crichton, *Journ. Agric. Sci.*, xvi, 65, 1926), it was demonstrated that the condition of "bent-leg" in sheep can be prevented by the provision of a suitable mineral supplement, and that the disease results in all probability from mineral deficiency in the diet of the animals. The occurrence of bent-leg on pasture grass may be taken as an indication that an extreme lack of some mineral constituent has become evident in the grass itself. Pasture analyses have shown that such grave mineral deficiencies do actually occur in large pastoral areas, and that these areas are correlated with high stock death-rates.

*Pasture Investigations at Cambridge.*—Attempts in recent years to establish the subject of the summer rationing of farm animals on a scientific basis have met with little success owing to the absence of reliable data concerning the nutritive value of grass as consumed by grazing animals. This gap in nutritional knowledge has largely been filled as a result of an investigation carried out recently at the School of Agriculture into the seasonal variations in the productivity, botanical and chemical composition, and nutritive value of medium pasturage on a light sandy soil (H. E. Woodman, D. L. Blunt, and J. Stewart, *Journ. Agric. Sci.*, xvi, 205, 1926). This work constitutes the initial stage of a comprehensive study of the nutritive properties of different types of pasture.

Grazing was imitated by the daily use of a motor lawn-mowing machine, the system of cutting being such as to ensure the whole plot being cut over once per week. The season

(April to November) was divided into ten periods, each period corresponding with the duration of a digestion trial carried out on two wether sheep. The lime, phosphate, and nitrogen balances in the animals were also followed throughout the trials. The pasture plot results were compared with data from contiguous plots which were allowed to grow for hay, and from which, after removal of hay, several successive aftermath cuts were taken. The productivity of the pasture, as conditioned by meteorological and soil conditions, was also the subject of investigation.

The account which follows is necessarily restricted to a brief consideration of the results obtained in connection with the chemical composition and nutritive value of the pasture cuts.

The pasture cuts were characterised by containing a very high percentage of protein, the value ranging from 21.20 to 27.92 per cent. of the dry matter during the season, and a low percentage of fibre in comparison with hay. In respect of digestibility, well-grazed pasture grass was shown to compare favourably with concentrates like linseed cake and to be far superior to the best quality of meadow hay. The fibrous constituent of the young grass possessed the unusually high digestion co-efficient of about 80 per cent., a value very little less than that for the carbohydrate constituent.

It was demonstrated that pasture grass, if grazed closely, possesses a much higher nutritive value than has hitherto been thought. In respect of its dry matter, it may be looked on as possessing the character of a concentrate rich in digestible protein and deficient in digestible oil. It is, in effect, a "watered" protein concentrate. On account of its low content of indigestible fibre, it should not be classed with the coarse fodders.

The nutritive value of the grass was highest during the early part of the season, a gradual diminution taking place during mid-season as a consequence of the drouthy conditions prevailing. With the coming of rain, the feeding value recovered progressively, until in early autumn, when the bulk of the herbage consisted of *Agrostis*, the grass was but little inferior in this respect to that available in spring. Even at the phase of lowest nutritive value, however, the pasture grass was still markedly superior to the best grade of meadow hay.

It is usually assumed that the nutritive value of pasture is relatively poor in the autumn, but the results of this investigation have shown that the falling off in feeding value is very slight provided the pasture is kept efficiently grazed throughout the season. Indeed, it appears probable that the so-called

poor grasses like *Agrostis* possess a nutritive value equal to that of the more esteemed grasses provided they are kept short and immature by frequent cutting for efficient grazing.

The results have shown that on account of its high content of digestible protein, well-grazed pasture grass possesses an extremely narrow nutritive ratio, comparable with that of linseed cake, and constitutes an unbalanced feed for all classes of stock, even for deep milking cows, making a particularly heavy demand for protein. It follows that the common practice of supplementing pasture with linseed cake or other protein concentrates is scientifically unsound, since such additions to a pasture diet further unbalance the ration in respect of protein. Young pasture grass should be supplemented by carbohydrate—rich foods, a conclusion which was further substantiated by the findings in connection with the seasonal variations of the lime-phosphate ratio in the pasture cuts.

The writers emphasised the point that the results of the inquiry were only applicable to such pastures as are kept uniformly short by being grazed to their fullest capacity, and various suggestions, based on the results of the work, were put forward in connection with the management of pasture and meadow. The possibility of enriching pasture with white clover by resorting to the mower (or alternatively by heavy grazing) instead of fertilisers was one which should be kept in mind by the farmer, since this plant flourished and spread in the experimental pasture plot under the system of frequent and close cutting in a most extraordinary manner, the improvement being equal to that which might be effected by the aid of basic slag.

The results of the digestion trials indicated that the commonly accepted figures for maintenance in adult sheep, based mainly on the results of old German trials, were probably on the low side, and that the figure 1.29 lb. of starch equivalent per day more nearly represents the maintenance requirement of the 100 lb. sheep than the old figure of 0.74 lb. Confirmation of this value has been forthcoming in a recent publication (T. B. Wood and J. W. Capstick, *Journ. Agric. Sci.*, xvi, 325, 1926), in which the value 1.26 lb. starch equivalent per day has been arrived at by statistical treatment of a large number of data accumulated in digestion trials on sheep.

*Pasture Investigations at Leeds.*—The results of an extensive inquiry into the costs of grazing on twelve different farms in Yorkshire have been published recently (A. G. Ruston and R. S. Crutchley, Dept of Agric., Univ of Leeds, *Bull.* No. 144, 1926). As a result of their investigation into the economic aspects of the pasture problem, the writers drew the following conclusions in respect of the management of pasture: "If

the most is to be of grassland, it must be kept sweet, which may mean liming ; it must not be water-logged, which may mean draining ; the supply of phosphates must be maintained, which may mean an application of slag ; it must be well and evenly grazed, which cannot be possible when the grassland is understocked and the stock given too free and wide a range."

It will be noted that the last conclusion is in harmony with the findings, cited in the previous sections, of grassland investigations carried out from botanical, chemical, and nutritional standpoints.

# ARTICLES

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## SURFACE TENSION

By N. K. ADAM  
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### PART I

THE subject of Capillarity has been one of the last branches of Physics in which systematic interpretation of the phenomena in terms of modern knowledge regarding molecules has been attempted, and it is still in a transitional stage, in which many diverse and incompatible views as to the mechanisms involved are constantly being advanced. The full molecular theory of liquids cannot yet be worked out, owing to the complicated nature of the forces involved between the curiously shaped molecules, close-packed, and yet in constant rotational and translational motion. Nevertheless it is fairly easy to give a consistent account of the principal phenomena of Capillarity, without invoking any properties of molecules other than their size, shape, mutual attraction, and mobility; although this account will at present be qualitative in some respects, it is hoped that analysis of the molecular properties on which the various phenomena depend may be useful, in showing the lines on which a quantitative theory may be worked out.

The first property of liquid surfaces is that of spontaneous diminution to a minimum area. This is possessed by all liquid surfaces, and results in the formation of spherical masses of liquid, when gravity can be neglected, and mechanical constraints are absent; and in the development of the beautiful surfaces described by Plateau,[1] when the surfaces are forced to assume a given shape at one point by adhesion to a rigid support. This tendency of surfaces to contract is due to the presence of a definite amount of free energy in each square centimetre of the surface.

The origin of this free energy is that, at the surface, there is no attraction outwards on the surface molecules, but all the other attractions which keep a molecule in equilibrium in the interior of the liquid are acting. Hence the net force on a surface molecule is a perpendicular attraction inwards. In

forming a fresh surface, molecules which are to form this surface must be dragged from the interior against this inward force, and consequently, a definite amount of work must be done for each square centimetre of new surface formed. The mechanism of the spontaneous contraction of the surface is that the surface molecules pass to the interior under the influence of these inward attractions, and since the molecules occupy space, the surface must necessarily contract. Thus the property of spontaneous contraction of liquid surfaces is not due to any mysterious "contractile skin" in the surface, and is dependent only on the fact that molecules have mobility, size, and mutual attraction; it will occur, whatever the range of molecular attraction, and therefore no evidence as to the range of molecular attraction can be deduced from the phenomena, which depend simply on the presence of free energy in the surface.

Now mathematically, by the principle of virtual work, a uniform tension in the surface may always be substituted for a uniform free energy. Hence all the multitude of phenomena which depend on the presence of that free energy may be treated mathematically by assuming that there is a tension of constant amount in every direction, acting tangentially to the surface. It is important to understand the exact nature of this "surface tension," for a great deal of confusion has arisen through the attempt to make the words "surface tension" bear a physical as well as a mathematical meaning. Substituting a tension for a free energy in a surface, means that the conditions of equilibrium of a system into which the surfaces enter can be deduced without going through the mathematical process of considering the changes in area of the surfaces involved in a slight displacement, and adding up the changes in free energy necessary to effect these changes of area, finally equating the sum to zero. It is sufficient to resolve the equivalent tensions in any convenient direction, to obtain the equilibrium conditions. The terms "surface tension" and "free surface energy" are precisely equivalent.

It follows at once from the fact of free energy in a surface, that there is a difference of pressure on the two sides of a curved surface. If a portion of the surface is displaced parallel to itself, the lines joining the boundary to the centres of curvature remaining fixed, it increases in area when moved with the convex surface foremost; the only source from which the work required to increase the area can come is a greater pressure on the concave than on the convex side. It is easily shown that

$$P_1 - P_2 = \gamma \left( \frac{1}{R_1} + \frac{1}{R_2} \right) \dots \dots \dots (1)$$

$\gamma$  being the surface tension,  $p_1$  the pressure on the concave side, and  $p_2$  that on the convex,  $R_1$  and  $R_2$  the principal (maximum and minimum) radii of curvature of the surface.

When the fluids on the two sides of the surface are weightless or of equal density, the pressure difference is the same all over a closed surface, and the equation to the surface is

$$\gamma \left( \frac{1}{R_1} + \frac{1}{R_2} \right) = \text{const.} \dots \dots (1.1)$$

When, as is nearly always the case, the densities of the two fluids are different, the equation is

$$\gamma \left( \frac{1}{R_1} + \frac{1}{R_2} \right) = gz (\sigma_1 - \sigma_2) + \text{const.} \dots \dots (1.2)$$

$\sigma_1$  and  $\sigma_2$  are the densities of the two fluids,  $z$  the height of the point at which the curvature is measured,  $g$  the acceleration of gravity.

Equation (1.2) gives the form of a meniscus of liquid under gravity. It is incapable of solution in finite terms, and numerous approximate solutions have been given for special cases. Far the most general, as well as accurate, solution is that of Bashforth and Adams [2], which gives the co-ordinates of surfaces of revolution about a vertical axis to five significant figures, a greater accuracy than has yet been attained in experimental work. This solution includes the important cases of menisci in vertical tubes, sessile, and hanging drops, and bubbles formed on cylindrical tubes in liquids. Unfortunately, this monograph has been out of print a long time and is now extremely rare; there is no adequate substitute for it.

Several of the common methods for determining surface tension are simply devices for recording the pressure across a surface of known curvature. The capillary height method is nothing more than this, the radius of the tube and the angle of contact (which will be shown later to be a function of the relative intensity of the cohesion between the liquid particles and the adhesion of the liquid for the solid) imposing a definite curvature on the liquid surface in the tube, the liquid then adjusting itself to meet the equilibrium conditions of equation (1) by rising or falling to the appropriate height in the tube, the driving force which brings the meniscus to the equilibrium position being simply the hydrostatic pressure difference. For a very small tube, in which the meniscus is not appreciably distorted by gravity from the spherical form, let the radius be  $r$  and the angle of contact  $\theta$ ; the radius of the meniscus is uniform and equal to  $r \sec \theta$ , the pressure under the meniscus

becomes  $\frac{2 \gamma \cos \theta}{r}$  less than that under the plane surface, and the liquid rises to a height  $h$  such that

$$\frac{2 \gamma \cos \theta}{r} = gh (\sigma_1 - \sigma_2) \dots \dots \dots (2)$$

$\sigma_1$  and  $\sigma_2$  being the densities of the liquids on the two sides of the interface. If the tube is too large for the divergence of the meniscus from the spherical form to be neglected, the exact equation for the height of the *lowest point* of the meniscus above the plane surface outside is

$$\frac{2\gamma \cos \theta}{b} = gh (\sigma_1 - \sigma_2) \dots \dots \dots (2.1)$$

$b$  being the radius of curvature of the meniscus at its lowest point. If  $\cos \theta$  is negative, the liquid falls in the tube, and the meniscus is convex upwards.

The only mathematical difficulty in the capillary height method is the determination of the radius of curvature  $b$ . Bashforth and Adams's tables give a means of finding the relation between  $b$  and the radius of the tube, and Sugden [3], working from these tables, has constructed a convenient table from which the surface tension may be calculated from the observed height of rise in tubes up to 17 mm. diameter (for water). The approximation formulæ given by other writers and summarised by Richards and Carver [4] do not cover such a wide range of sizes of tube as Sugden's tables and are not more accurate.

Washburn [5] has examined the movement of a liquid in a capillary tube under the driving action of the hydrostatic pressure caused by the curved meniscus, and the viscosity of the column of liquid, finding results in agreement with theory.

When a bubble is blown on a tube immersed in a liquid, if the tube is small, the bubble will be nearly spherical at all stages of growth. The pressure at each stage of growth will depend on the curvature of the surface and the surface tension, and will reach a maximum when the radius of curvature is a minimum. This will occur when the bubble reaches a hemispherical shape. At this stage the bubble becomes unstable, for any increase in size is attended by a diminution in pressure, and the bubble breaks away. The pressure exceeds that at the same level outside the bubble by  $\frac{2\gamma}{r}$ . It is easy to measure

the pressure required to produce bubbles in a liquid, and the method has been used on all classes of liquids, fused salts [6] and metals [7] included. Sugden [8] has applied Bashforth

and Adams's tables to the calculation of the corrections for the deviation from a spherical form of the bubble, and by the use of two tubes of different diameters, and calibration of the apparatus on a liquid of known surface tension, has reduced the method to one capable of giving results correct to 0.3 per cent. in a few minutes, with very simple apparatus.

Scarcely any method has been more used for determining surface tensions than the "drop-weight" method. The theoretical relation between the surface tension and the weight of drops breaking away from a vertical tube is not even yet fully understood, although hundreds of papers have been published on the method and the results obtained by it. In spite of its inadequate theoretical foundation the careful experimental work of Harkins and Brown [9] has mapped the form of the curve relating drop size to the ratio  $r/a$ ,  $r$  being the radius of the

tip from which the drop falls, and  $a = \sqrt{\frac{2\gamma}{g(\sigma_1 - \sigma_2)}}$ . Iredale [10] has introduced an alternative method of calculating from Harkins's results, calling attention to the theoretical work of Worthington [11]; from these papers the principal references may be obtained. It is important to note that the commonly adopted method of finding the weight of a drop of a liquid of known surface tension, and assuming that the weight of drops of other liquids from the same tip are proportional to their surface tension, may give results very seriously in error, if the values of  $a$  are seriously different for the calibrating and the other liquids. Using Harkins's technique and corrections, surface tensions may be obtained within one or two tenths per cent.; nearly all the results obtained prior to Harkins's work require correction.

Other important methods are (a) measurement of the downward pull on a vertical plate or cylinder in the water surface (Wilhelmy's method) [12]; (b) finding the pull required to detach a ring from the surface [13]; (c) measuring the wave-length of ripples in the surface [14]; (d) measuring the distance between successive nodes in an oscillating jet issuing from an elliptical orifice [15]; and (e) finding the time of oscillation of falling drops [16]. These cannot be described in detail; (a) and (b) depend on the fact that the plate and the ring have liquid adhering to them; it is usually assumed that the net effect of a slight rise of the plate or ring is to increase the area of the liquid adhering, as if it were a vertical film of the same length as the solid to which it is attached. It seems possible that method (b) is subject to more error than has been admitted by recent workers using it.

At present the capillary height method is the ultimate standard. Harkins and Brown [9], and Richards and Carver

[4] have ascertained the surface tensions of a few liquids to less than 0.05 dyne per centimetre. It has been shown [4] that with clean glass, the angle of contact is zero, for water and several organic liquids, provided that the liquid is allowed to reach the equilibrium position by falling, not by rising. The reason for the angle being smaller when the liquid is falling than when rising, will be shown later to be simply friction of the liquid on the glass. The surface tension of common liquids against air has not been found appreciably different from that against their own vapour in an evacuated vessel.

The labour involved in the selection of tubing for an accurate determination by the capillary height method is very great. For most purposes Sugden's modification of the maximum bubble pressure method [8] is sufficiently accurate; the drop weight method, using Harkins's corrections, is also accurate, but probably less convenient. The drop weight method is the usual one for interfaces between two liquids, though the capillary height also gives accurate results. Methods based on simultaneous measurements of curvature and pressure, using equation (1) directly, have often been proposed, but none seem to have been worked out sufficiently to be readily applied to everyday measurements.

It has been frequently discussed whether the surface tension of a liquid is a well-defined property. There is every reason, theoretically, to suppose that the average work necessary to drag the molecules required to form a new surface from the interior, should be capable of precise measurement, provided the areas are so large that the molecules may be treated statistically. There has been some variation in experimental measurements, but now the agreement between the most reliable measurements is within experimental error. It must also be remembered that owing to the mathematical difficulties in the theory of most methods, the use of approximate formulæ has been exceedingly common, and it is certain that results have frequently been expressed in numbers suggesting an accuracy greater than could possibly have been obtained with the approximations used. Many of the divergent results recorded in the literature are due to this cause. There seems no ground for the belief that the surface tension is not a property of liquids, just as well defined as density and temperature.

Methods (c), (d), and (e) measure the tension of a very recently formed surface. This may, in the case of mixtures, differ greatly from that of an old surface in which adsorption has had time to occur. With dilute soap solutions, Rayleigh found the surface tension by the method of oscillating jets to be nearly that of pure water, the rate at which the molecules

of soap left the interior to form the adsorbed layer being in this case small compared with the rate of measurement of surface tension. It has been claimed [17] that water acts as a mixture of polymers in this way, the final surface tension not being reached for several thousandths of a second after formation of a surface. But the experimental results seem conflicting; Bohr [18] obtained no such effect in accurate experiments with oscillating jets; and the experiments of the latest workers are not very concordant, leading to the improbable conclusion that the tension of surfaces less than 0.002 second old attains a maximum at 12.5° C., having at 8° and 25° values only half as divergent from the final value as at 12.5°. It seems possible that the experimental errors are nearly as large as the whole effect sought, and the time-decrease of the surface tension of water cannot be considered as an established fact.

There is an important series of relations between the adhesion across a surface of contact between two liquids, or a solid

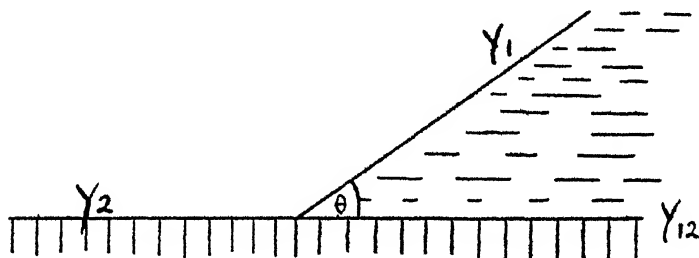


FIG. 1.

and a liquid, and the surface tensions. The presence of the second phase always diminishes the perpendicular attraction inwards on the surface molecules of each phase. Dupré [19] showed that the work  $W_{12}$  required to separate one square centimetre of two phases in contact is

$$W_{12} = \gamma_1 + \gamma_2 - \gamma_{12} \dots \dots (3)$$

$\gamma_1$ ,  $\gamma_2$  being the surface tensions and  $\gamma_{12}$  the interfacial tension between the phases in contact.

Solid surfaces are generally met by liquids at an angle of contact  $\theta$ . Solid surfaces have a surface tension, for the inward unbalanced attraction on the surface molecules is present, although the lack of mobility of the molecules prevents this appearing as a tendency of the surface to contract. In Fig. 1, the surface tensions are  $\gamma_1$ ,  $\gamma_2$ ,  $\gamma_{12}$ ; resolving the tensions parallel to the surface (a mathematical process equiva-

lent to considering the changes of area and energy involved in a slight displacement of the line of contact)

$$\gamma_2 = \gamma_1 \cos \theta + \gamma_{12} \dots \dots \dots (4)$$

and combining with (3),

$$W_{12} = \gamma_1 (1 + \cos \theta) \dots \dots \dots (5) [20].$$

These adhesions give more insight into the molecular structure of surfaces than does the surface tension, because the attraction across an interface is more easily visualised than the attraction exerted inwards on the surface molecules. The adhesion or cohesion of a liquid for itself is  $2\gamma_1$ .

Hardy [21] and Harkins [22] have found that between organic liquids and water, the adhesions  $W_{12}$  are, in any homologous series, nearly independent of the molecular weight, but are determined mainly by the end groups.

Compounds.	End groups.	$W_{12}$ (ergs per sq. cm.).
Paraffins . . . . .	Hydrocarbon	36 to 48
Alcohols . . . . .	$\text{CH}_2\text{OH}$	92 to 96
Acids . . . . .	$\text{COOH}$	89 to 96
Ethyl esters . . . . .	$\text{COOC}_2\text{H}_5$	ca. 75
Halogen derivatives	Halogen	ca. 70

Similar relations were found for mercury and organic liquids.[23]

Compounds.	End groups.	$W_{12}$ (ergs per sq. cm.).
Paraffins . . . . .	Hydrocarbon	ca. 120
Iodides . . . . .	$\text{CH}_2\text{I}$	ca. 200
Alcohols . . . . .	$\text{CH}_2\text{OH}$	ca. 150
Acids . . . . .	$\text{COOH}$	ca. 160
Acetylene tetrabromide	$\text{CHBr}_2\text{CHBr}_2$	ca. 215

Now these figures indicate that the oxygen-containing groups provide the main part of the adhesion between the organic liquids and water; the halogens also have a strong adhesion for mercury. This must mean a strong tendency to orientation of these groups towards the water or mercury. There is no proof, however, that the surface molecules of the liquids are rendered stationary in a layer with all the active groups oriented; the data do not justify the assumption that there is complete orientation. If that were the case, there would be no variations in the adhesion within each homologous series; the observed variations are probably well outside experimental error. There must be complex thermal motions, and in order to unravel these the influence of temperature on the adhesions will have to be thoroughly worked out and interpreted in terms of kinetic theory.

In the case of solid surfaces the adhesions may be determined from measurements of the contact angle by equation (5). Though this equation was quoted by Young [24], is deducible from Laplace's theory of Capillarity, and has been occasionally mentioned in the literature, it has been little used, and the factors which determine contact angles are not widely known. It shows that if the adhesion  $W_{12}$  is equal to, or greater than, the cohesion of the liquid  $2\gamma_1$ , the contact angle is zero. If the adhesion is very small in comparison with the cohesion, the angle approaches  $180^\circ$ ; for intermediate values, small angles correspond to large adhesions. Contamination of the liquid surface, which reduces the surface tension, without affecting the adhesion, diminishes the angle. Therefore in determining contact angles it is essential to provide means of cleaning the surface of liquid. Again, in practice, the contact angle may have any value between two extremes. Any drop of rain on a window-pane can be seen to have two angles, a large one at the lower edge and a small one at the upper. It is always found that when the liquid edge is tending to advance over the solid, the angle is greater than when it is receding, or trying to recede. This phenomenon, recently claimed as a new discovery, was certainly noticed by Rayleigh [25] and studied by Pockels [26]. It is easily explained as a friction between the liquid and the solid [27]. Let  $F$  be the force which just prevents motion of the liquid on the solid. Adding  $F$  to the side of equation (4) which tends to oppose the motion, for advancing motion ( $\theta_A$ )

$$\gamma_2 = \gamma_{12} + \gamma_1 \cos \theta_A + F$$

for receding ( $\theta_R$ )

$$\gamma_2 + F = \gamma_{12} + \gamma_1 \cos \theta_R$$

and by combination with (4)

$$2 \cos \theta = \cos \theta_A + \cos \theta_R \dots \dots (6).$$

Hence the equilibrium angle may be found from measurements on a liquid on the point of advancing and on the point of receding. Generally the angles remaining after advancing and after receding motion differ by  $20^\circ$  or more. In the case, however, of a smooth surface of paraffin wax, turned in a lathe, Ablett [28] has found that the angle of rest is the same when reached from either side, and is the mean between the angles when the liquid was advancing at a definite speed and receding at the same speed. The angles being in the neighbourhood of  $90^\circ$ , the mean of the cosine was practically the cosine of the mean angle, so that equation (6) has been verified in this case.

The relations between several hitherto uncorrelated phenomena are brought out by regarding the variation of contact angle as a frictional effect. Liquids falling in capillary tubes usually stop at a greater height than liquids rising up a dry tube. Liquids rising in a wet tube may reach the same height, because the friction of liquid on the wet surface is less than that on the dry. The variations in angle are sometimes explained as due to a residual wetting left on the solid by the receding liquid, increasing the attraction of liquid for solid [29]. This will not, however, explain the fact that the variation is found when there is only a tendency to move, not actual motion; and would, even in the case of actual motion, require a complicated law relating the hygroscopic state of the recently wetted surface to the vapour pressure of the liquid in the surrounding atmosphere.

This frictional force also explains why there is sometimes a resistance to the passage of solid objects through the surface of a liquid. When a waxed needle floats on water, the angle at which the liquid surface meets the solid approaches the "advancing angle" and must reach this before the water can move over the surface. The larger the contact angle, the more probable it will be, at any stage of immersion, that a slight displacement of the solid downwards without moving the line of contact will enlarge the surface of the liquid, and the force resisting this downward movement may be found by treating the surface energy of the liquid as a tension and resolving vertically. If it were not for the friction, the slight downward displacement would move the line of contact of liquid and solid, and the flotation could not be stable.

Sometimes the floating of heavy bodies is ascribed to a surface skin, and is considered to be direct ocular evidence of the existence of such a skin. Let us compare the essential properties of such a skin with that which is supposed to cause the spontaneous contraction of liquid surfaces to minimum area. The first essentially resists perforation; it is like a hammock in which the solid rests. The second has, as its essential property, the power of spontaneously expelling molecules from itself, for without the expulsion of molecules the surface cannot contract indefinitely. Such are the opposite, and contradictory, qualities, to which the hypothesis of an elastic skin surrounding liquids leads as soon as the molecular structure is considered. The hypothesis can be only a very superficial analogy.

The very important technical processes of flotation of minerals [30], and showerproofing of textile fabrics with waxes and similar substances, depend on the small adhesion between water and the solid surface, and on the existence of this fric-

tion. The showerproofed fabrics may be compared to a series of fine waxed tubes ; the advancing angle of contact being greater than  $90^\circ$ , any water entering will have its surface convex forwards. By equation (1), therefore, a considerable positive pressure will be necessary to force the water into the tube. Once, however, the pores are filled, there is little resistance to passage of water through the fabric.

Equation (5) has been used [27] to explore the orientation of molecules in the surfaces of solid fatty acids and other compounds. When a substance such as stearic acid crystallises from a molten mass, the surface has an angle of contact, that is, an adhesion for water, nearly the same as that of paraffin wax. This angle is  $105^\circ$  ; application of equation (5) gives the adhesion between wax and water as 54 ergs per sq. cm., in fair agreement with the value 48 recorded above for the adhesion between a liquid long chain hydrocarbon and water. This was the value for a naturally formed surface of the fatty acid ; but if the mass was cut with a knife, the cut surface gave a variety of angles, ranging from  $60^\circ$  to  $100^\circ$ . This variation is in complete accord with what is known from X-ray studies of the molecular arrangements within the crystals of fatty acids. There are parallel sheets of molecules in pairs, the oxygenated groups of each half of the double sheet being in contact. There is a very strong cohesion between the carboxyl groups, and a weak cohesion between the hydrocarbon ends of the chain. A cut with a knife which happens to be made parallel to these sheets will necessarily pass between the hydrocarbon ends of the molecules, and will leave a surface like paraffin wax, which has a high angle of contact. But a cut taken in another direction will expose a number of carboxyl groups, one pair for each unit sheet cut across. Hence the number of carboxyl groups exposed, and the water-attracting power of the surface, depend on the angle made by the section with the direction of the sheets of molecules within the solid. With paraffin wax, and long chain iodides, which have no group attracting water strongly, the angles of contact both of the natural and the cut surfaces were always close to that of paraffin wax. These results show a strong tendency of the long chain compounds to crystallise with hydrocarbon groups only exposed at the surface, the water-attracting groups being buried in the interior.

Devaux [31] has crystallised various substances in contact with water, finding that the surfaces which have been subject to contact with water attract water more strongly than those crystallised in contact with air. This result was obtained with paraffin wax, copper sulphide precipitates, and many organic compounds. The fact that paraffin wax showed the

effect indicates that it is probably not (as M. Devaux suggests) due to orientation of the molecules next the water, but more probably to water soaked into the surface layers, the absorbed water increasing the adhesion. The increased ease of wetting of a surface of fatty acid solidified in contact with water is not invariably obtained; the author and Jessop found [27] that such a surface might have the angle of paraffin wax, the cause being very probably that a surface layer had been oriented to the water during solidification, but on removing the solid cake, had adhered to the water instead of to the solid. The relative magnitude of the adhesions between carboxyl and water, and between the ends of hydrocarbon chains, would make it much more likely that the oriented layer would stick to the water than to the solid mass.

The term "surface" of a solid needs definition. It has appeared from a comparison of the water attracting, and catalytic properties of several surfaces [32], that the power of catalysing the combination of ethylene and halogens is exercised by oxygenated groups buried probably several times the length of a long hydrocarbon chain beneath the surface, while as a rule the water-attracting properties seem to be conditioned by the outer groups, a few carbon atoms at the most in length. Further evidence on this point is much needed; simultaneous study of a series of solids by X-rays, angle of contact, and their catalytic power would probably give much information.

There are many further complications in regard to solid surfaces. Schumacher has shown [33] that by stringent precautions to remove gas and moisture from glass and silica, the adhesion of mercury may be increased so far that the angle of contact falls below  $90^\circ$ . Again, there is abundant evidence that different parts of the same surface, even perhaps adjacent molecules, may have widely different energies; the catalytic properties have shown this, and observations on the heat of adsorption of oxygen on charcoal also indicate that some atoms are exposed, attached to the underlying solid by such slight forces that their heat of combination with the adsorbed oxygen is greater than that of solid carbon, when it burns. The evidence is too extensive to treat here [34]. Beilby has shown that the surface structure is profoundly disturbed by polishing or hammering, resembling a suddenly cooled liquid layer [35]. There does not seem any necessity to suppose that actual liquefaction occurs, for the polishing may mechanically tear away the molecules and redeposit them at random. Solid surfaces become soiled quickly, acquiring a lubricating layer; a freshly cleaved surface of calcite both wets with water and orients sodium nitrate crystals deposited on it from aqueous

solution, with their faces parallel to the faces of the calcite ; after a short time this power is lost. It has been suggested that this loss of wetting and orienting power is due to a re-orientation of the surface atoms of the calcite, but there seems to be no evidence as yet that the whole effect is not due to deposition of a lubricating layer.

All these properties are due to the lack of mobility of the surface atoms of a solid. Atoms left in a state of strain by the most recent treatment of the surface probably do not alter much spontaneously. Unlike a liquid surface, a solid surface is not equipotential. In the case of metals far below their melting points, there is evidence of the rounding off of the corners of crystals under surface tension ; this points to some mobility, but its amount is in any case very much less than that of liquid surfaces.

It is clear that the method employed by Hulett [36] for determining the surface tension of solids, based on the increased solubility of exceedingly small particles, cannot give results of much value, for the methods which must be used to reduce a solid to the necessary fine state of division are certain to affect its surface properties profoundly. At present we are without a reliable method for determining the surface tension of solids, and in most cases the "surface tension" of a solid can mean nothing more than an average value, which may vary from atom to atom.

The spreading of substances on solid and liquid surfaces is now fairly well understood. We may divide the discussion into two parts ; first, of the thermodynamic conditions under which spreading can occur, second, the mechanism by which it actually does occur. The thermodynamic condition for spreading is that the cohesion of the substance for itself ( $2\gamma_1$ ) should be less than its adhesion for the surface ( $W_{12}$ ). This is, by (3), also the condition that the tension of the surface should be greater than the sum of the tension of the substance, and of the interfacial tension of the substance against the surface *i.e.* that  $\gamma_2 > (\gamma_1 + \gamma_{12})$  [37]. This condition is, by (5), also the condition of zero contact angle. The difference  $\gamma_2 - (\gamma_1 + \gamma_{12})$  has been called the spreading coefficient ; and the spreading of liquids on solids, at zero contact angle, is often called "wetting." This affords a definition of the term "wetting" ; other definitions have been used, but this appears to be the commonest and the best.

When liquids do not spread on other liquids, there is a contact angle, just as with solids ; but usually no two of the three surfaces concerned are continuous. There are three angles  $\theta_1, \theta_2, \theta_{12}$ , between the three surfaces, and these angles may be determined by writing  $\gamma_1, \gamma_2, \gamma_{12}$ , for the tensions of

the surfaces and treating them as three forces meeting at a point. The condition of equilibrium is

$$\frac{\gamma_1}{\sin \theta_1} = \frac{\gamma_2}{\sin \theta_2} = \frac{\gamma_{12}}{\sin \theta_{12}} \dots \dots [7]$$

sometimes called "Neumann's triangle." The angle  $\theta_2$  between the surfaces  $\gamma_1$  and  $\gamma_{12}$  may be increased by contaminating the surface  $\gamma_2$ , so as to reduce its tension. A very striking illustration of the mechanism by which the contamination decreases the tension (or free surface energy) of water is afforded by the following experiment. Drops of a non-spreading oil, such as medicinal paraffin, are placed on the clean surface. The drops form small lenses. A little palmitic acid, or other substance which forms a stable surface film, is now put on the water surface, and the area is reduced by means of a barrier sweeping the whole width of the surface. This concentrates the contamination and further diminishes the surface tension, the angles at the edge of the lenses increase,

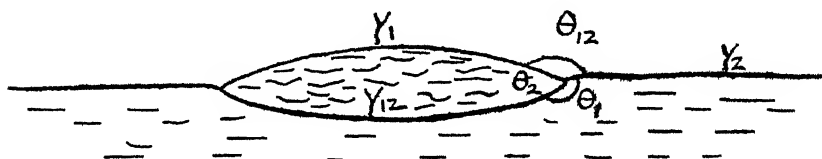


FIG. 2.

and the focal length of the lenses decreases. But the action of the barrier is evidently to push the film of palmitic acid along the surface, and the film in turn squeezes the lens at the edges, making it thicker. Thus the compression exerted on, and transmitted through, the film is the cause and the measure of the diminution of the surface tension of water by this film. This point of view will be seen to be of great importance in the discussion of surface films.

When a liquid spreads on a liquid, as a rule the drop spreads out into a thin sheet with great rapidity until it covers the whole surface, provided this is not too large. The advancing edge of oil may move at a rate of about 20 cm. per second. This sheet is thick enough to show interference colours, but it is not stable. It breaks up, generally very rapidly, into an invisible film (which is one molecule thick) and a number of droplets visible with a lens, or without [38]. Osborne Reynolds first noted [39] that the oil advances with a definite edge, which heaps up the water as a very narrow ridge in front. By dusting the surface of the water prior to putting on the oil, he found that there is no movement of the dust except just at

the advancing edge of oil. This observation caused him to remark that the oil appears to spread by some inherent expansive force. But he was so much inclined, *a priori*, to the view that it is the surface skin of the water which pulls out the oil, that he did not look for a cause for the expansive force of the oil, and was led into serious difficulties in trying to explain why the tension of the water surface was not made manifest by movement of the dust over a considerable area. There seems, however, no reason to suppose that the sheet of oil is actually pulled out by the contraction of any surface skin of the water, another mechanism being readily available. The thermal motions of the water molecules are continually causing motion to considerable distances along the surface. The oil molecules next the water adhere to the water molecules and are carried out by them. If the oil has the right adhesion for the water for spreading to be stable, there will be little tendency to return to the drop, and molecules some distance out in the spread film will be pushed out by other molecules from under the drop. Motions towards the drop are blocked, while motions away from the drop are not opposed except by viscosity, hence the thermal motions of the water molecules will be manifest as a spreading. This would appear at first sight only to account for the spreading of a thin layer, thin enough for all the molecules in it to be strongly attracted by the water. But the movement outwards of the molecules next the water is rapid, and the oil has viscosity; hence the layer next the water will drag with it, temporarily, a sheet of oil.

Leslie [40] explained spreading as an attraction of the lower liquid on the upper liquid, squeezing it out on the surface. This would require that the attraction of the water should extend strongly beyond the first layer of oil molecules. The explanation has been revived by some recent writers, but does not seem correct, for if the attractions of the water on the oil, perpendicular to the surface, are the cause of the spreading, it is not explained why the final and stable state is a film one molecule thick, in equilibrium with drops. The squeezing action should cause these drops also to spread. The explanation as a consequence of the horizontal thermal motion of the water molecules fits all the facts.

Solids, like liquids, may or may not spread upon liquids. At a given temperature they exert a measurable outward surface pressure [41], whose variation with temperature shows some extremely interesting discontinuities, which are not yet fully clear. The rate of spreading is determined by the rate at which the molecules leave the edge of the crystal in contact with the surface [42]. This is, for myristic acid on a water surface, at  $15.8^{\circ}\text{C.}$ ,  $4.5 \times 10^{12}$  molecules per second, per linear

centimetre of contact between the crystal and surface. The rate of diffusion along the surface, once the molecules are clear of the crystal, appears to be very great compared with this, for the surface pressure seemed to be quickly equalised on the surface. Volmer and Mahnert [43] found that the rate at which molecules of benzophenone left the surface of a crystal in contact with mercury was enormously greater than the rate of evaporation. Their figures agree as to order of magnitude with that quoted for myristic acid on water, but the perimeter of their crystal was not stated.

Upon solid surfaces we should not expect any surface diffusion, at least in the layer of molecules next the solid, because the molecules of solids have no appreciable mobility at ordinary temperatures. Hardy [44] finds that the usual and probably the only method of spreading from a drop placed at one point of a plate is through the vapour phase. The spread film forms a lubricating layer on the solid, and the rapidity of formation of this layer at a distance from the drop is much greater with high than with low vapour pressures; the film does not seem to be formed at all with practically non-volatile liquids. Volmer and Adhikari [45] claim to have found a measurable surface spreading across strips of clean glass a few hundredths of a millimetre wide; they placed benzophenone on a glass slip, very close to the edge, and washed the edge with a succession of drops of mercury. The mercury carried off any benzophenone which reached the edge by diffusion, and the loss from a slip treated with mercury was compared with that from a slip allowed to evaporate in air. Reducing their observations to a standard width of glass strip and a standard length of edge, I find variations of 340 per cent. in extreme cases in individual experiments; the evidence for the existence of this spreading does not therefore seem quite conclusive. The strip of glass over which diffusion occurred was so narrow that there may have been disturbances in the vapour pressure of the benzophenone in the atmosphere near the mercury, sufficient to account for the difference. The mean value obtained for the rate of diffusion across the strip of glass works out as  $2.8 \times 10^{13}$  molecules per second, per centimetre length of edge at  $17^\circ \text{C}$ . It will be interesting to find whether this is confirmed, and whether the diffusion occurs in the layer next the glass, or several molecules distant therefrom.

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# THE PROBLEM OF COMPLEX STRUCTURES FROM THE POINT OF VIEW OF CRYSTALLOGRAPHY AND X-RAYS

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NEARLY every solid thing in this world is crystalline. And that is a very significant fact, for it means that crystallography is one of the most important and comprehensive branches of modern science. This has been widely recognised only recently, just as it is only recently that it has been forced upon us that really amorphous substances are comparative rareties. And X-rays have been the cause of all this new knowledge. X-rays have put us on the track of a thousand and one things that had seemed to defy further analysis. By their aid we have been able to rescue Crystallography from the obscurity in which she was pining, and to bring her out to take her true place among the glorious band of Sciences that are now making such a determined attack on the problem of the constitution of matter. Before the advent of X-ray analysis, crystallography was almost despairing of getting any reliable details of the "regular irregularities" which the beautiful theory of Fedorov, Schœnflies, and Barlow had predicted with regard to the inner structure of crystals. But since 1912, "*nous avons changé tout cela*"; the structural theory has been applied, verified, and amplified, and a tremendously encouraging start has been made on the gigantic task of classifying all crystalline substances according to their *internal* structural features—an enormous advance on the old classification according to mere external form.

It is now almost commonplace knowledge that crystals are perfectly regular structures formed by the repetition through space of an exceedingly small unit of pattern, a unit cell, as it is called. Such a cell is built up of the substance of a number—generally small—of chemical molecules disposed after some fixed scheme of higher or lower symmetry. During the growth of a crystal, the cells are completed and repeated an indefinite number of times. It is a marvellously quick process, the formation and laying down of these minute bricks of matter,

yet it takes place in a marvellously beautiful and regular manner. It is easy to see that the nature of the laying down of these bricks, being governed by those intriguing laws of atomic and molecular physics for which we are seeking, must throw light on any amount of natural phenomena. What we are doing now is acquiring, through the medium of X-rays, knowledge about the nature of the bricks themselves, after deposition. The next step, the discovery of the nature of the actual deposition processes, must follow, sooner or later, as a matter of course.

X-rays enable us, in the first place, to find the dimensions of these bricks. They are not very big, of course, being only of molecular dimensions, but within these small walls are enshrined the secrets of the fundamental physical properties of the substance *en masse*, since the extended substance is only a repetition *ad lib.* of the primary unit of pattern. In the second place, when we know the density and the molecular weight of the substance, the dimensions give us the number of chemical molecules which go to make up the unit cell. In the third place, X-rays may then be made to reveal the scheme or fixed plan according to which the molecules build the unit cell, and finally, it has been found possible in many cases—of course, ultimately, all crystals must succumb to the attack—to deduce the absolute positions of the molecules and atoms of which it is constituted.

Let us look at this process of deduction step by step. In spite of its reputation, it is not, fundamentally, a difficult sequence of arguments. Forget now for a moment the solid bricks and think rather of a set of points that are distributed regularly through space and divide it up into a series of similar and parallel, brick-shaped cells. That more nearly represents what a crystal is like, for it is not really continuous like a set of solid bricks, but is simply a homogeneous discontinuum. The cells are six-sided parallelepipeds, not necessarily rectangular—the sides of the cells are, of course, imaginary—which lie in juxtaposition and fill the crystal-space completely.

This idea of the space-lattice of perfectly analogous points brings us directly to the problem of the relation between crystal symmetry and that of the constituent atoms or molecules. The properties of such a lattice show that the symmetry possible to crystals is, unlike that often found in the vegetable and animal kingdoms, comparatively low—no higher axial symmetry than twofold, fourfold, and sixfold is possible, for instance. Add now to these the centre and plane of symmetry, and the reader will find it an instructive exercise to try to find out how many combinations of these six symmetry elements are possible to a simple space-lattice,

such as we have described above. It depends upon the things used to mark out the space-lattice, you will say. Exactly. If you have marked it out simply with points, *i.e.* small spheres, you can easily deduce seven simple cells to correspond to the seven crystal systems. But if you have marked it out with *asymmetric* bodies, though they are all exactly alike and similarly oriented in space, and though you have placed them at the corners of the most symmetrical cell possible to crystals, the cube, yet the structure you have built up will show *no symmetry as a whole*. Truly, then, much depends upon the symmetry of the building stones—but not quite everything. You will have perceived by now that just as much depends upon the way the stones are pointing, their common orientation, in fact. Imagine a cubic space-lattice at the points of which are placed small cubical stones. *If these stones are not placed with their outlines exactly parallel to the lines of the space-lattice, the structure as a whole will not show cubic symmetry.* That is a very important point, as we shall see later.

It was shown by Hessel nearly a hundred years ago that there are possible to crystals no more than thirty-two ways of combining the six symmetry elements mentioned above, and Bravais, some years later, proved that there are no more than fourteen types of space-lattice. (Hence the name Bravais-lattices: they include the seven simple ones which the reader has already deduced and seven simple modifications of them.) Thus there are theoretically possible thirty-two classes of crystal-symmetry (all but one have been observed in Nature) which have to be referred to fourteen space-lattices. Now comes the difficulty, to harmonise these thirty-two known types with the fourteen possible space-lattices, *using atoms and molecules to mark out the homogeneous discontinua of actual crystals.*

There is one obvious solution, of course. Given cells of the right shape, *i.e.* the correct one of the fourteen, molecules placed at the corners, if they have *in themselves* the symmetry of a given crystal-class, and if they are all oriented in the right way, will give rise to a structure that will show as a whole the symmetry of the crystal-class. But clearly this is not at all the general solution of the problem, since there are many amounts of quite symmetrical crystals known that have been grown by the deposition of quite unsymmetrical molecules. And, what is perhaps more striking still, there are any amount of relatively unsymmetrical crystals that have been produced by the deposition of what the chemist would regard as very symmetrical molecules. It is easy, however, to make another approximation to the complete general solution of the problem by thinking of molecules not lying actually at the points of an

imaginary space-lattice, *but disposed in various ways round these points*. For instance, if we place at every point of a monoclinic space-lattice (*i.e.* a lattice whose cells are shaped like a sheared pack of cards) not one symmetrical molecule but a pair of asymmetric molecules disposed about the point in such a way that one is obtainable from the other by rotation about the two-fold axis of the cell, we arrive, in effect, at the same result as we had at first, a complete structure symmetrical about a twofold axis. Similarly, we can produce the combined symmetry of a two-fold axis perpendicular to a plane by the suitable arrangement about each point of the lattice of four asymmetric molecules, *or* of two molecules each possessing a plane of symmetry, *or* of two molecules each possessing a two-fold axis of symmetry. And so on, right up to the crystal-classes of highest symmetry of all.

Now note this point. To produce the effect of a plane of symmetry, we require either plano-symmetrical molecules or two sets of asymmetric molecules which are enantiomorphous to each other, *i.e.* related to each other as an object is to its mirror image. That is another result of considerable importance.

To cut a long story short, we may say at once that the mathematical crystallographers, Fedorov, Schoenflies, and Barlow, deduced independently that there are in all as many as 230 crystallographically possible ways of disposing asymmetric building stones about the points of imaginary Bravais-lattices. Two additional conceptions are necessary for the complete solution. One is the *glide-plane* and the other is the *screw-axis* of symmetry. To understand this, forget the boundaries of the crystal. However beautiful it may appear to us, a crystal, from the point of view of a Maxwell demon exploring the unit cells, is to all intents and purposes a structure extended to infinity with monotonous regularity, and any trick is permissible if it will delude him into the belief that his surroundings remain unchanged after a given symmetry operation. For instance, crystallographic arrangements are possible which, from the interior, appear quite unchanged after a reflection followed by a small translation of half the length of the unit cell parallel to the reflection plane, and similarly there are other arrangements which are indistinguishable after rotation about an axis of symmetry followed by a small translation equal to a simple fraction of the length of the unit cell parallel to that axis. These translations being of molecular dimensions, it is impossible to tell by eye whether a crystal is really symmetrical about a true plane or about a glide-plane of symmetry, or about a true axis or a screw-axis of symmetry. This is the point where X-ray analysis comes to the rescue.

When we know the dimensions of the unit cells of a crystal, we can calculate the periodicity or "spacing" of any plane that can be drawn through the lattice points. Such a plane is usually described by the "indices" ( $hkl$ ), which mean simply that the plane intersects the crystal-axes, as represented by the three edges  $a$ ,  $b$ , and  $c$ , of the unit cell, at distances  $a/h$ ,  $b/k$ , and  $c/l$  from the origin. In the normal way, all the calculated spacings would be observed as such in the X-ray analysis, but certain of the Bravais lattices and certain of the 230 ways of distributing bodies about the Bravais lattices (in fact, those involving glide-planes and screw-axes), give rise to abnormal spacings, as they are called, *i.e.* spacings which are simple submultiples of the calculated spacings. This circumstance is indeed a powerful weapon in the hands of the X-ray analyst, for it enables him to answer that hitherto baffling question—Are the elements of symmetry of a given crystal true planes and axes or only glide-planes and screw-axes? Much depends on a satisfactory answer to that question.

The results of the three celebrated mathematical crystallographers, Fedorov, Schönflies, and Barlow, being published before the discovery of the diffraction of X-rays by crystals, are not in a form suitable to present-day requirements. The method of discriminating by X-rays between the 230 space-groups which they deduced is a fairly recent development of crystal-analysis. Three somewhat different treatments of the subject are: (1) "Geometrische Krystallographie des Diskontinuums,"<sup>1</sup> (2) "The Analytical Expression of the Results of the Theory of Space-Groups,"<sup>2</sup> (3) "Tabulated Data for the Examination of the 230 Space-Groups by Homogeneous X-Rays."<sup>3</sup> Only the last-mentioned, being generally used among crystallographers of our own country, will be described further here. The tables therein are arranged after the form:

ORTHORHOMBIC HOLOHEDRY (BIPYRAMIDAL)

No.	S. G.	B. L.	$n$ .	Abnormal Spacings.	$p$ .	Possible Molecular Symmetry.
48	$Q_h^2$	$\Gamma_o$	8	$\{ohl\}$ halved if $(h + l)$ is odd; $\{hol\}$ halved if $(l + h)$ is odd; $\{hko\}$ halved if $(h + k)$ is odd.	2  4	2-A $\perp^r$ $\{100\}$ , $\{010\}$ , or $\{001\}$ ; C. 3 mutually $\perp^r$ 2-A's.

<sup>1</sup> P. Niggli, Leipzig, 1919.

<sup>2</sup> R. W. G. Wyckoff, Carnegie Institution of Washington, 1922.

<sup>3</sup> W. T. Astbury and K. Yardley, *Phil. Trans. Roy. Soc., A*, pp. 221-57, vol 224, 1924.

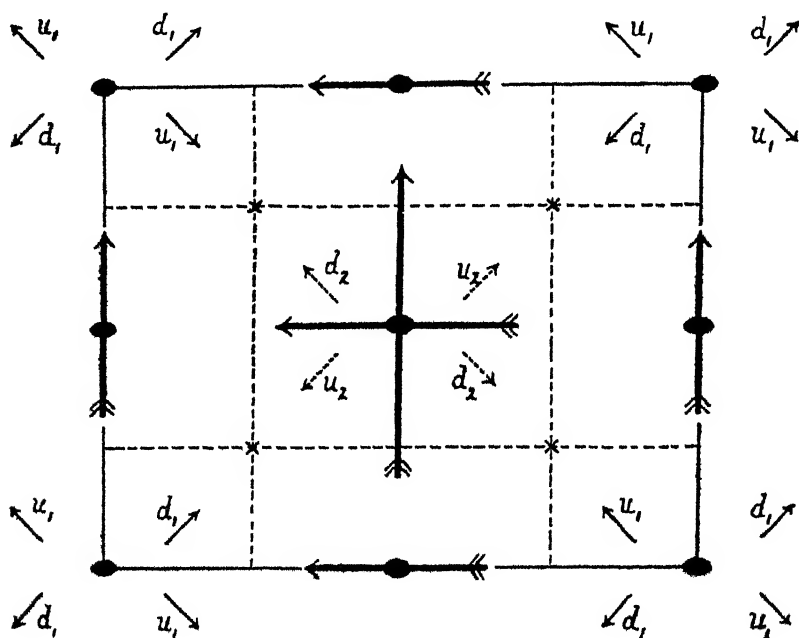
In this example, S.G. means Space-Group in the Schoenflies nomenclature : B.L., Bravais-Lattice ;  $n$ , the minimum number of asymmetric " molecules " required to show the symmetry of the crystal-class (in this case orthorhombic holohedry or bipyramidal) ;  $p$ , the symmetry of " molecules " that may be used instead of an equivalent number, *i.e.*,  $p$ , of asymmetric " molecules " ; 2-A, two-fold Axis ; C, Centre of Symmetry. (N.B.,  $n/p$  = actual number of " molecules " per unit cell.)

Corresponding to the 230 space-groups analysed and classified in the above fashion, are found in the same paper 230 diagrams, one for each space-group, which represent in projection form and in as concise a manner as possible all their geometrical characteristics. Such a one is shown in the figure, which corresponds to the example quoted from the tables. It is a projection of the unit cell, as defined by its symmetry elements, on the plane (001). All three of the mutually perpendicular symmetry-planes are glide-planes in this case (*cf.* the three sets of halvings mentioned in the table above), the two perpendicular to the paper being shown dotted. The thick arrows with two straight feathers indicate the twofold axes lying in the top plane of the cell and repeated half-way down. The twofold axes perpendicular to these—again not screw-axes—are shown by the full oval spots at the corners and centres of edges. The asymmetric " molecules " are represented by the small thin arrows marked  $u$  and  $d$ , molecules marked by the subscripts 1 and 2 being enantiomorphous to each other. The position and orientation of  $u$  (pointing *up*, say) are perfectly arbitrary, but once they are fixed, the positions and orientations of the other seven molecules are also fixed by the operation of the symmetry elements shown :  $d$  then indicates that the molecule points *down*. The molecules marked with small *dotted* arrows are half the depth of the cell below the others. The crosses correspond to centres of symmetry.

Such is a brief outline of the conclusions to be drawn from the theory of mathematical crystallography and the phenomena of X-ray diffraction. Time was when the former was theory only, but the systematic utilisation of the latter has vindicated its predictions. Tables of the effects to be expected make dry enough reading, but they represent what is being observed in the laboratory day by day. Once more let us go over the steps to be taken when X-rays are invoked to throw light on a crystalline structure. (a) Determine the crystal class by the use of both the older crystallographic methods and by X-rays. (b) From spacing measurements, determine the true unit cell and the number of chemical molecules contained in it. (c) From spacing abnormalities determine the B. L. and

S.G. (d) If the number of molecules per cell is fewer than the minimum number of asymmetric molecules required by the crystal class, determine the number  $p$  and note from the tables what the molecular symmetry may possibly be. (e) By various other arguments, direct and indirect, try to locate the absolute positions of the atoms and molecules in the cell.

An interesting example of this process is afforded by a recent X-ray investigation<sup>1</sup> of the isotrimorphism of the trivalent metallic acetylacetonates,  $\left[ M''' \left\{ \begin{array}{c} \diagup O - C(CH_3)_2 \diagdown \\ \diagdown O = C(CH_3)_2 \diagup \end{array} CH \right\}_3 \right]$ . Here we have complex co-ordination compounds with three



similar chelate groups arranged round the central metallic atom and crystallising in three different crystalline modifications, two orthorhombic and one monoclinic. Several questions regarding them at once arise in the mind. For instance—Why are there as many as three different isotrimorphous forms? What are the points of resemblance and difference between them? Considering that co-ordination compounds of this type can often be resolved into optical isomers, are any or all of these three modifications optically active forms or racemates? What is the molecular symmetry, if any, and

<sup>1</sup> W. T. Astbury, *Proc. Roy. Soc., A*, vol. 112, 448-67, 1926.

does this confirm or disprove the molecular configuration generally assumed by co-ordination chemists? We may say at once that X-ray analysis gives partial or complete answers to all these questions, even though, at the present state of our knowledge, we are as yet unable to interpret all the experimental data available. The monoclinic prismatic form has four chemical molecules per cell, while the other two forms appear to be rhombic pyramidal with four and sixteen molecules per cell respectively. Thus the two forms containing four molecules per cell are structures built up of simple chemical molecules functioning as *asymmetric* building stones, while in the other form the number of chemical molecules is four times the minimum number of asymmetric building stones required by the crystal class. This means that in this form associated groups of four chemical molecules each are functioning as asymmetric building stones or "crystal-molecules." (In this connection, the reader will recall that in describing the space-group tables and diagrams above, the word "molecules" was put in inverted commas. This was to show that the theory of space-groups takes no cognisance of the *nature* of the building stones. The molecules in the crystal (crystal-molecules) may be equivalent to one or more chemical molecules, the validity of the theory remaining unchanged.) The precise nature of this fourfold association in the  $\gamma$ -acetylacetones we cannot as yet define. Ordinary molecular weight determinations in solution give no indication of its existence. Still, it is undoubtedly present in one of the three forms, though not in the other two.

It was found possible to fix the centres of gravity of the molecules in all three forms. All three structures are based on the same molecular distribution, the only difference between the three lying in the orientations of the molecules. This internal similarity shows itself at once in a remarkable similarity in "habit" or method of growth. The three types of crystals look nearly alike on a casual examination.

The chemist has assigned an octahedral structure of sixfold symmetry (one threefold axis perpendicular to three twofold axes) to these tervalent metallic acetylacetones molecules. At first sight, crystal analysis gives no hint of this high molecular symmetry, since in all three crystals the chemical molecules are functioning as *asymmetric* bodies. The "free" molecular symmetry of the chemist is ignored. None of it is used. Nature has avoided using it by the simple expedient of making the molecules point the wrong way (*cf.* the little exercise in lattice construction mentioned above). Why is this? Once more it must be confessed that we cannot yet give a satisfactory answer. There is the experimental fact, and it is by no

means the first time that such an observation has been recorded. But we can answer without hesitation the question *re* optical isomers. All three forms are built up of both right and left-handed molecules, and are therefore all racemates. This is disappointing, for these compounds have never yet been resolved, and it was hoped that at least one of the forms would prove to consist of one kind of molecule only. The reader should note here how X-ray analysis may serve as an aid to the identification of optical isomers.

Finally, with regard to the last question, we may say that, though the molecules are crystallographically asymmetric, all the evidence furnished by X-ray analysis points to the fact that they are only "just asymmetric," and that a molecule, such as the chemist supposes, gives a complete explanation of quite a striking set of experimental results. For the present we can go no further. The problem of the actual atomic positions remains unsolved.

A still more intriguing but difficult study in crystal analysis is presented by the problem of the constitution of cellulose. But, here again, X-rays appear to have thrown a considerable light on the question. Much work has been done in Germany by Herzog, Gonell, Polanyi, and others on this subject, and their conclusions may be summarised as follows. We do not know the crystal class of cellulose, and because of this it is very difficult to apply the space-group data without ambiguity. Nevertheless, it is believed to be orthorhombic, or nearly so, with four groups of  $(C_6H_{10}O_5)_n$  per unit cell. If this is true, it is a very remarkable and important result. It means that the smallest group that is repeated exactly throughout the cellulose crystallites is no more than  $(C_6H_{10}O_5)_4$ , and that there is no need to assume any larger group in the structural theory of organic chemistry. Indeed, it may well mean that the cellulose molecule is smaller than  $(C_6H_{10}O_5)_n$ , since it is generally found that a unit cell is built up of the substance of more than one chemical molecule. But the X-ray analysis of cellulose is exceedingly difficult and full of pitfalls. No one, knowing the traps that beset the rash or unwary in crystal analysis, will grant that this is at all the last word on the subject. If only we could be sure that the true unit cell has been obtained!

In any case, sufficient has now been said to demonstrate our opening thesis—that Crystallography is one of the most important and comprehensive subjects in the world.

## THE PHYSICAL WORK OF DESCARTES.

By F. WOOTTON, B Sc., A.I.C.

THE early part of the seventeenth century marks the beginning of Modern Science, and few men have exerted a greater influence upon its progress than Descartes. Just as Galileo and Gilbert were pioneers in modern experimental methods of scientific investigation, so Descartes was the originator of the modern mathematical method of investigating. Kepler had formulated a complete hypothesis of the solar system; Descartes went further and attempted to account for the whole universe, including the earth and the objects upon it both inanimate and living. His system was essentially rationalistic; to him the sciences are instruments for cultivating reason, and it is through them alone that man can acquire a control of Nature. His philosophy leads to practical applications which teach men to realise the work of reason. These applications were stated as (1) mechanics or the appropriation by man of the forces of Nature, (2) medicine, (3) ethics. Descartes regarded the material universe as a mechanism, and this new way of looking at Nature is perhaps his greatest discovery. It was the first attempt to explain on a mechanical basis the formation of the solar system, the stars and planets, the tides, etc. If, as the Bishop of Birmingham recently remarked, modern science has banished the gods, it was certainly Descartes who led the way. He also prepared the way for many of the discoveries of Newton and Huygens which have proved so extraordinarily fruitful.

According to Descartes purely material things might in course of time have assumed their present form even if the world had originally been in a state of chaos, provided that God had established certain laws of nature and allowed them to act. In his *Principles of Philosophy* (Part II), after defining the nature of matter in general, he propounds these three laws.

The nature of matter consists simply in its being a substance extended in length, breadth, and depth. Such properties as hardness, colour, and weight may be taken from a body but its extension remains. Therefore its nature does not consist in being hard, etc., but solely in extension. When a body is rarefied or condensed, the particles composing it are

(1) removed farther apart, or (2) brought closer together ; the pores which it contains are larger or smaller.

Space and substance are not different in reality, but in our mode of conceiving them ; the former being regarded as extension in general, and the latter as extension in particular and inseparable from the body itself. A vacuum, *i.e.* a space in which there is no substance, therefore cannot exist, for since a body is that which has extension, it is contradictory to say that nothing should possess extension. A space which has extension must necessarily have substance in it. And it follows from this that indivisible particles or atoms cannot exist. For however small the particles may be they must possess extension, and can be imagined to be divisible, and what can be divided in thought cannot be considered indivisible. Also God cannot decrease his own omnipotence by depriving himself of the power to subdivide any small particle. All matter, then, is of one kind, recognised only by its being extended. Its variety and diversity of forms depend on motion, and its properties can be attributed to two causes : its capability of being divided, and the motion of its particles. Motion is defined as the transporting of a body from the vicinity of those bodies which are in immediate contact with it to the vicinity of other bodies, and it is carefully distinguished from the force which transports. It is a mode of the movable thing and not a substance. Quantity of motion (*quantité de mouvement*), according to Descartes, is the same as Newton's " *quantitas motus*," which we now call momentum, and is measured by the product of the mass and the velocity of the body. Descartes, like Aristotle, believed in the eternity of motion. God is the primary cause of motion ; in the beginning He created motion as well as matter, and He now preserves the same amount of motion in the universe as He then placed in it.

His three laws of nature are given as follows :

(1) Every body remains in the state in which it is, so long as no other agency changes that state.

(2) Every body which is moving, tends to continue its movement in a straight line.

(3) If a body which is moving meets another body stronger than itself, it loses nothing of its motion ; and if it meets one weaker than itself which it is able to move, it loses just as much motion as it imparts to the weaker body.

The first two of these laws are practically identical with Newton's first law of motion published in his *Principia* in 1687, and it is noteworthy that in the second law Descartes departs from Aristotle's conceptions of natural motion as

being circular. Descartes proceeds to formulate seven rules for determining the changes in motion which bodies undergo upon impact. Adam and Tannery describe this third law as the principal error in the Physics of Descartes. It was disproved by the work of C. Huygens upon the mutual impact of two bodies which led to the formulation of the Law of Conservation of Energy by Huygens in 1669.

Galileo's book, *Two New Branches of Science*, published in 1638, contains his Law of Acceleration, viz. the spaces described by a body falling from rest with uniformly accelerated motion are proportional to the square of the times taken to traverse these distances. In a letter to Mersenne, Descartes says that he has found in Galileo's book some of his own ideas which he had formerly communicated to Mersenne, e.g. that the space covered by falling bodies is proportional to the square of the time. It appears, then, that Descartes had independently discovered this relationship.

In considering the nature of solids (*corps durs*) and liquids, Descartes describes a liquid as being made up of small particles which are separately moving in different ways, while in a solid the particles touch each other without any tendency to move apart. The particles of a fluid tend to move equally in all directions.

Descartes concludes Part II of his *Principles* by stating that he does not accept any principles in Physics which are not also accepted in mathematics, and that these principles are sufficient to explain the various phenomena of Nature.

Part III deals with the visible world. The distances from the earth to the moon and to the sun are given in terms of the earth's diameter, and also the distances from the sun to the other planets. These values are all very low, the figure given for the moon's distance being thirty times the earth's diameter, which is much closer than any of the other estimates. The planets, he says, are extremely near in comparison with the fixed stars. The light of the sun and the fixed stars is, indeed, emitted by them, whereas that of the moon and the other planets, including the earth, is borrowed. He mentions that the light seen round the new moon ("the new moon in the arms of the old") is derived from the earth by reflection.

His account of the movements of the earth and planets is on Copernican lines. From an examination of sunspots he concludes that the sun rotates on its axis in eighty-six days. The earth rotates on its axis and the moon round the earth. The earth and the planets each rest in their own sky and are carried round the sun by the movement of the sky which contains the sun. The orbits are not exactly circular, and all the planets are not always in the same plane. Following

Copernicus, he attributes the absence of parallax in regard to the fixed stars as due to their extreme distance from the earth, and that this extreme distance is necessary to explain the movement of comets.

Descartes then proceeds to account for the formation of the visible universe by means of his celebrated theory of vortices. God has filled all space with matter and has given it movement. Assuming then that the matter in a given space is divided into a very large number of particles, and that these particles are moving, each one turning round its own centre, and all of them round the centre of the space, each space will thus form a vortex or whirlpool, and there will be as many vortices as there are stars in the world. The solar system is such a vortex. Remembering the fate of Galileo's dialogue in 1633, he is careful to state that the visible universe did not originate in this way, but was created by God in its full perfection. Nevertheless, in order to understand the nature of plants and animals and how they may proceed from seeds, it may be helpful to consider how the visible universe could assume its present form according to the operation of simple and easily understood principles: "It peculiarly satisfies the mind to comprehend how these things could occur by natural and gradual means." In consequence, then, of this circular movement, the particles of matter with which space is filled have their corners rubbed off and become round. Two elements or forms of matter thus arise, the first consisting of the fine dust or filings of the original particles, and the second of the small rounded particles. The first element gradually becomes finer and finer, and, losing its velocity, tends to settle in the centre of the vortex and becomes the sun or fixed star. The second element continues its circular motion, and the particles tend to fly off from the centre. They are very small compared with the bodies which are seen on the earth, and constitute the heavens. There is also a third element in those forms of matter which, by reason of their size and shape, cannot be so easily moved. This is also formed from the original particles. The very fine dust of the first element in passing through the interstices between the particles of the second element become entangled and twisted and striated in their passage, and as they reach the central part of the vortex they form a kind of froth on its surface. In this way sunspots are formed. These sometimes disappear into a kind of air or ether round the sun. Or they may gradually increase until they form a dense crust. This decaying star may be caught by a neighbouring vortex, and it will either settle in that portion of the vortex which has the same velocity and continue to revolve in that vortex, or if its velocity is greater than that

of the quickest part of the vortex, it will pass out of range of that vortex and so continue from one vortex to another. In the first case a planet will be formed, and in this way the several planets in the solar system have been caught up into the solar vortex. In the second case the comet is constituted.

Descartes in this manner attempts to show that all the bodies in the visible world are made up of these three elements : the sun and the fixed stars of the first element, the heavens of the second element, and the earth, the planets, and the comets of the third element ; and he justifies this manner of distinguishing them by their different actions on light : the sun and the fixed stars emit light, the heavens transmit it, and the earth, planets, and comets reflect it. He tries to reconcile the Copernican theory with the doctrine of the immobility of the earth, and he attempts to explain what causes the planets to move in their orbits. It fails to give a satisfactory explanation of the phenomena, but it does ascribe the motion of the planets to mechanical causes. No doubt Newton was brought up in this doctrine, and in France Newton's ideas did not displace it until the middle of the eighteenth century.

In the remainder of Part III the origin, nature, and movement of the sun, earth, moon, planets, and the fixed stars are dealt with, and there are several references to the nature of light, which will be discussed later.

Part IV deals with the earth and the materials found on its surface. To find the true causes of what is on the earth it is necessary, Descartes says, to retain the hypothesis already made, even if it is false. According to this, the earth was formerly a star made of matter of the first element which occupied one of the fourteen vortices which were contained in the space which he calls the first heaven. He divides the earth into three regions, the first being the innermost and the third the exterior. The particles of the third element which constitute this third region are larger than those of the second element, but not so solid or so much agitated. They were assembled originally by being pushed towards the centre of the vortex composed of particles of the second element. The interstices of the particles of his third element were filled with particles of the other two elements. Later on, different bodies were formed in the third region by four actions or influences : viz. (1) the movement of the particles of the first and second elements, which can bring about a change in the properties of the particles of the third element. The three effects of this action are : to make bodies transparent, to purify liquids and to convert them into other bodies, and to make drops of liquid spherical. The second action is that of weight. The subtile matter (second element) which by moving

round a drop of water pushes all parts of its surface towards its centre, in the same way by moving round the earth also pushes bodies towards the earth and so makes them heavy. A vacuum is defined as a space which is only filled with matter which cannot help or hinder the movements of other bodies. The matter of the heavens is light, because it has more force to fly away from the centre about which it is turning than terrestrial matter has. The third action is that of light, which agitates the particles of the air. Although the rays which come from the sun only exert a pressure in straight lines through the bodies which they meet, they, nevertheless, cause other movements in the particles of the third element which make up the exterior region of the earth. The fourth action is that of heat. This is defined as the agitation of the particles of terrestrial bodies, whether it is excited by the light of the sun or by any other cause, provided that it is greater than usual and is sufficient to move the nerves of the hand so as to be felt. The heat produced by light remains afterwards until driven away by some other cause, for as it is only the movement of the particles, this movement being once excited in them, remains until it can be transferred to other bodies. It is interesting to notice that the modern theory of heat as a mode of motion had been anticipated in the seventeenth century, although the experimental evidence was apparently not sufficient to convince scientific men in the eighteenth century.

Descartes next explains the nature of the three kinds of terrestrial particles. The air is merely a heap of particles of the third element which are so fine and so detached from one another that they obey all the movements of the heavenly matter which surrounds them. Water is made up of two kinds of particles, one kind soft and flexible (that is, water) and the other (the salt) hard and inflexible. There is such a proportion between the size of particles of water and those of air, and also between these particles and the force with which they are moved by the matter of the second element, that when this force is greater than usual the water vaporises, and when less than usual the vapour takes the liquid form.

Then follows a long and detailed account of the nature and properties of fire, glass, and magnets.

Chemical phenomena are reduced to mechanical phenomena, and his views on combustion are more like those of Boyle and Mayow than those of Becker and Stahl. When chalk is heated the action of the fire drives out some of the particles. He is particularly interested in glass, and describes its preparation by the action of fire upon ashes and lime. Its transparency is attributed to the effect of the fire matter which, when the

glass is melted, sinks into it and surrounds the particles so that pores are left through which the action of light can be transmitted in all directions.

A magnet is a body whose particles are so arranged and orientated that channels are left in it which allow the free passage of the ethereal matter (second element) in a certain definite direction. Thus there are two ethereal currents entering by one pole and leaving by the other, so as to form a kind of vortex round the magnet, and the resistance of air maintains this vortex. The repulsion of like poles and attraction of unlike poles is due to the action of these currents on one another. The earth is such a magnet.

Gilbert had stated that the magnetic declination is constant at a given place, but Descartes showed that this was not the case. He also deals with the attraction exerted by electrified amber, resin, etc.

He concludes that all the other remarkable results which are usually ascribed to occult qualities may be caused by the size, figure, situation, and movement of the diverse particles of matter. The book ends with an account of the five external senses.

#### DIOPTRICS

The practical application of the sciences is a very important feature of Descartes' philosophy, and the possibilities arising out of the discovery of the telescope and the fundamental importance of sight, seem to have had a big influence in attracting him to the special study of Optics.

He ascribes the discovery of the telescope to Jacob Metius, a maker of mirrors and lenses, but not a particularly well-educated man, who, by accident, happened to look through two lenses one convex and the other concave. Two other men have been credited with the discovery, viz. Hans Lippershey and Zacharias Jansen, two spectacle makers of Middelburg. The records show that Metius applied to the States-General for the exclusive right of selling such an instrument on October 17, 1608. Lippershey had previously made an application which was considered on October 2, 1608, and the instrument was tested by a committee on October 4, while on October 6 it was decided to give him 900 florins for his invention. On December 15 an exclusive privilege to sell such instruments was refused to Lippershey, on the ground that many other persons had knowledge of such an instrument. Jansen did not invent his telescope until 1610.

His views on the nature of light are given in Parts III and IV, *Principles*, and also in the *Dioptrics*. They are at variance with the generally accepted emission theory which may be

traced back to the Pythagoreans, and they resemble Aristotle's view that light was the activity of a medium. They may be regarded as the forerunner of the wave theory of Huygens.

According to Descartes, light is a pressure transmitted by the small round particles of the second element. In *Principles*, Part III, he says it is the effort which the particles make to fly away from the centre round which they turn that constitutes the nature of light. The force of light does not consist in the continuance of some movement, but only in the way the particles are pressed and endeavour to move, although they do not perhaps actually move. In Part IV he says that although the rays all come in the same manner from the sun and do nothing but press in a straight line the bodies which they meet, they nevertheless cause some movement in the particles of the third element of which the exterior of the earth is composed.

The first discourse also deals with the nature of light and colour. In the first place the comparison is made of a blind man feeling his way along a road with a stick. The movement or resistance of such objects as trees, stones, water, etc., passes along the stick in an instant to the blind man's hand and thence to the brain, and he is able to perceive and distinguish them. In the same way light is merely a certain movement which passes to our eyes through the medium of the air or other transparent bodies in an instant. The different movements of the stick by which the blind man distinguishes different objects are like the different ways in which coloured bodies receive the light and send colours to the eye. From this it follows that nothing material passes from the objects to our eyes, and the objects themselves need not be similar to our conception of them. Next a comparison is made with a vat of half-crushed grapes, the wine trickling out through the holes in the bottom. The bodies which we see around us are porous; and these pores must be filled with some very subtile matter. This subtile matter is like the wine in the vat, and the air or transparent bodies like the bunches of grapes. The particles of subtile matter tend towards our eyes from the sun in a straight line. It is the inclination to move and not the movement which must be considered as light, and the rays are the lines along which this inclination tends. Although the rays are straight when they pass through a transparent body, when they meet other bodies they are deflected or stopped like a ball when it meets an object. If the object is soft like mud all movement is arrested, but if hard the ball rebounds in another direction. Also the ball may have spin as well as direct, straightforward motion, and so it will be reflected in different ways, according to the ratio between

these two motions. Bodies which when met by rays of light take away all the force are black; those which merely reflect it without making any other change in its motion are white; while others which in addition make a change similar to what the ball receives when it is cut are red, yellow, blue, or some such colour.

When the rays meet the surface of a transparent body through which they pass more or less easily than through that from which they have come, they are bent, and this is called refraction. In order to explain refraction, the law of reflection is first deduced, using the analogy of a ball which strikes a hard surface obliquely. The motion of the ball is resolved into two components, one parallel to the hard surface and one at right angles to it. The first component is not affected when the ball strikes the surface, but the vertical component is reversed in direction, and it is assumed that its magnitude does not change. The angle of reflection will therefore be equal to the angle of incidence. (Descartes measures these angles, not with the normal but with the flat reflecting surface.)

In the case of refraction the ball is supposed to strike the surface of a cloth which does not stop or reflect the ball, but allows it to pass through with diminished speed, viz. one half. Since the vertical component only is reduced and not the horizontal, the ball will travel in a direction less inclined to the horizontal surface, and in covering a certain distance its horizontal component will be twice as great after refraction as before.

The ball travelling along AB loses half its speed at the point B in the surface CBE, and consequently takes twice as long to travel to the circumference of the circle below B as it does from A to B. Make BE twice BC and drop a perpendicular from E to I, a point on the circumference, then BI is the path of the ball. (See Fig. 1.)

If it be supposed that the ball acquires an increased velocity at the point B (the horizontal component again remaining constant), the ball will move in a direction more inclined to the horizontal.

If, for example, the ball acquires an increased velocity at B, say one third, its path will be to I so that BE or GI is one third less than CB or AH. (See Fig. 2.)

From this it is concluded that when light enters a medium which offers less resistance to it, it is less inclined to the normal (as in Fig. 2); whereas, if the medium offers more resistance, the rays become more inclined to the normal (as in Fig. 1). Moreover, this inclination is measured by the ratio AH to GI, and not by the ratio between the angles ABH and GBI, for the ratio between these angles varies with the different



Descartes, like Newton, assumed that the sines are the direct ratio of the resistances of the media ; Fermat corrected them by stating that the sines are inversely as the resistances of the media.

Descartes gives as the reason why the rays are more inclined to the refracting surface in air than in water (contrary to the movement of the ball), that the movement of the subtile matter which fills the pores of other bodies is more hindered by the air than by water. The harder and firmer the particles of a transparent body, the more easily do they let the light through. It is also suggested here that light does take some appreciable time to travel, although in the first discourse light is described as passing " in an instant " (not instantaneously) from the sun to us, and that light travels more quickly in a denser medium like water and glass than in air, an assumption which was experimentally disproved in 1850.

The eye is next explained briefly. The space inside the eye is filled with three different kinds of transparent humour. The middle one is the crystalline humour, and has nearly the same refraction as glass and crystal. Behind this is the vitreous humour, and in front of it the aqueous humour, both of which have about the same refraction as water and less than the crystalline humour. Part of the exterior skin is transparent and more curved than the rest (the cornea). The second skin is opaque, but has a small round hole in the middle called the pupil. This hole can be enlarged or decreased in size by the action of a small muscle, according as the object of vision is near or far, bright or obscure. This movement is voluntary, depending only on the will to see well. The crystalline humour can similarly be made more curved or more flat according to the desire to observe near or far objects. The optic nerve is composed of a bundle of small threads, the extremities of which, mixed with small veins and arteries, form a very delicate piece of flesh at the back of the eye. Before describing how visual images are formed at the back of the eye, a short discourse is devoted to the senses in general. The mind, residing in the brain, receives impressions of external objects through the nerves. These nerves have a three-fold character : first, the exterior skins, which have their origin in the brain and are like little tubes, spread out through all the limbs ; second, little threads which extend along the tube from the brain to the extremities of the limbs to which they are attached ;, third, the " animal spirits " which are like a very subtile wind coming from the hollows in the brain and flowing through the tubes to the muscles. The spirits keep the tubes inflated, and cause the movement of the limbs according to the way in which the brain distributes them. The

threads, which are much finer than silk, are used for feeling. If the objects we perceive really produce images in the brain, these images do not in every way resemble the objects they represent, but this does not prevent visual objects being clearly impressed at the back of our eyes. This has been explained by comparison with the images formed in a "camera obscura," a lens being placed in front of the hole and a white screen behind it. The screen represents the retina, the lens the refracting materials, and the whole chamber the eye. It would, however, be more satisfactory if the eye of a man or large animal were used instead of the lens, and a thin sheet of white paper put at the back of it in place of the retina. If you were in a dark room into which no light could pass except through the eye, you would see on the white paper a clear picture of the objects in front of the eye, from which light passes through the eye. In order to produce a clear picture, all the rays coming from one point on the object must meet at a point on the white paper, and this is accomplished by the nature of the refraction caused by the three humours contained in the eye. Similarly no rays come to this particular point except those from the corresponding point on the object. The colour also of the object would not be changed, for colour is caused by the ratio which exists between the direct motion or pressure of the particles of subtle matter and the rotation of these particles about their centres, and this would not be changed in passing through the eye.

The perfection of the image depends on three things, that, as the pupil has a certain size, the several rays from each point in the object converge at a single point on the white screen; that these rays undergo such refractions in the eye that those coming from several points meet again in as many points on the white screen; and that no other rays enter to confuse the action of these rays. The picture is not so distinct at the edges as in the middle, and that is why it was thought that sight takes place principally along the straight line which passes through the centre of the crystalline lens and the pupil—the axis of vision. The distinctness also decreases as the size of the pupil increases, so that what is gained in intensity is lost in clearness. The image also is inverted—what is on the left appears on the right, etc. The picture which is formed in the eye of a living person must be similar to the one which can thus be observed in that of a dead animal, except that the former is more perfect, since the fluids are more transparent, being "full of spirits" and have exactly the required shape. If an eye could be made with a very great depth, a very large pupil, and the refracting surfaces in proportion, the images formed would be much brighter.

The images are not only formed at the back of the eye, but pass from there to the brain by means of the little threads of the optic nerve. These threads convey the effects produced by light on the nerve extremities in the retina to corresponding points in the brain where another picture is formed, and this is traced farther to a small gland in the middle of the brain which is the seat of the "common sense." (This is regarded as the common bond or the centre of the five senses, by which the various impressions are reduced to the unity of a common consciousness.)

The qualities perceived in the objects of vision can be reduced to six principles, viz. light, colour, position, distance, shape, and size. As regards light, the power of the movements caused in the brain where the little threads of the optic nerve end gives the mind the feeling of light, and the manner of these movements produces the sensation of colour, and there is no resemblance between the ideas conceived and the movements which produce these ideas. As the strength of the light seen depends on the force with which each of the little threads of the optic nerve is moved, it is not necessarily equal to the quantity of light in the objects seen, but varies according to their distance and the size of the pupil, and also the space which is occupied on the retina by the rays which come from each point on the object. For if this space only covers one nerve end, it will excite that thread more violently than if it is spread over several.

The position depends only on the situation of the small parts of the brain where the threads terminate. Distance is recognised by the change in the shape of the lenses in the eye which is altered slightly when more distant objects are being observed. It is also recognised by the relation which the two eyes have to one another, and the angle between the two rays from one object entering each eye. Another way of perceiving distance is by means of the distinctness of the shape and the power of the light, for these two are inversely proportional to the distance. Finally, if we have previous knowledge of the size and shape of an object, we can imagine its distance by comparing this knowledge with the visual impression received. All these means of ascertaining distance are very uncertain, for the sun and moon appear larger when near the horizon than when high in the heavens, although their actual angular diameter is the same. But various objects lie between them and our eyes, and these make us realise their great distance better. Also white and luminous bodies appear larger and nearer than they would do if they were dark, as the light causes the pupil to contract as it would do to observe near objects.

In the seventh discourse the means of perfecting vision

are considered. Vision is concerned with three things : the objects seen, the interior organs which receive the influences of these objects, and the exterior organs which cause the influences to be received as they should be. As regards the objects themselves they can, if accessible, be moved, and the illumination altered in such a way as to render them more visible and distinct. The interior organs are the brains and the nerves, and nothing can be done to them. The exterior organs include the various parts of the eye, and also other bodies which can be put between the eye and the object. Now, there are four points to be provided for by means of these exterior organs : first, that all the rays that converge on each of the ends of the optic nerve should come from one point of the object without change ; second, that the images formed on the retina should be as large as possible ; third, that the rays which form these images should be strong enough to excite the threads of the optic nerve without injuring them ; and fourth, it should be possible to see many objects at the same time. The first of these conditions is approximately fulfilled in the normal eye, but not in the case of long-sighted people, or in short-sighted, such as the very young. To remedy these defects it is necessary to find out the shapes of the surfaces of some transparent body such as glass, which will refract the rays coming through them so that they appear to come from a point nearer or more distant—nearer for those who are short-sighted, or more distant for old people or those who wish to see objects nearer than the shape of their eyes allows. Now, there are a variety of different shapes which will effect this, so we must choose those which are simplest and easiest to cut. Two methods of increasing the size of the images formed on the retina are described. One is by placing in front of the eye a tube filled with water whose refractive index is practically the same as that of the fluids of the eye, and having at the ends a thin piece of glass of the same shape as the exterior surface of the eye. This has the same effect as if the depth of the eye were increased and its pupil made larger. The other way is to put lenses at each end of the tube which would have the same effect as the tube of water, and it is on this that the principle of the telescope depends. To ensure that the rays which move each end of the optic nerve are neither too strong nor too feeble, Nature has given us the power of contracting or expanding the pupils of our eyes. But if still too strong, a slightly obscure body can be interposed or a black one with a narrower hole in it. If too feeble, the illumination on the body from which the rays come may be increased by means of mirrors or lenses.

Another way is to use an arrangement of lenses to make

the object appear nearer or more distant. Nature has provided all that is necessary for us to see as many objects as possible at the same time, and if, by artificial means, we try to see some object more distinctly, the other objects will appear more confused. Although the shape of the crystalline lens and the size of the pupil cannot be sufficiently changed, these defects can be gradually corrected by usage, for the muscles controlling them can be strengthened by exercise.

The eighth discourse deals with the shapes which transparent bodies must have in order to refract rays in all ways which are useful to sight. The shapes considered are made up of ellipses, hyperbolas, circles, and straight lines. The properties of an ellipse are first described, and it is shown geometrically using the law of refraction that lenses bounded by elliptical and spherical surfaces only, can be made which will cause divergent, convergent, and parallel rays to change from one to another of these three kinds of arrangement in all imaginable ways. The hyperbola is then described, and it is again shown that divergent rays may be made parallel or convergent, parallel rays may be made to diverge or converge, and convergent rays may be rendered parallel or divergent, by means of lenses whose surfaces are composed of hyperbolas and straight lines. A detailed discussion then follows of the differences between these two classes of lenses. Those whose surface is formed from a hyperbola and a straight line are in most respects superior to those formed from an ellipse and a circle. The former are easier to cut, one of them sometimes has the same effect as two of the latter, but they focus the rays coming from different points in as many different points less accurately. The conclusion is therefore drawn that the hyperbolic and the elliptical lenses are preferable to all others, and that on the whole the hyperbolic are to be preferred to the elliptical.

The ninth discourse contains a description of various kinds of optical instruments with the arrangement and shapes of the lenses. The transparent material used should be easy to cut, durable, and very transparent and colourless. Glass made of very pure alkali is the best medium, as rock crystal, although it seems clearer, causes more reflection at its surface. This is explained as due to the fact that the solid particles of crystal are coarser than those of glass and its pores are more closed, and consequently fewer rays can pass through it. To correct short-sight, a concave-convex lens is recommended, both surfaces being traced by hyperbolas, the one nearest the eye being concave and having a shorter focal distance than the other surface away from the eye. To correct long sight the same kind of lens can be used, but the concave surface next

the eye should have a longer focal distance than the convex surface away from the eye. A magnifying-glass for observing near objects is next described. This consists of a concave mirror which focusses the sun's rays on to the object to be observed; from there the rays pass through a plano-convex lens set in the mirror, to the eye, the convex surface being away from the eye and being traced by a hyperbola whose focus is at the point where the object is fixed. A description of a telescope for viewing stars or other very distant objects is next given. It consists of two hyperbolic lenses mounted at the ends of a tube. The eye-piece is a double-concave lens, the surface next the eye being traced by a hyperbola so that the distance from its focus to the eye is that of most distinct vision. The other hyperbolic surface focusses the rays on to the retina. The objective lens is plano-convex, the flat surface facing the object of vision, and the convex surface being formed from a hyperbola whose focus is on the retina. The tube should (by a telescopic arrangement) be capable of being made longer or shorter, to allow for the different optical qualities of different people's eyes. At the side of this tube, two similar tubes with similar but less powerful lenses should be fixed, so that they are directed to exactly the same spot as the first tube. Having a lower magnification, they are more convenient to spot the object required and to determine its relation to objects near it. Sights are also placed on the outside of the tubes, and the whole is mounted on a stand so as to arrange for rotating and altering the elevation. Descartes is curiously sanguine about this invention: he says that if the hand of the artificer does not fail us, we ought to be able to see objects in the stars as small as those we usually see on this earth! He next explains how this telescope may be modified so that it can be used to magnify the size of very near objects much better than the arrangement previously described. The only alteration in the telescope itself is to make the objective lens small and its flat surface hyperbolic with a very short focus at which the object is placed. Also, at the end of the tube a parabolic mirror is fixed to focus the sun's rays on to the object. This is mounted on a black substance, and completely closes the hole in this black body. If the object is at all transparent a small lens, the same size as the objective, can be placed between the sun and the object, to focus the sun's rays directly on to it. The defects hitherto noticed in optical instruments are attributed to the use of lenses with spherical surfaces instead of hyperbolic. Occasionally good results have been obtained when by a happy error the spherical surface has approached the hyperbolic. In view of the difficulty experienced in grinding lenses with hyperbolic surfaces, a machine is described by which

these may be conveniently produced. In the tenth discourse a full account of this invention is given, but before cutting the glass it is necessary to find its refractive index, so Descartes devised the following very interesting method.

A pencil of rays AL passes through a prism RFP made of the transparent material whose refractive index is required. AL is at right angles to RF, and since the angle F of the prism is also a right angle, FI is parallel to AL. The angle R of the prism is less than the angle P. The light is not refracted at L, being normal to the surface RF, but it is refracted at B, in the surface RP and meets FP produced at the point I. The positions of the points B, P, I are then carefully marked on a sheet of paper. Then with centre B and radius BP an arc of a circle TPN is described cutting BI in T. The arc PN is made equal to the arc PT, and BN are joined by a straight

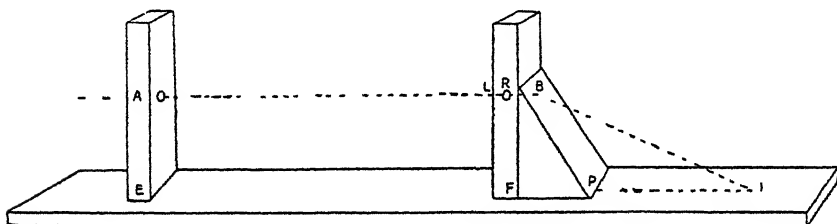


FIG. 3.

line cutting IP produced in H. Then from BI a part BO is cut off equal to BH. The ratio  $\frac{HI}{OI}$  gives the refractive index of the material of which the prism is composed. The proof (which Descartes does not give) may be shown as follows :

$$\frac{HI}{OI} = \frac{\sin HOI}{\sin OHI} - \frac{\sin BOH}{\sin \alpha} - \frac{\sin \beta}{\sin \alpha} \text{ and}$$

$\alpha$  and  $\beta$  are the angles of incidence and refraction.

Descartes then goes on to point out how this figure may be used to obtain the hyperbola required to give the lens its proper shape. For if IM be made equal to OM and HM bisected at the point D, then H and I are the foci of the hyperbola and D is its summit. To obtain larger or smaller hyperbolas, triangles similar to BHI may be used.

In concluding the discourse the opinion is expressed that the microscope will prove the most useful of optical instruments, as it will give us an insight into the composition and structure of the materials comprising animal and vegetable

substances whose essence lies in the shape, size, arrangement, and movements of their particles.

The second essay entitled "*Les Météores*" is an attempt to explain all kinds of weather phenomena on the principles already laid down. Descartes asserts that we have more admiration for what is above us than for what is at our own level or beneath us, and that is why poets and others describe the clouds as God's throne, and imagine that He uses His own hands to open and close the gates of the wind and to pour out the rain.

He hopes to explain the nature and causes of all these things, and to show that they depend on certain general principles of Nature which hitherto have not been adequately explained. First, the nature of terrestrial bodies is discussed;

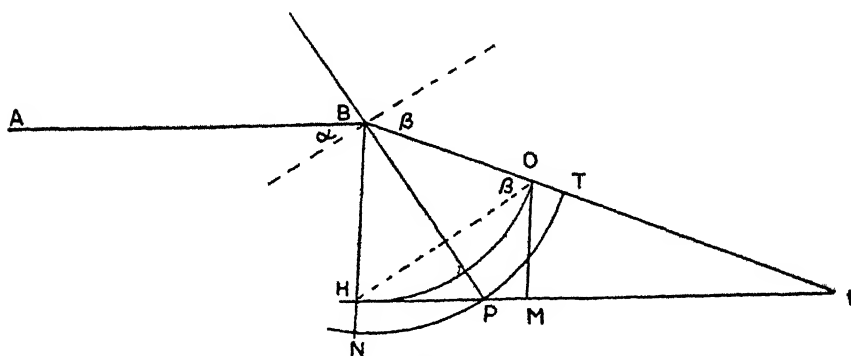


FIG. 4.

then the vapours which arise from some of these. The formation of clouds and winds follows, and the way in which the clouds dissolve to form rain, hail, and snow. Next, the causes of tempests, thunder, and lightning are dealt with, and, finally, the effects of light which causes rainbows and other luminous phenomena in the heavens. Water, air, and other terrestrial bodies are all composed of small particles, and between these are interstices filled with every "subtile matter." The particles of water are long, smooth, and compact, those of hard substances are interlaced and joined together and have irregular shapes. If the particles are smaller and less interwoven, they can be more agitated by the particles of the subtile matter which is always moving, and so they form air or oils. A very interesting definition of heat occurs here, which is very much in advance of the ideas current at this time and resembles those formulated in the nineteenth century: Heat and cold are due to the greater or less agitation of the particles (whether due to the subtile matter or any other cause), and in agitating the

extremities of the nerves which are the organs of touch, they give us the sensation of hotness or coldness. If the agitation is greater than usual, they feel hot; if less, they feel cool. A little farther on he says, "Heat is the agitation of the small particles of which terrestrial bodies are composed."

The freezing of water is likened to a tank full of live eels being converted into one full of dead eels. A curious mistake is made in the statement that water which has been heated for a long time freezes more readily than other water, and the reason given is that the more volatile and active particles are evaporated during the heating.

The particles of salt are like cylinders or sticks, those of water like string, and not so large or heavy. From these assumptions the properties of salt and the differences between salt water and fresh water are deduced. The winds are caused by vapours which are drawn upwards by the sun from terrestrial substances. The vapours come from water and the exhalations from other substances. Clouds are composed of drops of water or little bits of ice. These drops are formed by the coalescence of small particles of vapour, and are quite round unless their shape is altered by the wind. When their size is so large that the resistance of the air cannot sustain them, they fall as rain, or as snow if the air is not warm enough to melt them, or as hail if after being melted they meet a colder wind which freezes them. Winds are caused not only by the clouds being dissolved into vapour, but sometimes also the clouds suddenly fall and drive away the air which is under them with such violence as to cause a tempest. Thunder is similarly attributed to one cloud suddenly falling on another one beneath it, and lightning to the presence of inflammable exhalations between the two clouds.

The eighth discourse contains a more complete explanation of the rainbow than any which had previously been published. Newton refers to it in Book I, Part II, of his *Opticks*. He says that it had been attributed by some of the ancients to the refraction and reflection of the sun's rays by falling drops of water (*e.g.* Theodoric, *c.* 1311). It was more fully discussed by de Dominis in a book published in 1611. De Dominis taught that the interior bow is made by two refractions and one reflection in round drops of rain, and the exterior by two refractions and two reflections between them in each drop of water. He proved this by experiments made with a phial of water and with globes of glass filled with water and placed in the sun. Descartes pursued the same explanation and improved that of the exterior bow. But as these men did not understand the true origin of colours, Newton considered it necessary to pursue the matter a little further. Even he did

not provide a complete explanation ; that was left for Young and Potter and Airy during the nineteenth century, when the principle of interference was understood.

Biot remarked that the only thing wanting in Descartes' explanation was a knowledge of the unequal refrangibility of light, but this statement is a little too optimistic.

Unfortunately Descartes, in this explanation as in so many others, does not allude to the work of previous investigators of which he must have been aware. Apparently he repeats the experiment of de Dominis, using a large glass phial filled with water, and he observes the colour effect when the ray from the phial to his eye makes an angle of about  $42^\circ$  with the ray from the sun, and also a less vivid effect when the angle is about  $52^\circ$ . He traces the path of the rays through the water, and then repeats the experiment, using a crystal prism instead of the flask. From this he concludes that it is neither the curved surface nor the size of the incident angle, nor the number of refractions or reflections, which produce the colour. But the two surfaces through which the ray passes must not be parallel. He next gives a more detailed explanation of his theory of colour than that given in the *Dioptrics*. Conceiving light to be the action or the movement of a very subtle kind of matter, of which the particles are like little balls which roll in the pores of substances, the rolling may take place in different ways. Ordinarily, *i.e.* with white light, this turning movement has the same force as the movement straight forward, but when the particles meet a surface obliquely, some of them acquire an increase in the rotatory movement and some of them a decrease. Consequently they diverge, those which have the greatest increase in rotatory movement diverge the least and produce the sensation of redness, while those whose rotatory movement is most reduced, diverge the most and produce the sensation of violet light. Next, returning to the rainbow, he shows by calculation that of all the rays which fall on a drop of water and are there refracted twice and reflected once, a greater number will come to our eyes making an angle of  $41^\circ$  or  $42^\circ$  than at any other angle. Similarly for those rays which are refracted twice and reflected twice, more rays enter the eye at an angle of  $51^\circ$  to  $52^\circ$  than at any other. He concludes that the largest angle which the radius of the interior (primary) rainbow can subtend at the eye is  $41^\circ 47'$ , and for the exterior rainbow the smallest angle is  $51^\circ 37'$ . If the water is warm its refraction is a little less, and the radius of the primary rainbow will be increased by one or two degrees at the most, while that of the secondary will be diminished. And since the drops of water produce the same colour effect as the prism, the outside of the primary rainbow will be red

and the inside of the secondary also red. The appearance of an inverted rainbow above the smooth surface of a sheet of water is attributed to the sun's rays being reflected from this surface to the drops of rain, the direct rays being cut off by some intervening cloud. A third rainbow may appear above the second, and this is due to very round and transparent hail among the rain, which, having a greater refraction than water, makes the bow have a larger diameter and appear above the others. The ninth and tenth discourses explain the colour of the clouds, and of the circles or crowns sometimes seen round the stars, and the appearance of several suns in the clouds. These last-named are caused by the refraction and reflection of the sun's rays at the surface of a round cloud made up of snowflakes, those on the side towards the sun having been melted and again frozen.

Some of Descartes' letters show that he knew that the air had weight and that he sought to determine this weight. He realised why the mercury remained at a certain height in Torricelli's tube, and Tannery states that Descartes suggested the famous Puy de Dôme experiment to Pascal, knowing that it was crucial. Tannery concludes that one of the greatest experimental discoveries in Physics of the seventeenth century should be traced back to Descartes' genius.

In conclusion, the general physical theory of Descartes, as stated in his *Principles of Philosophy*, is extraordinarily comprehensive. Its aim is to trace the genesis and development of the existing universe from principles easily understood and according to the laws of the transmission of motion. Instead of attacking particular points of the Aristotelian doctrine as Galileo did, Descartes laid down general principles. His error was in constructing *a priori* hypotheses without the necessary experimental verification. The laws of nature dealing with the impact of bodies are incorrect, but those dealing with inertia and curvilinear motion follow from Descartes' geometrical analysis, although they were not clearly understood by even Kepler and Galileo. His *a priori* doctrines of the constitution of matter have been confirmed by the modern work on the electron theory, and by the results of spectrum analysis with regard to the composition of the sun and the stars. The vortex theory is one of the grandest hypotheses which have ever been imagined to account for the movements of the universe by mechanical means. It goes much further than the Newtonian hypothesis, for it attempts to explain why the planets move and not merely how they move. It certainly assumes a simplicity in the phenomena which is not present, but that is a necessary step in solving the complex problems of Nature. It banishes the occult.

His optical work is perhaps Descartes' greatest contribution to Physics, and here his aim was a practical one ; viz. to improve the telescope. The construction of lenses was the only practical purpose which he pursued with any ardour. The laws of reflection and refraction were necessary consequences of his *a priori* hypothesis on the nature of light which approximates to the wave theory and may have inspired Huygens. The phenomena of light, heat, magnetism, and gravity are all explained by means of the movements of the particles of matter, and even Chemistry and Biology are included in the science of Physics and reduced to a problem of mechanism. The clue is found in a letter to Mersenne, where he says, " My Physics is nothing but Geometry." Since modern science tends to become more and more mathematical, it is therefore no exaggeration to say, " Plus la science marche, plus elle se rapproche de Descartes."

## ELEMENTS WHOSE EXISTENCE HAS BEEN ANNOUNCED, BUT WHICH ARE NOT RECOGNISED

By J. G. F. DRUCE, M.Sc. (Lond.), R.Nat.Dr.(Prague), F.I.C.

DURING the last century a number of elements were discovered, and the discoveries have been well substantiated. On the other hand, many new elements have been reported, but their existence has not been established, or the supposed new bodies have been found, on further examination, to be identical with previously known substances.

In a few cases it is possible that the "discoverers" were guilty of a fraud or hoax. In others they were themselves deceived. But it is quite possible that in some cases in which a new element has been reported the matter has not been as fully investigated as might be desired. There have been, for instance, many reports of a new element in zirconium minerals, but it was left to Coster and Hevesy in 1922 to prove the existence of hafnium, which they were able to isolate from these sources.

The rare earth minerals have been a most fruitful source of new elements, including those whose existence has been established and those which remain unconfirmed, or which have been shown to be mixtures, or bodies already known. The difficulties encountered during an examination of rare earth minerals are usually great. The products consist of oxides or salts of metals, which are very similar in their properties. Separations involve long series of fractional crystallisations or precipitations.

Considerable doubt has been cast upon the existence of many of the so-called elements, since there is no place for them in the Periodic Classification. In one instance, however, that of the inert gases, there was no foundation for this conclusion. Mendělejev [1] did not make provision for these undiscovered elements of the zero group.

Moseley's work [2], about twelve years ago, has resulted in a surer guide than the Periodic Law being available for the identification of the elements. He showed that the K spectrum of each element contained two strong lines from the position

of which it is possible to calculate the "atomic number" of the element. It affords a means of checking other evidence. Thus coronium, nebulium, and asterium have been claimed as elements on "spectroscopic evidence"—*i.e.* on certain lines observed in the spectra of heavenly bodies. They had been assigned places in the Periodic Table between hydrogen and lithium; but if present views are correct, there is no vacant position for them there.

The atomic number of uranium, the heaviest element, has been found to be 92. Assuming that there are no more beyond it, few elements remain to be discovered. Prof. B. Smith Hopkins recently claimed to have discovered the hitherto unknown rare earth metal of atomic number 61. Number 75, Mendělejev's dvi-manganese, was discovered last year [3], and his eka-manganese (43) has been claimed to have been found in platinum ores [4]. There remain eka-iodine (85) and eka-cæsium (87) undiscovered [5].

Since Prout's [6] hypothesis was advanced in 1815 to account for the evolution of the elements from a primordial source, there have been many attempts [7] to show that hydrogen is the basis of the structure of the other elements.

Attempts to account for the atomic weight of hydrogen being in excess of unity led Hinrichs [8] to regard the excess as the exact atomic weight of an hypothetical element, pantogen. The union of pantogen atoms in cubes, prisms, etc., was considered to give atoms of the other elements, *e.g.* oxygen, nitrogen, fluorine, etc. From the number of pantogen atoms forming the atom of the new element so made, the latter's atomic weight may be calculated. This hypothesis did not meet with general acceptance among scientists, but Prout's hypothesis has been revived from time to time in a modified form.

#### ELEMENTS SAID TO HAVE BEEN IDENTIFIED BY SPECTRUM ANALYSIS

There are certain lines in the spectra of nebulae which have not been associated with any known terrestrial element. These are sometimes regarded as belonging to Archonium (*Handbuch der Spektroskopie*, von H. Kayser and H. Konen), a purely theoretical element, whose atomic weight is nevertheless given as about 2.947. In the memoirs of Fabry and his co-workers [9] it is called nebulium.

Huggins [10] also discussed the significance of the line 5571, which has been attributed to aurorium, and Sir N. Lockyer also investigated the spectra of the corona [11].

Nasini [12] stated that he had obtained a bright line 531.5, corresponding with one attributed to coronium, in his study

of the gases from Pozzuoli, Grotto del Cane, and Vesuvius. It was pointed out that coronium would have a vapour density far below that of hydrogen.

In 1895 Runge and Paschen [13] found lines in the helium spectrum that could be divided into two sets, and they suggested that one set belonged to par-helium. It was afterwards shown that pressure changes produced the same phenomenon, and no other element was present.

#### ELEMENTS BELIEVED TO HAVE BEEN FOUND IN RARE EARTHS

In the course of researches on monazite sand, Barrière [14] appeared to have found a new element which he called lucium. Its oxide was patented for use in making incandescent gas mantles in opposition to the rare earths used by Auer von Welsbach. It was said to be distinguished from cerium, lanthanum, and didymium by the solubility of its double sodium sulphate. Further, while thorium and zirconium form insoluble double potassium sulphates, lucium did not; and whilst yttrium, ytterbium, and erbium are not precipitated by sodium thiosulphate, lucium chloride was. It was distinguished from beryllium by the fact that its solutions were precipitated by oxalic acid. Lucium oxide dissolved in sulphuric acid, nitric and acetic acids forming salts, either white or slightly pink. All were soluble in water. The spectrum was said to be characteristic, corresponding only slightly to that of erbium. Its atomic weight was given as 104.

The subject was carefully investigated by a number of distinguished chemists, including Schutzenberger, Cleve, Fresenius, and Lecoq de Boisbaudran [15]. Sir W. Crookes [16] expressed the view that the claims for its elementary character were not justified. Spectroscopic examination showed that it contained didymium and erbium. All the lines of the spectrum could be accounted for as belonging to elements already known. Shapleigh could not detect any lucium in monazite from North Carolina [17].

In 1846 Rose announced the discovery of an alleged new element, pelopium, in Bavarian Tantalite. His pelopic acid he found on further examination to be convertible into niobic acid, and thus no new element was present.

Similarly ilmenium was announced by Hermann [18] as being present in ilmenite, but its oxide was ultimately shown to consist of niobium oxide, mixed with a third its weight of tantalum oxide.

In a lengthy paper in 1877, Hermann collected the evidence in favour of the existence of ilmenium. He stated that this element was formed as a black powder. Its oxide had the

formula  $\text{U}_2\text{O}_5$ . Other compounds were described, and analyses were given.

In the course of this work he investigated columbite and ferro-ilmenite from Connecticut, and came across what he considered still another element, neptunium. This was isolated by dissolving the hydroxides in a mixture of hydrofluoric acid and potassium fluoride. Addition of water precipitated potassium tantalum fluoride. Recrystallisation of the double fluorides from the mother liquor separated the more soluble potassium neptunium fluoride. When a solution of this was treated with sodium hydroxide it was said to give a precipitate of sodium neptunate. The new element was characterised by giving a wine-yellow coloured bead with microcosmic salt.

Aridium was the name given by Ullgren [19] in 1850 to an apparently new metal. It was isolated from certain chrome iron ores by a rather lengthy process of extraction resulting in the separation of a brown oily precipitate of the sulphate, which eventually crystallised. Its oxide dissolved in hydrochloric acid, without evolution of chlorine, and gave an uncrystallisable chloride. Hydrogen sulphide reduced the higher oxide to a lower one, solutions of which gave a grey precipitate with ammonia. The precipitate turned brown on exposure to air. It was shown that aridium was a mixture of iron and chromium phosphates.

A new element, donarium, was claimed to be present, according to Bergmann [20], in orangeite from Brevig, Norway. Orangeite is yellowish red, and has a density of 5.34 to 5.40, and Bergmann supposed it to contain chiefly  $\text{D}_2\text{O}_3$ , resembling zirconium oxide in properties. It was not completely reduced by hot hydrogen, but this was effected by potassium, rapidly, and with the emission of light. The element burned to a red oxide, was insoluble in hydrochloric acid, but easily formed a sulphate, which gave the hydroxide when treated with alkalis. Damour [21] and Berlin showed it to be identical with thorium, but this was disputed by Bergmann.

Danium was the name given by von Kobell [22] to a body obtained from Finnish tantalite ores, but it was found that the so-called dianic acid was a modification of niobic acid.

Bahr [23] gave the name wasium to what he thought was an element, but which was shown by Nicklès [24] to be a mixture of yttrium, tellurium, and didymium.

That a new element, mosandrum, occurred in gadolinite was held by Smith [25], who gave an account of the evidence in favour of his contention. This was not, however, accepted.

Norwegium was announced by Dahll [27] and Prochalka [28] stated that it was present in American lead, but it was

afterwards found to be impure bismuth. Another element to be discovered and abandoned was rogerium [29].

Decipium was the name given by Delafontaine [30] to what he believed was an individual body in samarskite, but what were described as its salts are now regarded as mixtures of those of several of the rare earth metals.

At the time Delafontaine noted that its compounds resembled those of other rare earths. He determined its equivalent as about 122, and considered the oxide to have the formula  $\text{DpO}$  or  $\text{Dp}_2\text{O}_3$ . He was unable to separate it completely from didymium. Nevertheless, its absorption spectrum was studied.

Vesbium was found, according to Scacchi [31], in the 1631 lava of Vesuvius.

Its oxide was acidic, and formed salts with alkalis, giving yellow solutions, which yielded precipitates of various colours when mixed with other metal salt solutions. The silver precipitate was yellow-red, that of zinc was green.

Idunium was described as an element by Websky [32], who isolated it from zinciferous lead vanadate occurring in La Plata. On treating the vanadic acid obtained from this source with ammonium chloride the ammonium vanadate separated. On addition of ammonium sulphide to the mother liquor, red idunic oxide was precipitated.

Austrium, which Linnemann [33] separated from orthite in 1886, was shown to be identical with de Boisbaudran's gallium [34].

Russium was reported (1889) as a new metal in monazite by Chroustschoff [35], who regarded it as one of the missing elements predicted by Mendélejev. Its properties approximated to those of thorium.

During his investigations on the atomic weight of tellurium, Brauner [36] believed he had found a higher homologue of atomic weight 214, to which he assigned the name Austriacum. Later he modified his claims. Flint [37] also commenced a series of hydrolytic fractionations of tellurium compounds, and believed the end products were different. To one variety he gave the atomic weight 124.3. Kothner [38] and Staudenmaier [39], however, concluded that tellurium was elementary. On the other hand, Grünwald asserted that the assumed elements tellurium, antimony, and copper contain traces of eka-tellurium. He gave the wave-lengths of 16 rays observed in the ultra-violet. Its atomic weight was given as about 212, and was supposed to agree closely with Brauner's Austriacum.

Richmond and Hussein Off [40] stated in 1892 that the Egyptian mineral, masrite or johnsonite, a fibrous alum, contained a new element, masrium. Assuming it to be bivalent, analysis of the oxalate gave it an atomic weight of 228, and the

authors pointed out that there was a vacancy in the second group of the periodic system for an element of atomic weight 225. A complete account of the analysis was given and pointed to a 0.2 per cent. content of masric oxide. Its sulphate was assigned the formula  $\text{MsSO}_4 \cdot 8\text{H}_2\text{O}$ , and crystallised from 50 per cent. alcohol very well. It formed an alum. Other reactions were indicated.

Chroustschoff [41] in 1897 announced the discovery of an alleged element, glauko-dymium, in didymium.

To account for the close resemblance in properties shown by the rare earths, Crookes [42] put forward the idea that they were Meta-elements. As a result of thousands of fractional crystallisations and partial decompositions of mixed nitrates, he thought he had isolated an element, victorium, sometimes referred to as monium [43]. It was associated with yttrium in crude yttria. Crookes described some of its salts and its phosphorescent spectrum. Its oxide was considered to have the formula,  $\text{V}_2\text{O}_3$ , and its atomic weight was supposed to be in the neighbourhood of 117. Marc [44] regarded it as a mixture, as a result of an exhaustive examination of its phosphorescent spectrum.

In 1900 and 1903 Baskerville [45] announced the separation of what he considered new elements, carolinium and berzelium, from radioactive ores.

Whilst assaying copper carbonate ore from Frazer Claims, British Columbia, Courtis [46] obtained 160 milligrams of what was stated to be a new element, amarillium. All was accidentally lost except 14 mg., on which tests were made. The discoverer expected to receive large quantities of the ore which would furnish further results. Nothing more has been heard of amarillium, so that either the supplies were not forthcoming or they contained nothing new.

#### ELEMENTS REPORTED IN ZIRCON MINERALS

A particular interest now attaches to the investigations of zircon minerals since Coster and Hevesy's isolation of hafnium in 1922 [47]. There have been frequent announcements of the detection of new elements in zircons since the discovery of zirconium by Klaproth in 1789. Most of these have been made to account for special reactions which were considered not characteristic of zirconium. According to Coster and Hevesy, however, hafnium shows very similar chemical properties to zirconium. Thus in 1845 Svanberg [48], who noticed the great variations in the composition of zirconia, stated that in decomposing zircons there was another earth, noria, present, which differed in the solubility of its salts, and

also had a lower atomic weight. This he gave as 1100 to 1150 (oxygen=100), and he called the new element norium.

In 1853 Sjogren also claimed that he had found norium in catapleiite by precipitating the solution with potassium ferrocyanide. The density of the oxide was 5.5, whilst that of zirconia is 4.3. Knop [49] showed that there was a difference in the composition of the phosphates of zirconium and norium. He also mentioned certain dry-test distinctions between them.

Berlin [50], however, showed that the results of a fractional precipitation of ordinary zirconium chloride did not warrant the conclusion that zirconia was a mixture. He and Hermann [51] declared norium to be non-existent. Hermann investigated the density of zirconium and its oxide. He also determined the composition of the sulphate, and its behaviour towards potassium sulphate. He found no evidence to suggest the presence of a new element. Marignac [52] also disputed its existence. From a study of a large number of double zirconium fluorides he concluded that Svanberg had made a mistake.

In 1864 Nylander [53] again reported the existence of two earths in zirconia, but this was not definitely confirmed, except that Sorby [54] claimed to have found a new element, jargonium, in jargon, or jargoon, from Ceylon. Later he expressed the opinion that he had mistaken it for uranium. Similarly Church [55] concluded from a spectroscopical examination that zirconium was accompanied by a small quantity of a new element, nigrium, so named from its black absorption bands.

More recently Ogawa [56] claimed that he had found nipponium in thorianite, molybdenite and reinite. Its equivalent was given as 50, and therefore its atomic weight was about 100, and it probably filled the gap between molybdenum and ruthenium. It was stated to have two oxides, the higher being acidic, and was reduced to the lower basic one by zinc and hydrochloric acid. It formed an anhydrous chloride, soluble in water to a pale green solution, and gave a characteristic line 4882 and feebler ones. Fusion with sodium carbonate and potassium nitrate gave a green mass. When carbon dioxide was passed into the solution it gave a brown precipitate, readily soluble in hydrochloric acid. The aqueous solution of the fused mass gave no precipitate with ammonia or ammonium sulphide, but addition of hydrochloric acid to the ammonium sulphide solution gave a brown precipitate. Ogawa pointed out that his nipponium was apparently identical with the alleged element in the tin group, traces of which were found by Miss Evans [57] in 1908 in thorianite.

Loew [58] also claimed to have obtained a nameless earth in American zirconia, and Hoffmann and Prandtl [59] also

stated that about half the zirconia extracted from euxenite consisted of an element of higher atomic weight, but Hauser and Wirth failed to obtain the characteristic reactions reported for it in twelve different samples of euxenite [60].

#### ELEMENTS REPORTED IN PLATINUM MINERALS

Ores of the noble metals have been a fruitful source of unconfirmed announcements of new elements. In 1828 pluranium was announced by Osann [61], but he afterwards found it was a mixture of titanium, zirconium, and silicon; and at the same time he reported a new body, polinium. This was later shown to be impure iridium.

Genth [62], in 1853, separated two grains of a white metal resembling platinum from Californian iridosmine and platinum. Qualitative tests showed that it possessed properties unlike those of any known element. No quantitative analyses were possible. In 1862 Chandler [63] found something which might be identical with Genth's metal in native platinum from Oregon. It was malleable, fused on charcoal in a blowpipe flame, giving a black oxide. The borax bead was colourless. The metal dissolved in hot hydrochloric or nitric acids. Its sulphide dissolved in hydrochloric acid in the presence of potassium chlorate.

Davyum was the name given by Kern [64] to a body isolated from platinum residues, and considered by him to be elementary. Mallet [65], however, showed it to be a mixture of iridium, rhodium, and iron.

Uralium was announced in Russian platinum by Guyard [66] in 1879, who stated he had found it ten years previously. Next to silver, it was the whitest metal known. It was softer than platinum, and had an atomic weight of 187.5. It gave an orange melt with potassium cyanide. It was as malleable as platinum, and much more ductile; its density was given as 20.25. In its chemical properties it was difficult to distinguish from platinum.

Another alleged element from similar sources was Wilm's nameless metal [67]. French stated that he had separated a beautiful white metal, canadium, which occurred in the form of semi-crystalline grains or short rods. It did not tarnish. It was distinguished from other platinum metals, silver, etc., by various reactions [68]. In the *Annual Report* (1912) of the British Columbia Minister of Mines it stated that authentic samples of materials in which the presence of platinum metals and canadium was reported had been sent to representative assayers, to the Canadian Geological Survey, and to the British Columbia Government laboratory. In no case were any traces

of platinum metals detected, nor was there any evidence of the presence of a new element.

In the early part of the last century a number of elements were reported in cobalt and nickel ores. These included Richter's (1805) niccolanium, which was shown to contain nickel, cobalt, arsenic, and iron. Vestium (or Sirium) was alleged to have been discovered in nickel ore by Vest (1818), but this proved to be a mixture containing nickel, sulphur, and arsenic. Lampadius's wodianum (1820) from cobalt ore was found to be composed of nickel, arsenic, and other known elements.

In 1820 Trommsdorf thought he had found a new element, crodonium, in a sulphuric acid incrustation, but he later found it was composed of calcium and magnesium salts with copper and iron as impurities.

Lavoesium was the name assigned by Prat [69] from spectroscopic evidence to what he thought was a widely distributed new element. He named it after Lavoisier, and described it as being silver-white, malleable, fusible, and forming crystalline salts, solutions of which gave characteristic reactions. It also gave distinctive absorption bands. Its alleged occurrence in pyrites should have made it plentiful, but nothing further has been heard of it.

Radioactive changes have naturally led to reports of [70] fresh isolations of elements, but the transitory ones, *e.g.* brevium, etc., are usually designated by a different nomenclature.

#### ANNOUNCEMENTS OF UNSPECIFIED NEW EARTHS AND NEW ELEMENTS

Under the title "A New Earth of the Cerium Group, with Remarks on a Method for Analysis of Natural Columbates," Smith [71] described the method of decomposing columbates with hydrofluoric acid and separated what he supposed was a new earth, the element having an atomic weight of 109. This earth differed from the yttria group by its behaviour with potassium sulphate, from ceria by its solubility in very dilute nitric acid, from didymia by its colour and by its absence of absorption bands. It was also shown to differ from lanthana.

B. W. Gerland [72] in 1878 described a mineral found in a special sandstone. It contained about half the known elements and also a new member of the rare earth group. The solution was precipitated with sodium hydroxide, excess of which dissolved the hydroxide of the new substance, but this could be reprecipitated with carbon dioxide. The dried residue melted at red heat. It did not give the reactions for aluminium, but was said to show characteristic properties of a new body.

In 1896 Kosman [73] claimed the discovery of two new elements to which he gave the names kosmium and neokosmium. Although patented, no description of them was forthcoming. Winckler [74], the discoverer of Germanium, treated this claim with scorn in a presidential address to the Berlin Chemical Society. He pointed out that the names were not derived from Kosmos, but from Kosman. It might have been an April 1 joke, except for the cost of the patent!

Demonium was the name given to an element in a rare earth by Rowland [75], on account of the trouble he had had in separating it.

From a qualitative examination, Bayer [76] thought he had found a new element in a French bauxite, but apparently did not pursue the subject.

After removing all known elements from thorianite, Miss Evans [77] considered there was distinct evidence of the existence of a new element, probably belonging to the tin group. Skrobal and Artmann [78] pointed out that this might coincide with the alleged element detected in cast iron by Bouchier [79] and in steel by Ruddock [80], and by themselves in ferro-vanadium. At first Skrobal and Artmann believed that their body was molybdenum, but concluded that it must be a new element by its reactions. They intimated that the subject was still under investigation, but published no more upon it. Bouchier's product was obtained by treating the iron with dilute sulphuric acid, and saturating with hydrogen sulphide. Precipitate and undissolved matter were dissolved in hydrochloric acid with potassium chlorate. After removing arsenic and copper, a dark brown sulphide, soluble in potassium hydroxide, was obtained. The metal was isolated by reduction as a black powder and constituted from .0019 to .006 per cent. of the iron. It was insoluble in dilute acids, but readily in *aqua regia*. The yellow oxide was volatile. Jones [81] contended that the element was identical with molybdenum, which had apparently not been removed during the treatment of separation.

It is almost certain that many other announcements of new oxides or new elements have been made, but it is believed that this article makes reference to the large majority of alleged discoveries which have not been confirmed.

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## POPULAR SCIENCE

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### LOCKE ON SEVENTEENTH-CENTURY SCIENCE

By JOSHUA C. GREGORY, B.Sc.

WHEN Sir Francis Bacon wrote that "learners owe to their masters only a temporary belief," admonished the pupil to suspend his own judgment, and cautioned the teacher to claim no "perpetual captivity,"<sup>1</sup> he expressed the true relation between pupil and master, or between successive generations, in a sentence. Perhaps an echo from Bacon sounded in John Locke's "learners must at first be believers."<sup>2</sup> Similarity of notion, rather than borrowing of thought, may prevail in these two quotations, but Locke more manifestly appropriated thoughts from his age in his *Elements of Natural Philosophy*. Locke was born in 1632 and died in 1704; no reflectively appropriative mind could write on this theme when he did as if Descartes, Gassendi, Boyle, Sir Isaac Newton, and others, had not reaped where their predecessors had sown. Leibniz was insisting on *monads* and discerning in each a peculiar mirror of the whole universe. In Locke's *Elements of Natural Philosophy* the scientific thought of his period is effectively reflected, though in his own personal perspective. Thought is ever on the wing—in the single mind or in the collective mind that may be conveniently, even if untruly, assigned to an epoch. Historical retrospect can distinguish phases in the flow of thought through all the individual versions that sometimes seem to make a jumble of a period. Though Locke wrote shortly in his *Elements of Natural Philosophy*, and the total of the words is small, he provided the modern student with one interesting glimpse into the seventeenth-century manner of thinking physical reality. A context is available in the writings of his contemporaries and in those of his predecessors. The modern reader who cares for the past can, with the aid of this context, see the material world as Locke saw

<sup>1</sup> Bacon, *Adv. Learn.*, i.

<sup>2</sup> Locke, *The Conduct of the Understanding*, The Works of John Locke, in 9 vols., 12th ed., ii, 1824, p. 371.

it, and as the mind of his age directed him to see it, in this miniature treatise.

During every epoch old ideas, ripe for discarding, linger in the company of new ideas that will finally discard them. Copper grew lustily in Cyprus, if legend did not lie. The *De Mirabilibus Auscultationibus* is included in the Aristotelian *corpus*. Though Aristotle was not its author, it contains a revelation of past opinion and belief. It records, perhaps with hesitating credence, one ancient tradition. The men of Cyprus were said to cut up copper, as the modern allotment owner cuts up a potato, plant the pieces, and gather a harvest when the rains had fallen. Others also reported even stranger doings at Philippi, for the refuses from some mines there were said to increase and produce gold.<sup>1</sup> In the seventeenth century Boyle still hesitated over the appearance of "metalline particles of tin" in piles of earth.<sup>2</sup> Earlier in the century Bacon was doubtful about Cyprus crops of metal, which he called "a kind of iron," but lead certainly multiplied and increased, as was "known of old." When "old statues of stone" were left in cellars with their feet bound in leaden bands the metal swelled and "hanged upon the stone like warts."<sup>3</sup> Boyle surmised two possible sources for the "growth of metals"—using the phrase in a "lax and popular sense." The air might be responsible; an internal disposition might, alternatively, cause the "metalline increment." Very significantly he compared such presumed "internal disposition" to a "metalline seed or ferment."<sup>4</sup> Tradition had been insistent on the "seeds" of metals. In the seventeenth century Nicolas Lemery still chided alchemists for covetousness and a continual hope of making gold. Some of them, he complained, searched through gold for its elusive "seed," as gardeners might try to pick seeds out of vegetables.<sup>5</sup> Such literal comparison of mineral increase to organic growth was dying with the century, and was already dead in Lemery's pages. The old notion lingered, however, even in the minds of the great. Locke had not discarded it, though he was not obdurate towards many revolutionising ideas in the domain of natural science: "All stones, metals, and minerals, are *real vegetables*; that is, grow organically from proper *seeds*, as well as plants."<sup>6</sup>

<sup>1</sup> Dowdall, *De Mirabilibus Auscultationibus*, 1909, 42, 43.

<sup>2</sup> Birch, *The Works of the Hon. Robert Boyle*, 1772, iv, pp. 79 ff.

<sup>3</sup> Bacon, *Nat. Hist.*, viii, 797.

<sup>4</sup> Birch, *loc. cit.*

<sup>5</sup> Lemery, *A Course of Chymistry*, trans. Harris, 1686, p. 49.

<sup>6</sup> Locke, *Elements of Natural Philosophy*, *The Works of John Locke*, in 9 vols., 12th ed., ii, 1824, p. 430. This little treatise was published posthumously in 1720. For an implied correction of the opinion quoted in the text *vide* Locke, *Essay*, III, vi, 29. The *Essay* was first published in 1790.

The circulation of the blood had been disclosed by William Harvey. Locke credited the brains with separating, or manufacturing, "animal spirits" from their share of the blood. The parts of the body then received their power of sense, and their various motions, from the animal spirits. The nerves were intermediaries between brains and organs.<sup>1</sup> Locke lagged behind the discarding of metallic vegetables; he was neither behind his times, nor before them, in this physiological mechanics. For Descartes the nerves were conduits to carry a subtle air, or wind, that moved the muscles. For Descartes also the *animal spirits* were generated from subtle parts of the blood to flow through nerves and excite muscular movements.<sup>2</sup> The *spiritus animalis* had surged through nerve-canals in the Galenic physiology of the second century after Christ.<sup>3</sup> In the ninth century, as in the pages of Costa ben Luca, the *spiritus* passed through hollow nerves to operate sensation and movement.<sup>4</sup> In the seventeenth century Descartes was no solitary adherent to a past unanimity. Gassendi thought that vibrations travelled to the brain along nerves which were filled with *animal spirits* to serve as spiritual radii.<sup>5</sup> Leviathan himself, with his huge mass, Boyle thought, leaped when invisible *spirits* raced from his brain through his nerves to his limbs, though the *animal spirits* were too minute for prying anatomists to discover their channels.<sup>6</sup> In Berkeley's *Alciphron*, 1732, *animal spirits* were still messengers which ran to and fro in the nerves to keep touch between the soul and outward objects.<sup>7</sup> Though Gall, 1758-1828, stirred phrenology into vagaries he first dismissed the "vital spirits" from the "ventricles of the brain."<sup>8</sup> Locke was in the prevailing fashion with his *animal spirits*. He did not, in the *Elements of Philosophy*, conform them openly to one important item in seventeenth-century tradition. Descartes' *animal spirits* were very minute bodies that moved quickly like particles of flame.<sup>9</sup> Aristotle had disapproved of both competing views when the universe was supposed by some to be "discretes-in-contact," and by others to be atoms moving in voids. The same opinions,

<sup>1</sup> Locke, *loc. cit.*, p. 433.

<sup>2</sup> *The Philosophical Works of Descartes*, trans. Haldane and Ross, i, 1911, pp. 334 ff.

<sup>3</sup> Galen, *On the Natural Faculties*, trans. Brock, Loeb Class. Libr., 1916, *Introd.*

<sup>4</sup> Thorndike, *A History of Magic and Exper. Science, etc.*, i, 1923, pp. 658 f.

<sup>5</sup> Brett, *The Philosophy of Gassendi*, 1908, p. 132.

<sup>6</sup> Birch, *loc. cit.*, v, p. 10.

<sup>7</sup> Berkeley, *Alciphron*, 1732, par. 4.

<sup>8</sup> Elliot Smith, in *Nature*, 1924, 113, p. 390.

<sup>9</sup> Descartes, *loc. cit.*

Aristotle had added, recurred infinitely in cycles.<sup>1</sup> When corpuscular, or atomic, theories of matter invaded seventeenth-century science after their escape from the Aristotelian ban the old competition recurred. Descartes corresponded to the "discretes-in-contact" of the older philosophy, for his particles, or corpuscles, moved in a corpuscular crush of particles that touched throughout without void spaces. Subtle matters might simulate voids, but every point at every instant was occupied. Gassendi encouraged science to substitute an atomic spray for a corpuscular tongue. Boyle discussed the "strange subtilty," the "determinate nature," and the "great efficacy" of "effluvioms." Corpuscular assaults were a great expository vogue during the seventeenth century. Boyle noted that very minute corpuscles could achieve great effects by battery if they were many, swift, fitly shaped, and assisted by "catholick agents." Agitated particles of water could disjoin the corpuscles of saltpetre, and so divide them that they could be kept swimming in the liquid. Antimony exerted its "emetick power" by "substantial effluxion"; gems might be "medical stones" because they emitted powerful effluvia. Boyle did not shrink from spraying effluvia, steams, and corpuscular rushes, like showers of shot, though Descartes had maintained an ancient prejudice against voids.<sup>2</sup> Locke did not avowedly spray his "animal spirits," though he hinted at a conjectural condoner of presumedly separated particles. Boyle often remembered the Cartesian "subtile matter," which might make bodies springy, stream through pores, urge particles devoid of innate motion, tumble moving parts into calorific motion, promote intestine movement in liquids, assist spirit of nitre to attack iron, participate in the conveyance of light, and operate the physical world. An etherial substance, Boyle also noted, helped many to resolve the puzzling problem of the vacuum, and qualms about absolute voids induced some philosophers to adopt an ether, or ethers.<sup>3</sup> There might be in the vast "intermundane spaces" between stars and planets, Locke thought, "some fluid matter," very "thin and subtile." He did not, however, devolve duties upon it in his short sketch of physical nature in the *Elements of Natural Philosophy*.<sup>4</sup>

Though Locke did not vex the readers of his miniature treatise with puzzles about the movements of corpuscles, whether they were innate or contrived by subtle matters, nor with qualms about voids, he did freely adopt the seventeenth-

<sup>1</sup> Aristotle, *De Generatione et Corruptione*, i, 8; *Meteorologica*, i, 3.

<sup>2</sup> Birch, *loc. cit.*, i, p. 388; iii, pp. 542 ff., 661 ff.

<sup>3</sup> Birch, *loc. cit.*, i, pp. 37, 195, 367, 379, 445; iii, pp. 278, 309; iv, pp. 253; v, p. 28.

<sup>4</sup> Locke, *loc. cit.*, p. 418.

century corpuscular, or atomic, mechanism. All sensible bodies, great celestial masses, the earth, their inanimate parts, and their animate inhabitants, were composed of "unconceivably small bodies, or atoms." From the combinations of these "bigger molecu<sup>l</sup><sub>æ</sub>" were made, bigger bodies still from their "greater and greater composition," and the whole material world was constituted by this progressive process. Locke was a seventeenth-century corpuscularian, or atomist. Descartes had insisted on extension as the distinctive attribute of matter. The Cartesians, Boyle noted, allowed extension only a notional difference from body.<sup>1</sup> He himself, though no slavish follower of Descartes, was considerably faithful to Cartesian expositions of corpuscular mechanics. Sensible bodies derived their properties, according to Boyle, from the size, shape, motion or rest, of their final corpuscles, and from their texture or disposition. He did not forget *weight*, nor did he wholly neglect it during systematic exposition, but he did not include it among the fundamental sources or accompaniments of property. His exposition of the corpuscular mechanism bears throughout the Cartesian impress, though he was no subservient chemist who meekly adopted dogmas from Descartes, and his disregard of weight is characteristic of his times.<sup>2</sup> Boyle did not include weight among the fundamentally important properties of corpuscles; Locke did not include it either: "by the figure, bulk, texture, and motion, of these small and insensible corpuscles, all the phænomena of bodies may be explained."<sup>3</sup>

The vogue phrases, and private vocabulary, of each age leave permanent etymological reminders of its pet notions. Boyle loved the phrase "parcel of matter"; Locke used it in his fifth paragraph. It probably suggested a corpuscular content. "No parcel of matter," wrote Locke, "can give itself either motion or rest": a resting body required a cause to make it move, and a moving body also required an external cause to make it rest. As much force was needed to stop a moving body as to stir it from rest. Motion was too well known by sight and touch to need a verbal effort at description. Its speed was measured by distance and time. If two bodies contained the same quantity of matter and moved with equal speeds, their quantities of motion were the same. If A had twice as much matter as B, and moved with equal swiftness, it had twice as much quantity of motion. The same force, applied to two different bodies, always produced the same quantity of motion in each. The regime of the New-

<sup>1</sup> Birch, *loc. cit.*, i, p. 145.

<sup>2</sup> Boyle, *The Origine of Formes and Qualities*, 2nd ed., 1667.

<sup>3</sup> Locke, *loc. cit.*, p. 440.

tonian mechanics was beginning. Newton's Law of Gravitation was exerting its great power: it seemed to be "a settled law of nature," Locke observed, that all bodies tended to attract one another, or gravitate. Manifest molar attractions were destined to perturb thought by their mysterious occurrence. Locke did not speculate on the mechanism of gravitation. He was content to accept attraction from experience, to acknowledge its inexplicable nature, and adopt it as "a principle in natural philosophy."<sup>1</sup> This private perspective of the seventeenth century contains no hint of the mental fuss over the notion of "attraction." Cartesianism required no distant action between pushing particles, nor an extra mechanism to explain it. Electrical attraction did not convince thinkers against the Cartesian presumptions. When light objects raced to amber, a stream of sticky corpuscles, on one theory, had issued from the rubbing, stuck to the paper or straw, and retracted, as a chameleon's tongue shoots out and returns with a fly. All modern naturalists, affirmed Boyle, who aim at intelligible explanation, ascribe electrical attraction to corporeal effluxes. Boyle carefully corrected any apparent concessions to *attraction* by identifying it with a kind of "pulsion," and recognising that it was usually "trusion," as when a gardener drove a wheelbarrow.<sup>2</sup> Attraction was naturally extended to pervade matter through and through. Newton's *Queries* at the end of his *Optics*, first published in 1704, formally invited chemists to recognise attractions between final particles, or atoms. He proposed attractive powers between particles, as when oil of vitriol drew moisture, elective attractions, as when blue vitriol solution exchanged its copper for immersed iron, and commended his proposals by constructing an ethereal mechanism for contriving attractions (and repulsions).<sup>3</sup> Chemistry finally accepted chemical affinity, and preferential, or elective, affinity, from Newton's *Queries*. The ether slowly nursed attractions into favour against reluctance to accept distant actions—even at insensible distances between insensible particles, but "forces" often did not please chemists even in the earlier eighteenth century. The alarmed auditors of the eulogium on Geoffrey in 1731 muttered "disguised attractions" at his table of relative affinities.<sup>4</sup> Locke, however, whose main interest in the mind concluded his *Elements of Natural Philosophy* with a chapter entitled "Of the Understanding of Man," accepted the mystery of gravitation without exploring the perplexities it was producing, the controversies

<sup>1</sup> Locke, *loc. cit.*, pp. 415-17.

<sup>2</sup> Birch, *loc. cit.*, iii, p. 279; iv, pp. 96, 129 ff.

<sup>3</sup> *Newtoni Opera*, ed. Horsley, iv, 1782; *Optics*, pp. 216 ff.

<sup>4</sup> Dumas, *Leçons sur la Philosophie Chimique*, 1837, pp. 366 ff.

it was causing, or observing its disconcerting intrusion on the final particles of matter.

Locke reveals in some of his adjectives and comparisons the diffusion of ideas that would help to date his treatise within limits. The air, he remarks, is a "springy body"; Boyle had been copious and informative on the *spring* of air. The atmosphere, he adds, is less and less compressed as its strata extend upwards. In a pile of fleeces the bottom is compressed most, and the top least; Boyle had looked to the *spring-like hairs* of the fleece for an analogy to the springy particles of air.<sup>1</sup> Locke himself quotes Boyle's *Physico-mechanical Experiments* when he declares for an atmosphere probably several hundred miles high. He very significantly recognises other particles or *steams*, "besides the springy particles of *pure air*."

"Steam" had a wide sense when it dropped from seventeenth-century pens. The obtrusive evaporation of water had very anciently suggested a dry exhalation from earth. Aristotle distinguished between the vaporous exhalation from water and the earthy exhalation which was dry like smoke. Moisture was raised into the air by heat and condensed from it by cooling. The dry exhalation rushed in wind, pounded the earth into earthquakes, and flashed from the clouds in lightning, for fire was a dry exhalation on the boil.<sup>2</sup> A lively sense of emanations culminated in Boyle's recognition of the air as a colossal rendezvous of "effluvioms." "Camphire" insisted that dry, consistent bodies could emit "steams." Fixed bodies could project "effluvioms," corporeal emanations could reach the air from suns and planets, and they could reach it from the inner earth. Locke's mild remark that "several steams or minute particles of several sorts" wandered in the air but coldly sets forth the multitudinous chaos that Boyle persuaded chemists to imagine in the atmosphere. There was scarce a more heterogeneous body in the world, Boyle urged, and Boerhaave, in the eighteenth century, responded with samples of virtually every substance in the world in the air that he compared to chaos.<sup>3</sup> Boerhaave consummated a tradition, if Boyle had not already consummated it, but the chaotic immigrants were not *air*. That is the significance of Locke's "springy particles of pure air." *Real* air, elementary air, was permanently elastic: perennial *air* had a perennial *spring*. In a letter to Boyle Sir Isaac Newton spoke of the "true permanent air," which he then conjectured to be of a "metallic original."<sup>4</sup> "The particles of water raised into the atmo-

<sup>1</sup> Birch, *loc. cit.*, i, p. 11.

<sup>2</sup> Aristotle, *Meteorologica*, i, 4, 9; ii, 9.

<sup>3</sup> Shaw, *A New Method of Chemistry*, trans. Boerhaave, i, 1741, pp. 379 ff.

<sup>4</sup> Birch, *loc. cit.*, i, p. cxvi; iii, pp. 279 ff.; iv, pp. 85 ff.; v, p. 612.

sphere," that Locke notes,<sup>1</sup> were not *air*, no more were any of the other "heteroclite effluvioms" that haunted it.

Words usually extend or narrow their meanings in three centuries, and often alter them. Locke obviously uses "meteor" in its older, more extended, sense of "any atmospheric phenomenon," that it still bears in "meteorology," when he declares that "clouds are the greatest and most considerable of all the meteors." More wariness is needed to interpret the "sulphureous and saline particles" that were presumed to be raised up with aqueous vapours and mixed with the watery parts of clouds. Locke incorporated a traditional theory of combustion, and leaned on a very popular analogy, in his exposition of thunder and lightning. Celestial burning was still the source of the flaming lightning, as it had been in Aristotle. Nitrous and sulphureous particles, however, fired together, and broke with violence into light and noise. "Saline" and "nitrous," though the former is the wider term, here refer to the same particles. Locke's comment that the roll of thunder, with its associate lightning, "very much resembles gunpowder" did not merely compare two sounds, or two sounds and two flashes, for the explosion of gunpowder was supposed to be in a very literal chemical sense an analogue of the play of lightning and the crash of thunder.

Brimstone contained "sulphureous particles," but so did charcoal. The phlogiston theory began its childhood in Germany before the seventeenth century ran out, but Sprat mentioned a very prevalent notion of combustion when he connected fire with a dissolution of heated "sulphureous bodies." \* Combustible bodies contained the *sulphureous principle*; they emitted it during combusive dismemberment. Nicolas Lemery of France, 1645-1715, represents the common stock of ideas on which Locke drew. Saltpetre, he remarked, was not inflammable, and it contained no *sulphur*—no principle of combustion. He had watched the violent deflagration of saltpetre with coals. This combusive episode intruded its vigour as a model of all combustion. If the combustible charcoal flamed up with saltpetre, emitting its sulphureous particles when the two were heated, and if sulphur and charcoal combined their vigours in the explosion of gunpowder, then charcoal, or sulphur, or, analogously, any combustible body containing "*sulphureous*" particles, might burn in air, and flame, because the *air contained saltpetre*, or the spirit of nitre. Saltpetre, or nitre, was well qualified for its combusive rôle. Oil of vitriol revealed its piercing, active, volatile "spirit" (nitric acid). The spirit of nitre contained many fiery parts; the subtle

<sup>1</sup> Locke, *loc. cit.*, p. 425 f.

<sup>2</sup> Sprat, *The History of the Royal Society*, 3rd ed., 1722, p. 215.

matter of these, when its particles moved rapidly as they escaped during deflagration, fussed with the escaping sulphureous particles of the combustible, rarified the "sulphureous fuliginosities," and this commotion was the fire, the heat, and general aspect of combustion.<sup>1</sup> Each chemist had his private variations of combusive detail, but in Lemery's exposition one compelling analogy, that compelled many expositions, can be discerned: combustion in *air* was essentially a deflagration of the combustible with *aerial nitre* or *aerial spirit of nitre*.

Boyle suspected that some "anonymous" substance dispersed through the air was necessary for the "subsistence of flame." He also suspected it of being "volatile nitre."<sup>2</sup> The participation of nitrous and sulphureous particles in thunder and lightning, as mentioned by Locke, was clear to his contemporaries. The nitrous particles of nitre and the sulphureous particles of the combustible mingled in the commotion of charcoal deflagrated with nitre. The commotion was very violent with gunpowder, when nitrous particles of nitre and sulphureous particles from charcoal and sulphur escaped violently together. Fugitive sulphureous particles were raised into the air; saline nitrous particles of nitre, or its spirit, were likewise raised. When the sulphureous and nitrous particles were atmospherically collected, as they were suddenly collected during the firing of gunpowder, the lightning flashed in the heaven as the flame flashed in the cannon, and the thunder rolled in the sky as the peal of artillery sounded.<sup>3</sup>

If contemporary chemists read in Locke that "the air may be looked on as a clear and pellucid *menstruum*, in which the insensible particles of dissolved matter float up and down,"<sup>4</sup> they would add a copious context to it. Burning bodies dismembered in the solvent air, and spread through it, as salt dismembered in water and spread through the liquid.<sup>5</sup> If spirit of nitre dismembered and solutively dispersed metals, aerial nitre, or its spirit, might dismember and disperse the burning body. Boyle noted that the air retained most of its elasticity when the flame was extinguished in an enclosed space.<sup>6</sup> Unanimity on combusive detail was not complete, but it was quite usual to confine the *attack* on the combustible body, when it burned, to the aerial nitre, or aerial spirit of nitre. The *air*, which had received the nitre, also received the dismembered combustible. Sprat defined flame as a dissolution of "smoak": the combustible particles were carried up by the heat of the rarified air. The ashes, he added, were

<sup>1</sup> Lemery, *loc. cit.*, pp. 289 ff.

<sup>2</sup> Locke, *loc. cit.*

<sup>3</sup> Sprat, *loc. cit.*

<sup>4</sup> Birch, *loc. cit.*, iv, p. 90 f.

<sup>5</sup> Locke, *loc. cit.*, p. 426.

<sup>6</sup> Birch, *loc. cit.*, iv, p. 90.

not dissoluble by the air.<sup>1</sup> The participation of air in combustion troubled chemists, and was variously conceived by them, till Lavoisier disclosed its nature. When Locke wrote of the menstrual air it was very usual to confine any participation of the *air* to a purely mechanical office, to exclude it from disruptive effect, and to identify *air* with a *receptacle*. Aerial nitre was an immigrant into air, and not a part, or component, of it. When charcoal deflagrating with nitre was the compelling analogy, as it often was, the aerial spirit of nitre discerpted the burning body, or participated in the discerption, and the permanently *elastic*, or *springy air*, genuine air, received the discerpted products. The meagre ash, as from wood, dismembered from the discerpted whole, residually resisted the aerial menstruum.

Locke understood that the heat of the sun raised water into the air: "ordinary distillations" did the same. The collection of the steam back into drops was less easily explained. It seemed to answer, however, to the "precipitation" of the "chymists." He noted that "anomalous" quicksilver might be joined to the heavy, fusible, and malleable metals. Quick, tremulous movements in bodies urged vibrations on the air, and the motion produced in men the sensation of sound. The brisk "agitation of the insensible parts" produced the sensation of heat. Heat and cold were the two great instruments of nature. Greater or less briskness in the moving insensible parts of bodies corresponded to the two sensations of hotness and coldness. The axle-trees of coaches demonstrated the connection between heat and motion: *friction* had always been, since the days of Aristotle, the tutor of speculators on heat. Thus Locke summarised the science of his time with a brevity intelligible to contemporaries who possessed the context. Perhaps the deductiveness, as well as the piety, of his age is manifest in his plea for the Plurality of Worlds. If God was wise and good and great, the "fixt stars" would be suns, and their retinues would be "systems of inhabitable planets." These remote bodies were little useful to men on earth, though God could display his goodness to their inhabitants. Locke gave no astronomical or physical arguments for his belief: it seems unwise to be too ready to deduce how the wisdom, power, and goodness of God will necessarily be displayed in the depths of space.<sup>2</sup>

<sup>1</sup> Sprat, *loc. cit.*, p. 216.

<sup>2</sup> Locke, *loc. cit.*, pp. 421, 426, 429, 436 ff.

## NOTES

### **How the Central Library for Students can assist the Scientist (Luxmoore Newcombe, Librarian)**

THE Central Library for Students was founded in 1916, primarily to supply books to the University Tutorial and W.E.A. Classes. The enormous advantage of having a central source for the supply of books required by students of all kinds rapidly led to the development of the Central Library on much broader lines. Last year, apart from the issues to Adult Education Classes, no less than 28,991 volumes were lent to students through 313 municipal and other libraries. The importance of this figure will be better appreciated if it is remembered that in the great majority of these cases the student could not have obtained his books in any other way.

The function of the Central Library, as it now exists, is twofold. In the first place, it acts as a reservoir library to all the other libraries in the country for the supply of books wanted by students; and, secondly, it forms a clearing-house for the loan of books from certain special libraries. It is the second side of its work that is likely to be of particular value to scientists and research workers generally, but it may be useful to explain briefly the limitations attached to the library's own stock. The Central Library will not supply any books costing less than 6s., nor does it supply textbooks which the student should provide for himself, or books which the local library could reasonably be expected to add to its own stock. Books on pure medicine and law are also excluded, but books on the medical sciences, and law books which are of value from the point of view of history or economics, are supplied. Books supplied from the library's stock are mainly modern English and American books on any branch of science or the humanities, other than those just mentioned.

As has already been indicated, the more valuable work of the Central Library, from the point of view of the research worker, is its recently introduced system of what is known as "Outlier" Libraries. An "Outlier" Library is a special library which, in return for a grant received from the Carnegie United Kingdom Trust, will lend books through the Central Library. The following is a list of the libraries at present

co-operating in this scheme. It is probable that this list will be considerably extended in the near future.

Animal Diseases Research Association.

British Drama League.

British Institute of Adult Education.

British Optical Association.

College of Nursing.

Co-operative Reference Library. (Agricultural economics.)

Geographical Association.

King's College for Women, Household and Social Science Department.

League of Nations Union.

London School of Economics.

Manchester Library for Deaf Education.

Manchester Literary and Philosophical Society. (Rich in scientific periodicals.)

Rothamsted Experimental Station. (Agriculture. Rich in periodicals.)

Rowett Research Institute. (Animal and human nutrition.)

Royal Aeronautical Society.

Royal Anthropological Institute.

Royal College of Veterinary Surgeons.

Royal Institute of International Affairs.

Royal Scottish Society of Arts.

Scottish Marine Biological Association.

Society of Antiquaries.

Solon Ceramic Library.

It will be seen that several of the libraries in this list contain important scientific collections. One or two of the libraries are particularly strong in periodicals, and several contain sets which are believed to be the only copies in the country.

Anyone wishing to borrow a book from an "Outlier" Library, or from the Central Library, should ask the Librarian of his local library (whether municipal, county, university, or institutional) to apply to the Librarian of the Central Library for Students, Galen Place, Bury Street, London, W.C.1, who will make the necessary arrangements. In no case must application be made direct to the "Outlier" Library, nor will books be issued to borrowers except through their own library. No fee is charged for this service, but the borrowing library must pay postage both ways.

**The Stockholm Physiological Congress (R. W. Gerard, University College, London)**

In all gatherings of people the personal element is of necessity the most interesting and usually important, and physio-

logical congresses are no exceptions. Certainly at the twelfth triennial International Physiological Congress, held last August at Stockholm, the physiologists were of more significance even than their physiology, as the following observations show.

The meeting was truly international, for one thing. Over six hundred active men and women from more than twenty-five countries in five continents met and mingled in cordial good fellowship. As they walked in the corridors, all alike wearing clothes standard the world over, or even as they jostled one another on the dance floor, all similarly responding to the universal tunes, an observer would be hard put to choose one nationality from another. Only the polyglot babble of many tongues and the impartial use in talks and speeches of English, French and German revealed the true nature of the group. And it was a fine thing to hear ex-enemies, finding themselves neighbours at the elegant banquet offered by the City of Stockholm, sound each other's linguistic powers, and then slide into animated conversation in whatever language suited best.

The Swedes supported the Congress well with 111 members, 82 from Stockholm. Of the visitors, Germany came first in number with 110, Berlin alone contributing 43, far more than any other city except Stockholm. The delegation from the United States numbered 87, a splendid turn out from across the ocean, and a good example to physiologists the world over, who are expected to return the visit at the next meeting in 1929. The British Empire was represented by 70, of whom 10 also crossed the ocean from Canada. Forty-seven came from England, over half of them from London, one-third from Cambridge, and only one from Oxford—the contemporaneous meeting of the British Association in that city having kept another score at home. France sent 38, 30 from Paris, and the Netherlands 26. Switzerland, Denmark, Poland, and Russia also sent more than 10 physiologists each, and no country in Europe lacked representation.

The physiologists who gathered were, as all said, well rewarded aside from scientific considerations, for the Swedish people yield first place to no one in downright good nature and hospitality; Stockholm is one of the most beautiful cities in the world, and the City Hall, in which the sessions of the Congress were held, is certainly one of the few perfect buildings of modern times. Those writers of the day who upbraid science and its builders as coldly unfeeling, the source of most of humanity's woes, would have been quite shocked at the large number of men who neglected important papers to stand a little longer in admiration of an odd lamp or wander at leisure along impressive passages. And few who attended the

banquet will forget either the room given to it, well called the Gold Room because all walls were surfaced with a gold stone mosaic, or the efficient handling of 700 guests within it, and the bounteous assortment of "refreshments."

The programme as a whole was well planned and smoothly executed, and the local committees and officers, especially the popular secretary, Dr. Liljestrand, well deserved the enthusiastic applause given at the concluding session. Particularly happy was the arrangement for limiting communications strictly to a set time. Two minutes before time was up a red light appeared on the speaker's table and remained till thirty seconds from the end, time being finally called by a bell. The system was very convenient to the speaker, giving him warning in ample time, and to the audience, for few indeed had the temerity to overstay their time.

The scientific programme was initiated by Sir F. Gowland Hopkins, who sketched the status of present knowledge of "The Mechanisms of Biological Oxidations." The rival theories of oxygen activation (by iron) and hydrogen activation (by specific enzymes) he believes are not mutually exclusive, and each mechanism may act concomitantly with the other, though the evidence marshalled especially supported the hydrogen theory of Wieland rather than Warburg's iron-oxygen mechanism.

Following this single meeting, four separate meetings were continued throughout the Congress during three mornings and afternoons, making twenty-four sessions in all. Each session was, with few exceptions, devoted to one major topic, as: heart, special senses, muscle, central nervous system, peripheral nerve, gonads, endocrines, blood coagulation, insulin, kidney, vitamins, digestive system, metabolism and blood gases, circulation and lungs, and the like. A session was given to problems of cell oxidation, one to the various applications of physical chemistry in biological systems, one to anaphylaxis and related problems, one to pharmacological matters and one even to investigation of pure structural and synthetic chemistry—which shows how all-embracing a physiological congress may be! Demonstrations were given at the same time as the four parallel meetings, 40 in all, which, with the 270 papers, represented contributions from over 300 physiologists.

Any summary of the new (and old) knowledge presented is, of course, impossible, and no one contribution stands out as especially spectacular: but in each of the fields indicated has the steady progress of discovery gone on, and in many notable advances have been made. A few examples must suffice.

Sensory physiology, so much neglected, had perhaps the largest boom with careful studies of all phases of vision, temperature sense, and thirst; and especially of pain and other senses by Adrian and Zotterman, who reported studies with the capillary electrometer of nerve impulses in single fibres generated by natural stimulation of receptors, showing a gradation of sensation produced by frequency of discharge, work which gives promise of wide and fruitful application.

Otto Lœwi demonstrated very clearly the liberation in the blood of a cardiac inhibitory substance upon stimulation of the vagus, and told of chemical studies which suggest that this substance may be acetyl cholin. Vagus and drug action on the heart he is able to interpret in terms of this mechanism. Anrep and co-workers with very sensitive electric methods have analysed the time course of the coronary circulation and seem to have proved a control by the vagus (vaso-constrictor) and the sympathetic (dilator), both tonic and regulative.

The separation from muscle juice and concentration of a lactic acid ferment was reported by Meyerhof. With its aid he has studied the breakdown of glycogen and finds an active and a passive form of hexose diphosphate, the active breaking readily into lactic acid in the presence of the ferment, the inactive being formed from half the molecules of active and breaking down slowly in the absence of phosphate. Hoet and Marks have established in thyroid-fed and other animals that rigor mortis may appear in muscles giving an alkaline reaction, and therefore in the absence of lactic acid. They find rather a correlation between absence of glycogen and appearance of rigor, and suggest that the inability of the muscle to maintain a normal concentration of hexose diphosphate is the direct cause of rigor. Embden, also studying muscle glycogen, finds more in the muscles of a leg that has been "trained" by continued exercise than in the normal control.

The Utrecht School (of Pharmacology!) showed beautiful slow-motion pictures of the standing and righting reflexes of animals subjected to various operations, especially decerebellation.

The production of heat by nerves during activity has at last been demonstrated by Hill and co-workers, and shown to correspond fairly well with previous studies of gas exchange. A new method for studying the conduction of the nerve impulse cannot fail to be fruitful. Kato elaborately expounded his "theory of decrementless conduction in narcotised nerve," and gave demonstrations of its validity.

Thunberg described a barospirometer in which an animal or human can be placed and respiration maintained with an entirely immobile chest by alternating positive and negative

pressures, and he pointed out its possible application in the treatment of pulmonary disease and in thoracic surgery.

The empirical discovery that intravenous injection of calcium eosinate protects rabbits against anaphylactic shock was reported by Girard and Peyre.

Caridroit, Pezard, and Sand propounded an "all or none" law for the action of testicular hormone in the cock. The comb transplanted on the back withers if the bird has been castrated, but grows if the testes are present. Grafting different amounts of testes in castrated birds showed that any sized graft below a certain limiting value was ineffective, but that once this value was reached the graft was just as effective as very much larger ones.

Howell reported chemical studies on the heparin he discovered and believes it is a glucoside of glycuronic acid with sulphuric acid and some nitrogen-containing body. Mann reviewed the work at Rochester on liver function. Dogs with livers removed die in a few hours of a hypoglycemia and, if carried over this period by sugar injections, they die later with a characteristic syndrome, like human cholemia, the cause of which has not yet been unearthed. He also described quantitative studies with the spectrophotometer which showed that most of the bilirubin is formed in bone marrow, a little in the spleen, and least in the liver itself.

Best, Dale, and others found in eviscerated cats that about half the sugar disappearing under the influence of insulin is burned and the remainder converted to glycogen in the muscles. The decrease in glycogen in muscles reported by others is due to the convulsions of hypoglycemia. Heymans described technically beautiful experiments on crossed head circulation in dogs and studies on vagus and other nerve functions in their preparations.

Among the demonstrations may be mentioned Henderson's ethyl iodide inhalation method for measuring the circulation in humans; a portable glass electrode outfit for electrometric pH determinations, developed by Mrs. Kerridge and just marketed by the Cambridge Instrument Co.; and a group of very convincing experiments on iron and tissue oxidation by Warburg.

A large number of papers, as usual, might well have been omitted, but considering that practically no executive veto was exercised, the sheer trash was very little. With ever-growing attendance and participation, it will surely be necessary in time to develop an "editorial" board, but for the immediate future division into more groups than four or some similar device may serve. But apart from the scientific give and take in meetings and in corridors, physiologists the world over

exchanged greetings and impressions, praised the beauties and hospitality of Stockholm, and departed richer for new friendships made or old ones renewed.

### **The Exploration of the Sea**

We have received a copy of the report of the British delegates to the International Council for the Exploration of the Sea summarising the proceedings at the meeting of the Council at Copenhagen in September last. Perhaps the most important item on the agenda from the point of view of this country concerned the proposals for the conservation of the plaice industry in the North Sea. It appears that in 1921 suggestions were made which would have excluded from certain selected areas vessels of the type commonly employed by British fishermen, while admitting those employed by some at least of their principal competitors in the trade. Needless to remark, these proposals did not meet with the approval of the British Fishing Industry, and the Government remained firm in its determination to have no part in any scheme which lacked the support of the industry in this or any other country. The Council, at a meeting at Amsterdam, then suggested the complete closure of the areas in question, but this again was not acceptable to the fishermen, and the Council is now faced with the problem of finding some other method of conserving the industry.

One method by which not only the plaice fisheries but other fisheries also might be protected against excessive exploitation would be the general employment of gear which would enable under-sized fish to escape capture. The design of such gear is, however, by no means a simple matter, for it must permit the escape of undersized flat-fish without at the same time releasing round fish of marketable value. Experiments have been made with different types of gear, and it was decided that these should now be tested one against the other in the Baltic and the North Sea.

The Council had been urged by certain of its members to stimulate the organisation of a world-wide international oceanographical expedition. This was regarded as quite unfeasible by the British delegates, and it was resolved instead to take steps to secure an exchange of views between expeditions organised in different parts of the world by different countries, in order to secure discussion of the best methods and apparatus for marine research.

In 1925 a committee was appointed to consider the regulations for fishing in the Moray Firth, and it was to have met in Scotland last summer. The General Strike and the Coal

Strike interfered with the arrangement, but the members of the Committee visited the Firth after the Council Meeting, and will report to the Council at their meeting in Stockholm in 1927. Their duty was "to consider whether it is desirable to continue the existing prohibition of trawling in the Firth, and whether the prohibition in its present or any modified form should be applied to any other method of fishing, and to fishing vessels of all nationalities in the whole or any part of the extra-territorial waters of the Firth."

The meeting was rendered notable by the re-entry of Germany to the Council. The co-operation of that country is most valuable, especially in investigations in the North Sea and in Icelandic waters. It has also rendered possible the reduction from 25,000 to 20,000 Danish kroner of the contributions of the larger powers to the expenses of the Council. The smaller powers, *e.g.* Belgium, pay only 5,000 kroner (18.22 kroner = £1).

### Protozoology (R. R.)

Dr. F. J. Cole, Prof. of Zoology at the University of Reading, delivered two interesting lectures last year before the University of London at King's College, and these have been nicely printed this year by the University of London Press with the title *The History of Protozoology*. The work contains only sixty-four pages, including seven pages of the literature quoted; and there are excellent portraits, one of Leeuwenhoek, the founder of Protozoology in the seventeenth century, and another from a medallion of Maupas. Of course, in such a short space no one could do absolute justice to such a large theme, but Prof. Cole, who is master of an admirably clear and good style of English, has certainly achieved a great deal within his limits. Above all, he is not only biographical but also philosophical, and is able to balance and weigh the evidences collected by history in the *pro-and-con* scales. Beginning with Leeuwenhoek, the author follows on with Robert Hook and Müller, and then coming to the invention of the microscope, proceeds to the modern protozoologists. The story told is that of gold-seekers who must examine many barren sands before reaching their quest; but every grain acquired is of much more value than any gold or gems. Error often has a singular function in science, in that the most surprising mistakes sometimes lead to the most surprising truths. The concluding pages of the book deal with proto-parasitology and do so in a manner which is rare amongst writers, in that it is generally sound. Prof. Cole allows himself only a brief sketch, but that is better than lengthy but often erroneous histories

by many writers on the theme. Of course there is a fundamental difference between protozoology applied to the study of disease and the general science. We doctors and others who have been engaged in the former cannot also be masters of the latter. We have to confine our attention to very particular points, and our aim is not the progress of general knowledge but the relief of the public from definite diseases. Prof. Cole, in his prefatory note, tells us that the lectures are based on material for a general history of Zoological Discovery which he has been collecting for many years. We hope at least that he will some day produce the complete history of protozoology which he also suggests.

### Conjecture and Proof

It is curious how few people seem to recognise the difference between conjecture and proof in scientific matters; and medical men, who are compelled to act chiefly on conjecture, are especially prone to such confusion of thought. Hundreds of persons, or in fact almost anyone, can concoct or even publish an hypothesis on almost any subject. Our politics consist almost entirely of hypotheses; our philosophies, our creeds, and even our actions in life are and must be generally based on hypotheses. So-called science comes to birth only with proofs. The man who finds the proof, not he who finds an hypothesis, is the discoverer. For instance in *The Times* of October 9, Señor A. Rico Avello seems to maintain that Miguel Servet was the first to enunciate the circulation of the blood; but he admits that Harvey was the first who proved scientifically the circulation of the blood. The whole history of science is a road which is paved with good and bad hypotheses—that is, conjectures; but the monuments along that highway are the proofs. Another case is exemplified in *The British Medical Journal* for September 18, p. 531, in an article on Fracastorius. The author thinks that this old writer of the sixteenth century deserves or might deserve as much credit as Pasteur himself; but it is obvious that Fracastorius was merely guessing at the germ theory; he did not prove any case of diseases being due to living organisms; he could not prove it because he was not possessed either of sufficient knowledge or of the necessary instruments. The author says that “Fracastoro may have been, perhaps was, as great a genius as Pasteur, but the world actually owes little to Fracastoro and much to Pasteur, for the seed Fracastoro sowed fell upon stony ground and that sowed by Pasteur upon good ground.” Not at all; the true reason is that the conjectures of Fracastoro were only conjectures, but the theorems of Pasteur

were theorems—and that is why the latter succeeded. Pasteur was preceded by many persons who seem to have had similar ideas—unfortunately they did not furnish similar proofs. Almost every textbook contains references to clever people who “thought so all along,” but who failed to prove their case. In science, mere guess-work is not good enough; but many medical men seem to think that it is.

### **Gratitude for Medical Discovery (R. R.).**

SCIENCE PROGRESS has frequently called attention to the necessity for some kind of reward, or rather pecuniary compensation, for men who have achieved important medical advances at their own cost. In vol. xiv, No. 56, April 1920, p. 635, it described in detail the proposals of a conjoint committee of the British Medical Association and the British Science Guild for effecting this object. As stated in subsequent numbers, the proposals were rejected by the Government of the day, who preferred, as most of our pauperised public do, to obtain their national “scientific benefit” for nothing, that is, at the expense of the doctors and other humble individuals who confer it. Since then, I have continued to argue the case backward and forward many times, but of course without any practical result. Recently one of our most distinguished investigators has died, and his friends are now obliged to go round, hat in hand, in order to raise money by subscription in order to help to educate his children! His work on typhoid alone must have saved this country hundreds of thousands of lives and some millions of money during the war, and his discovery of a new human parasite has been of enormous value in India. If the modest proposals just referred to had been accepted there would have been no necessity for any public appeal such as the one now launched. The necessity does not seem to strike the British public as being at all disgraceful to it. The public pours out many millions a year in doles, often given to the undeserving; it allows hundreds of persons to make large fortunes by work which confers little or no benefit upon the people; it gives handsome salaries and pensions to many whose services have been of comparatively little value to anyone: but it refuses to allot one penny out of the public purse to men whose labours have saved the life and health of thousands of the present generation, and will continue to do so for generations to come. On the other hand, it is true, our king honours them, and they receive a certain amount of respect even among their fellow doctors and scientists.

The Americans are still worse. They find it equally difficult

to imagine that their medical benefactors can ever desire professional payment for their services, and have, I believe, never attempted to give them any. Even their countryman Walter Reed, who assisted largely in the discovery that the great American plague of yellow fever is carried by mosquitoes, was allowed to die a few years later in a state of apprehension regarding his wife and daughter. They have never, so far as I know, bestowed the smallest thanks on any of the foreign medical men whose discoveries have helped them so largely—as for instance in connection with tropical medicine. They have deserved much credit, it is true, for financing institutions and sanitary work abroad ; but all this has done little or nothing for the men on whose original investigations such institutions and such sanitary work have been based. On the other hand, they have certainly “ wiped the Britisher’s eye ” in one thing—abuse of the foreign medical benefactors whom they have failed to honour. Some years ago an American book called *Béchamp or Pasteur* vilified Pasteur and other workers through hundreds of pages ; and now an unspeakable American publication called “ Microbe Hunters ” has appeared, which slanders living medical men (including myself) in a similar manner. It reminds me of what I once saw in the Zoological Gardens. A good little boy offered a biscuit to a monkey ; the monkey seized the biscuit with one hand and the boy’s hair with the other and pulled the latter until the would-be “ benefactor ” shrieked with pain—and then the monkey ate the biscuit ! There is evidently a close similarity between the two cases.

#### **Curing One Disease by Another (R. R.)**

One of the most remarkable developments in the cure of disease has now been investigated sufficiently to justify one in making some comment upon it. In 1917 Dr. Wagner von Jauregg of Vienna suggested that the fatal disease called general paralysis of the insane, usually caused by the *Spirochæta pallida*, might be cured or improved by injection of living germs of other diseases, and employed the malaria parasites for the purpose. His work has been followed with great interest in many mental hospitals, and paper after paper is now appearing on the subject. I notice especially the articles in English by Drs. Warrington Yorke and MacFie (Royal Society of Tropical Medicine, 1924), by Colonel S. P. James of the Ministry of Health, 1926, and by Drs. Adrianus Pijper and Elisabeth D. Russell (*South African Medical Record*, July 10, 1926). The patient is subjected either to the bites of malaria-infected mosquitoes or to the inoculation of the blood of a case of malaria. He contracts malaria after the incubation period, suffers from

a certain number of malarial paroxysms, and is then cured with quinine or salvarsan. In about 30 per cent. of all the cases very considerable and sometimes apparently permanent benefit is conferred in what was formerly thought to be almost an incurable malady—though about two-thirds of the cases still remain refractory. Even this 30 per cent., however, is a great gain. All the investigations have been most carefully and thoroughly performed. The inoculation of live malaria enables us not only to improve general paralysis but also to study experimentally the progress of malarial infection. I must say that one point strikes me very forcibly—that the malaria is cured in these cases much more easily than in infections which occurred naturally during the war. A very few doses of quinine seem to have the desired effect, whereas in natural infections (and I can speak from nearly forty years' experience) we are compelled to continue the drug for months before relapses can be excluded, and even then we do not know whether the patients are really completely quit of the disease. Apparently other parasites besides malaria, such as the spirochætes of relapsing fever, have a similar effect. Is this due to some antagonism between the infective organisms, or simply to the fact that the fever caused by malaria destroys or inhibits the *Spirochaeta pallida*—or possibly, to some more recondite cause? I have watched the progress of these researches with great interest, but can now scarcely avoid the conclusion that the result claimed has been substantiated. Other similar results may possibly be obtained also in connection with other maladies, but it is of no use to speak of this at present.

### Remote Control (F. G. Marshall)

Little does the average reader realise what a large part Remote Control plays in the huge factories and power stations of to-day.

Year after year, larger and more powerful machines are being made, and yet the initial control gear is becoming simpler and simpler.

A series of eight or ten super-power stations can be made completely "dead" by the pressing of a single push-button in the switch-room. Hence we have inconceivable power and energy that can be controlled by a child simply pressing a half-inch ebonite button.

The old clumsy switches, that used to take two or three men to move, have been replaced by the Remote Control System, for the age when brute force ruled in the switch-room is over. The button shuts a small auxiliary switch, which in turn

closes others, and finally the mammoth oil switches themselves.

In the power station of to-day all the heavy, cumbersome control gear is stowed away in some suitable fireproof building, and is operated by this means. The switchboard operator can control everything in the power station without stirring from the clean switch-room, with its highly polished panels.

In the modern factory the entire machinery can be stopped by a push-button in the manager's office, by means of a sequence of relay switches and automatic devices.

In engineering machine shops the fitter, or machine-man, has a push-button at his elbow, and with this he can stop his machine immediately without moving from his working position, even though his main switch may be several feet away. The days of long lines of belts, driven from one main shafting, are over. Thus Remote Control has been instrumental in preventing scores of accidents, which used to happen in the days of the old regime, due to clothing catching on moving parts.

Again, in the printing industry, every modern printing machine is equipped with a dozen or more control buttons situated in different parts all over the machine, thus enabling it to be stopped from any position. Consequently, in the case of a "break" the machine can be stopped before serious damage has resulted.

Even now the system of remote control has not yet reached its zenith. The control of ships and torpedoes from land by means of electricity and light-rays is now looming up close in the future, and the satisfactory control of aircraft from the earth may soon follow.

### **The Deterioration of Silk Fabrics**

The members of the National Association of Dyers and Cleaners of the United States recently became much concerned at the numerous claims to which they were being subjected on account of the damage which silk articles sent to them for cleaning had apparently undergone during that process. They maintain a research associateship at the Bureau of Standards for the study of persistent troubles which occur throughout the industry, and through this associateship the matter has been thoroughly investigated. The results of the study of tin-weighted and unweighted samples of silk, after various treatments and exposures to sunlight and storage at standard conditions (65 per cent. relative humidity and 70° F.), are stated as follows: (1) No deterioration results from exposures to standard atmospheric conditions over a period of two and

a half months, even when acid or alkaline perspirations are applied. (2) Sunlight exposure causes a marked deterioration in both unweighted (but dyed) and tin-weighted silks, the loss in strength in 100 hours' exposure amounting to about 25 per cent. for unweighted and about 50 to 75 per cent. for weighted silks. (3) Acid and alkaline perspiration treatments increase the deterioration when sunlight exposures are given, so that the loss in strength in 100 hours' exposure is about 35 per cent. for unweighted and about 65 to 100 per cent. for weighted silk. (4) Dry-cleaning solvents in no case caused any appreciable deterioration of the silk fabrics.

There are several points in this abstract upon which further comment may be made. The treatment of silk to increase its weight (and so also its selling price) began when it was observed that silk, dyed black with an iron-tannin mordant and logwood, was distinctly heavier than the undyed material. The iron-tannin process is now almost entirely superseded by the use of tin salts for which silk has a great affinity. They have the further advantage that they do not discolour the silk, so that dyeing in all kinds of light shades is possible. Excessive weighting, however, tenders the material, and although fabric is sometimes found in which there is more weighting than silk, the present practice is not to exceed 25-30 per cent. of the original weight of the raw material. When more than this is used there is a possibility that the silk may rot in the hands of the manufacturer or retailer before it reaches the public! Deterioration is due to the formation of free acid in the silk, as the result of exposure to sunlight and to the air, especially in the presence of sodium chloride (in sea air, perspiration, etc.). This occurs with the unweighted fabric, and the increased liability of metallic weighted silk to deterioration may be due to the action of the acid on the weighting material itself.

The advantages of the so-called dry-cleaning process are (i) that the solvents used are chemically inert in contact with the fibres, and (ii) that they do not alter the shape of the article, *e.g.* a pleated material retains the pleating in a dry solvent though it is entirely lost in water. Naphtha is now commonly used as the solvent, although the more inflammable motor fuel is still employed, and, indeed, can be used in the household (or, rather, in the garden!) with complete success. A soap, known as benzine soap, is used to aid the solvent in the cleaning process. A formula for this is as follows: Part i: 1 pint of petrol and  $\frac{3}{4}$  pint of oleic acid; Part ii, dissolve 1 oz. of caustic soda in  $\frac{1}{2}$  pint of soft water. Add part ii slowly to part i in a small stream, stirring constantly, and continue stirring for fifteen minutes after all of part ii has been added.

Very little of this soap is required, only 70 grains to the gallon of solvent, and, if it is used, the articles must be rinsed free of it in fresh, clean solvent.

Various varieties of silk were tested at the Bureau, and the behaviour observed varied very much with the sample. The satins all showed a marked resistance to deterioration, so also did the single sample of chiffon taffeta, and the unweighted crêpes. The weighted crêpes and taffetas did not survive nearly so well, especially when treated with baths equivalent in their effect to perspiration.

A full account of this work on silks will be found in Technologic Paper No. 322 of the Bureau of Standards (Government Printing Office, Washington, D.C., U.S.A. Price 15 cents).

### **The Young Delinquent**

Prof. Cyril Burt, M.A., D.Sc., Psychologist to the London County Council, discussed the ill effects which may result from forcing adolescent children into unsuitable employment giving no scope for their natural abilities in a lecture at the London School of Economics on November 19 last, arranged by the National Institute of Industrial Psychology. The chief points in his address may be summarised as follows :

" During recent years considerable interest has been shown in the subject of crime in young children. Inquiry shows that most hardened criminals begin their career during childhood. Since a crime is a conscious act, the study of the criminal, whether young or old, must be a branch of psychology.

" Many inquiries into the causes of delinquency have now been carried out. They show that we can no longer assume, with Lombroso, that the habitual criminal is a born offender, inheriting a propensity to crime which can never be rooted out. The factors at work are numerous and complex ; they are partly environmental and partly hereditary ; and they differ in different cases.

" When young criminals are brought to the psychologist for examination, it is found that a large percentage of those who are of employable age are in employments that are not in the least suited to their capacities and interests. If, therefore, a youth or girl of employable age is placed upon probation, one of the most important tasks is to find him congenial work which will make him a satisfied, self-respecting person instead of a restless malcontent with a grudge against society. Again and again, practical experience has shown that, once a safe, suitable and satisfying occupation has been found for them, such persons quickly drop their lawless habits.

" Society, instead of simply punishing the young offender,

now seeks to prevent his offence from recurring ; and doubtless a further study of the causes of crime will show how much may be done to prevent it from ever occurring at all.

" Statistics reveal a great increase of crime about the age of adolescence, that is, soon after the time at which the child leaves school for industry. It is then that a criminal career, like any other career, is most apt to be chosen.

" As unemployment increases, crime tends to increase as well : for the youth or girl who is out of work takes easily to theft or to more sordid forms of earning a livelihood. But uncongenial employment may be almost as disastrous as unemployment. The youth who is unhappy or discontented in his job, who finds his work boring and monotonous, whose particular aptitudes and emotional energies find no outlet in his daily task, is just the person who is likely to react impulsively when work is over for the day ; and is prone to seek a compensatory excitement in dangerous adventures, or to desire compensatory forms of amusement for which his simple wage will not suffice.

" To find a fitting occupation on the very first occasion that the choice has to be made will, therefore, be one of the surest preventives of crime. Much valuable work has already been done in this direction by the vocational experiments of the National Institute of Industrial Psychology ; and, as an incidental effect of the adoption of these methods on a wider scale, a decrease in juvenile crime may be confidently expected."

### Notes and News

The Council of the Royal Society has, with the approval of H.M. the King, awarded Royal Medals to Sir William Hardy, F.R.S., for his pioneer work on colloidal chemistry and the theory of lubrication ; and to Prof. A. V. Hill, F.R.S., for his work on the physical and chemical aspects of muscular contraction. The Council has also made the following awards : Copley Medal to Sir Frederick Hopkins, F.R.S., for his work on biochemistry ; Rumford Medal to Sir Arthur Schuster, F.R.S., for his services to physical science, especially in optics and terrestrial magnetism ; Davy Medal to Sir James Walker, F.R.S., for his work on the theory of ionisation ; Hughes Medal to Admiral Sir Henry Jackson, F.R.S., for his pioneer work in radio-telegraphy and its application in navigation.

The Swedish Academy of Science has decided to divide the Nobel Prize for Physics for 1925 between Prof. J. Franck of Göttingen and Prof. G. Hertz, of Halle. The Nobel Prize for Physics for 1926 is awarded to Prof. Jean Perrin, of the Sorbonne.

The Chemistry Prize for 1925 is awarded to Prof. Richard Zsigmondy of Göttingen and the prize for 1926 to Prof. Svedberg of Upsala.

The death of the following well-known men of science has been announced during the last quarter: Prof. J. G. Adami, pathologist and Vice-Chancellor of the University of Liverpool; Prof. F. W. Gamble, F.R.S., zoologist; Prof. J. D. F. Gilchrist, Professor of Zoology in the University of Capetown; Dr. Paul Kammerer, biologist; Mr. G. W. Lamplugh, F.R.S., geologist; Dr. Ernst Lecher, physicist, of Vienna; Mr. J. H. Mummery, microscopist; M. Ed. Naville, archæologist; Mr. A. G. Rowe, geologist; Mr. Oberlin Smith, past president of the American Institute of Mechanical Engineers.

Sir Alfred Yarrow has given a sum of £10,000 to the British Association with the condition that it shall all be expended in the course of the next twenty years.

Prof. V. H. Blackman, Prof. F. G. Donnan, and Prof. F. A. Lindemann have been appointed members of the Advisory Council to the committee of the Privy Council for Scientific and Industrial Research.

The Salters' Institute of Industrial Chemistry has awarded Fellowships to Mr. E. A. Bevan, East London College; Mr. R. M. Deanesly, University of Oxford; Mr. R. Edgeworth-Johnstone, Manchester College of Technology; Mr. H. B. Spalding, University of Oxford.

The Ramsay Memorial Fellowship Trustees have made the following awards of new Fellowships for the session 1926-7: A British Fellowship of £300, tenable for two years, to Mr. R. F. Hunter, M.Sc., Ph.D., for work at Imperial College, London. A Glasgow Fellowship of £300, tenable for two years, to Mr. J. D. Fulton, M.A., B.Sc., for work at the University of Manchester. A Swedish Fellowship of £307 7s. to Mr. Gunnar Hägg for work at University College, London. A Swiss Fellowship of £300, tenable for one year, to Dr. Max Brunner, for work at the University of Cambridge.

Reference has already been made in these notes to the scheme for the reorganisation of scientific and industrial research in Australia. The Commonwealth Council, whose formation was recommended by Mr. Frank Heath, has now been appointed. It consists of Mr. G. A. Julius (engineer), chairman, Prof. A. C. D. Rivett (chemistry) and Mr. W. J. Newbiggin (industrialist), who form the Executive Committee, and the following State representatives: Queensland, Prof. H. C. Richards (geology); New South Wales, Prof. J. D. Watt (agriculture); Victoria, Sir D. Orme Masson (chemistry); Tasmania, Mr. Keam (pastoralist); South Australia, Prof. T. Brailsford Robinson (physiology); Western Australia, Mr.

Perry (industrialist). The Council thus formed has the power to co-opt additional members—a very essential provision, since in its own selection no account is taken of the branches of science on which its members can give advice. This power has already been exercised in favour of Prof. E. J. Goddard and Prof. H. A. Woodruff representing biology and veterinary science respectively. In view, however, of the very wide scope of the activities of the Council it is rather remarkable that none of its members is a physicist. This defect will be remedied, no doubt, by the addition of another co-opted member later on.

The economic importance of forestry is coming more and more into prominence and to assist in its technical development in Great Britain a Society of British Foresters has been formed. The first President is Mr. R. L. Robinson ; Vice-President, Prof. R. S. Troup, and Secretary, Mr. R. Angus Galloway, 8, Rutland Square, Edinburgh. Membership of the Society is open to those engaged in forestry and its allied sciences. There is, of course, already in existence the Empire Forestry Association (H.R.H. Prince of Wales, President), which publishes a most excellent Journal, and presumably the new Society will co-operate with the Association, the councils of both having several members in common.

The zone of totality in the eclipse of the sun, which takes place on June 29 next, crosses England from Southport to West Hartlepool. The duration of totality is only 25 seconds, and occurs about 5.30 a.m., *i.e.* about one and three-quarter hours after sunrise.

Prof. F. Erhenhaft has put forward new experimental evidence in support of his contention that it is possible to detect smaller quantities of electricity than the electron. This is, apparently, based on the drift of ultramicroscopic particles of selenium in a magnetic field.

Prof. F. Paneth and Dr. K. Peters have communicated a paper to the German Chemical Society describing certain experiments they have made which appear to have resulted in the transmutation of hydrogen into helium (*Berichte*, September 1926). Hydrogen was exposed to the action of finely divided palladium for periods of the order of twelve hours. The hydrogen was then burnt off with oxygen, the water vapour formed and the excess oxygen were removed with charcoal and the residual gas passed into a fine capillary tube for spectroscopic examination. Four or five of the helium lines were observed and the amount of helium present seemed to be proportional to the time during which the hydrogen had been exposed to the palladium. Elaborate precautions were taken to prevent access of helium from the atmosphere and to ensure that none was present initially in the palladium itself.

The amount of helium formed was very minute ; so minute that the amount of energy that should have been liberated as a result of the decrease of atomic mass, was estimated to be only a quarter of a calory—an amount which it would be most difficult to detect under the conditions of the experiment, even if it was all set free as heat and not, as is probable, as short wave radiation.

The main cause of the spontaneous combustion of coal is most often direct action of the oxygen of the air on the coal substance, and it is a matter of importance to determine which constituents of the coal conglomerate are most readily attacked by oxygen. Two papers recently published by the Safety in Mines Research Board deal with this point. The first (Paper No. 26) contains an examination of the various types of pyrites in coal and a laboratory study of the rates at which these various varieties oxidise. The results show that the probable influence of the pyrites on the spontaneous combustion of coal depends very largely on the form in which the pyrites occurs. The second paper (No. 28) deals with the more general problem of the determination of the most readily oxidised of the coal constituents. The experiments showed that the degree of liability of a particular coal to spontaneous combustion can be estimated by determining the proportion and character of the ulmin compounds that it contains. (These papers are obtainable from H.M. Stationery Office, Adastral House, Kingsway, price 1s. each net.)

We have received a copy of the second number of a new monthly, *Continental Metallurgical and Chemical Engineering*, edited and published in Berlin, but written in English. It contains a number of useful and interesting articles relating to large-scale chemical processes, a review of current literature, a section devoted to commercial topics, and a large number of short notes. The future of the continental metal trade is discussed by Dr. Alfred Marcus. He considers that the peculiar position of England as a clearing-house for its dominions and colonies exercises a favourable interest on its metal trade and that the future outlook appears much more favourable in England than on the Continent. But "There can be no doubt that the relative position of London to New York to-day is the same as that of New York to London before the war, because the fate of the important movements on the metal market is more or less decided in New York, even though London may occasionally be able to take the lead. If, quite frequently, London is the cause of some movement, New York will always determine the final result. This fact has best been proved by the relatively small effect of the English mining strike on the international metal markets. How different

would have been the consequences of a strike of American miners of similar extent and duration on the international markets ! ”

Dr. Marcus considers that in comparison with pre-war conditions and in view of the high figures of production and comparatively low consumption, the price levels of lead and zinc are clearly excessive. No mention is made of tin, which is “talked up” to £350 per ton next year, or mercury, the high price of which is of much concern to those who buy it for laboratory purposes.

Among the shorter articles is one dealing with materials for internal combustion engine valves. Tests on valves made of different kinds of steel were made by Mahoux (*Revue de Métallurgie*, 22, pp. 39-51). Those made from high-speed steels containing chromium, tungsten, and vanadium in different proportions gave good results, but by far the most efficient of all the alloys tested was sil-chrom. This has the composition carbon, 0.44 per cent.; chromium, 9.04 per cent.; silicon, 2.67 per cent.; phosphorus, 0.012 per cent.; molybdenum, 0.17 per cent., together with traces of sulphur and vanadium.

The Government of India Report on Education in that country for the year 1924-5 contains some interesting and important remarks concerning the relations between Indian and English universities and the Indian student in England. On the former point it states that “Some of the *newer* universities have found difficulty in obtaining recognition of their degrees by English universities . . . there is no doubt that the recent rapid increase in the number of universities in India, the absence of any authority for co-ordinating university standards, and the very mediocre attainments of some of the Indian graduates who have proceeded to England account for this hesitation on the part of the English university authorities. The London University, for example, has recently warned one of the *older* Indian universities that it can no longer accept its graduates without question, but must in future consider each application on its merits.” (The italics are ours.) Again : “Complaints are sometimes heard that Indian students who now come for training in this country [*i.e.* England] are of inferior calibre and qualifications compared with those who came a generation ago. . . . Sir Atul Chatterjee has, however, observed with great satisfaction that among those who now seek training here there is a very considerable proportion of serious and earnest students who have already done well at the colleges and universities in India and who do credit to themselves and their country during their career in England.” This, of course, is quite true; but the fact remains that, at the present time, the possession of a degree awarded by an Indian

university gives no indication of the knowledge or ability of the holder thereof as judged by standards in this country. The matter is one which deserves the most serious consideration by the Indian authorities.

The Report states that there are between 1,500 and 2,000 students studying in the United Kingdom. Of these, 583 were reading at the Inns of Court, 360 at London, 224 at Edinburgh, 117 at Cambridge, and 86 at Oxford.

The current number of the *Empire Forestry Journal* (vol. 5, No. 1) contains, as usual, a large number of articles interesting as well to the layman as to the forester himself. They include details of work in Western Australia, New South Wales and Victoria, Uganda, Burma, and East Africa. East Africa is represented by an account of the cedar *Juniperus procera*, which the writer, Mr. Gardner of Kenya Colony, calls the East African Pencil Cedar. It appears that supplies of the American pencil cedar are almost exhausted and that substitutes are being sought for far and wide. The Kenya product is not a substitute but the genuine thing, alike in odour and whittling properties to the original pencil cedar. Its use is, however, making slow progress, partly because the producers have put on the market insufficiently seasoned wood improperly selected as regards grain and partly because it has not been possible to give manufacturers any exact information as to the quantities available. These difficulties will in due course be overcome, and the tree will find its right place as one of the Empire's most valuable timbers. It is to be hoped that British users—pencil manufacturers, cabinet-makers, and others—will begin to take a serious interest in the product before the trade becomes entirely directed to foreign countries.

We have received from Messrs. William Collins & Sons, Ltd., the well-known diary specialists, a copy of a new diary compiled specially for engineers. The pages are, approximately, 3 in.  $\times$  5½ in., and in the diary portion a double page is devoted to each week. The paper is good, and the space for notes, cash accounts, etc., ample. The engineering data are given on 128 pages of thin india paper, and the publishers state that this "special information has been prepared by experts, reviewed by practical engineers, and approved by the Professor of Engineering at one of the largest Technical Colleges in the country. Every care has been taken to see that the information given is thoroughly sound and reliable; our object being to provide a diary that will be of standard value and regarded by engineers of all branches as a vital annual, convenient for the pocket and well arranged for quick reference purposes." It would appear that this object has been attained; certainly the compiler has endeavoured to cover every branch of engi-

neering in the relatively small space at his disposal. We have tested several of the tables and found only one of them at fault, namely, that headed "Useful Temperatures" on p. 39, which contains a number of grotesque errors (*e.g.* greatest artificial cold =  $-166^{\circ}$  F.; water (*in vacua*) boils at  $98^{\circ}$  F.). The book contains a good index, so that any table required is easily found, and at the end are sixteen pages of squared paper—a most useful addition. The diary is supplied in various bindings; the sample sent to us is in pig skin, and its price is 3s. 6d. net.

We have received from Messrs. Longmans & Co. a copy of the fifth edition of Kaye and Laby's well-known *Tables of Physical and Chemical Constants*. Some of the tables have been brought up to date, notably that on isotopes, but the alterations are relatively few. One point, however, demands attention. The price is still 14s. net. When it first came out in 1911 it cost only 4s. 6d. The purchasers to-day are mostly students who can ill afford the present greatly enhanced figure. We wish to draw the attention of Messrs. Longmans to this point, because in certain other cases they have been most helpful to students in keeping down the price of essential textbooks.

Optimism and pessimism regarding malaria control are both common, the latter especially in British territory and the former in America. Thus in the transactions of the *Fifth Conference of Malarial Field Workers* issued by the Treasury Department, United States Public Health Service, Washington, D.C., we find at the end a note by Dr. C. H. Kibbey, Director of Sanitation, Tennessee Coal and Iron and Railroad Company. He remarks: "I hope to live to see the day when the entire south will be as free from malaria as it is free from yellow fever to-day. We have demonstrated, time and again, that malaria may be eradicated from a limited territory by anti-mosquito methods. It is only necessary to enlarge the territory in order to make malaria no more than a name throughout the whole South. . . . I have absolutely no patience with that man who permits himself to see things from the angle of the pessimist. . . . The fellow who believes that he has a problem in malaria control which cannot be solved is probably correct in so far as he is concerned, but he need look no further than under his own hat for the reason." Quite true.

Sir Ronald Ross left England on October 29 for the Federated Malay States, where he has gone to inspect the great work against malaria which has been carried out by Sir Malcolm Watson and other medical men during the last quarter of a century—a work which is really as distinguished as that of Gorgas in Panama, but has been conducted merely by

private and not by national funds. Thence, he hopes to proceed to Calcutta, where he will probably make similar inspections, will possibly deliver some lectures, and will unveil a medallion portrait of himself which is to be erected in the grounds of the old laboratory at the General Hospital in which he completed his malaria researches in 1898. He may be able to return via Madras and Colombo in February or March next. The expenses are being defrayed by the Indian Tea Association, and his visit has been organised by the new Ross Institute for Tropical Diseases at Putney Heath.

The following notice has been received :

“ On February 21, 1927, two hundred and fifty years will have passed since Spinoza's death, and it is proposed to hold then a memorial gathering at The Hague which will be addressed by foremost thinkers from different parts of the world. The presence of all interested is invited.

“ A special committee has been formed for the purchase of the house in the Pavilioensgracht in which Spinoza lived his last years. It is hoped to hold the memorial assembly in the house and to inaugurate it formally as the Spinoza House.

“ Further particulars will be published in due course in the public press.”

## CORRESPONDENCE

### " ON THE WRITING UP OF SCIENTIFIC PAPERS "

*From* PROF. CHAS. H. O'DONOGHUE

*To the Editor of* SCIENCE PROGRESS

DEAR SIR,—We read with considerable interest the article under the above title by Dr. E. P. Farrow, that appeared in the number of this Journal for July 1926. For some time we employed a very similar method, but from experience in different types of work we were led to make several modifications of it. These relate more especially to the method of making the notes ; and we hope they will not be without interest and use to others, particularly those who work in the field or on expeditions.

In the first place the notebook employed, whether in the field or in the laboratory, is of the loose-leaf type. Secondly, it is provided with a thin metal sheet cut the same size and perforated in the same way as the paper. This is made of zinc ; aluminium would probably be just as good, although it would have to be thicker to give the same stability ; tinned iron is not satisfactory, since it is very liable to rust, a most important consideration when dealing with fresh water or sea work. Secondly, the notebook is provided with a supply of good carbon sheets similar to those used by shopkeepers to duplicate their bills. It is an easy matter to cut both the zinc and carbon sheets to the right size ; and the addition of perforated, circular cloth patches, procurable at any stationers', adds greatly to the life of the latter. The notes are written in indelible ink pencil and a duplicate taken. It was found that this utilisation of a metal sheet provided a first-class writing surface, and avoided the difficulty of carrying and using ink, often a trouble on a long trip or under wet conditions. Moreover, on one occasion when the pencil also was lost carbon notes were taken with a sharpened piece of wood.

On returning to the laboratory or camp from an expedition, the notes are separated out and the carbon copy placed on file. Any of the pencil copies which contained information that did not need to be checked could also be left behind, and by

adopting a consecutive numbering of the sheets no difficulty is encountered subsequently in filing the copies. The sheets for each day are dated, and the first observations are those, such as locality, weather conditions, state of tide, physico-chemical observations on the water, and so on, which will apply to all the notes following and prevent reduplication.

Now, this method is admittedly a little more complicated than that suggested by our friend Dr. Farrow, but offers several advantages. Firstly, the filing of the original copy from time to time ensures a certain amount of safety, for when working on horseback, in a canoe or on a rolling boat at sea, the losing or wetting of a notebook is by no means an unknown contingency. Secondly, while this safety is ensured, it still allows the taking into the field of such notes as need to be re-checked. Thirdly, when one set of notes is cut up for sorting, the other set remains intact, and if each slip is provided with simply a page number it is easy to refer to it in the complete file. Moreover, the preservation of a set of notes in its entirety is a very useful feature, since it retains the "associational atmosphere" under which the set of notes were taken, which is totally destroyed when the only set of notes available has been cut up. Further, it makes it easy to get the sequence of the various notes on one animal, species, or set of phenomena without the necessity of dating and duplicating information on each slip as it is cut. Lastly, owing to the multiplicity of loose-leaf systems, *i.e.* books, folders, files, types of paper, etc., now available, it enables anyone with the minimum amount of inconvenience to choose whatever size or type is best adapted to his own particular needs.

We can illustrate the third advantage given above by an example. Supposing a Nudibranch were collected on a certain date, together with egg masses, then duplicate notes would automatically be made. When the observations came to be written up later—that is, after the cutting up and sorting out of the notes—it might be advisable to enter into the question a little more fully, and a reference to the original notes might prove of great value. For instance, one could tell at once all other animals and plants noted at the same time, the date, time, weather, and tide conditions, etc.; and finally the totality of the observations would provide the background for a much more complete mental picture of the entire surroundings of the animal than would otherwise be possible.

To the explanation of how to deal with the separate notes we have little to add, save that in some types of work we have found it convenient to paste all the note slips made in the field and in the laboratory, and relating to one limited object or topic, on loose-leaf pages of standard writing size, *i.e.* about

$8\frac{1}{2} \times 11$  inches. It is then possible to add to them duplicate prints of photographs or curves and tracings of drawings. This brings together one unit, which it is not difficult to cut up again and reassemble if necessary.

Lastly, if an extensive bibliography and reference list is involved, the advantages of separate slips is very obvious.

After all, the whole procedure is simply a matter of adjusting the "card index system" that is now becoming almost universally used in all professions and businesses requiring the registry and classifying of a number of observations. In spite of its obvious value, our own experience has been that it is not nearly so widely used by scientific men as it might be, and we think Dr. Farrow is to be thanked not only for calling attention to its possibilities, but also for showing how it can be adapted to meet scientific needs.

Yours faithfully,

CHAS. H. O'DONOGHUE.

University of Manitoba,  
Winnipeg, Canada,

September 14, 1926.

## THE INDUSTRIAL MOVEMENT OF COLOUR

From WILHELM OSTWALD, DR. HON. C. CAMBRIDGE, LIVERPOOL, ABERDEEN, TORONTO.

*An den Herausgeber von SCIENCE PROGRESS*

SEHR GEEHRTER HERR,—Eben kommt der Aufsatz *The Industrial Measurement of Color*, von Hrn. W. G. Raffé, S. 662–720 im April-Heft Ihrer Zeitschrift mir zur Hand. Ich kann die Ausführungen dieses kenntnisreichen und klar urteilenden Farbenforschers fast Zeile für Zeile unterschreiben, was die Notwendigkeit einer Ordnung, Messung und Bezeichnung der Farben anlangt. Nur muss ich hinzufügen, dass das, was er als Wunsch und Hoffnung ausspricht, bereits vollständig Wirklichkeit geworden ist. Es gibt eine streng wissenschaftliche Ordnung der Farben, welche auf der vor 10 Jahren entdeckten Möglichkeit beruht, jede Farbe zu messen und sie als die Summe dreier Komponenten, nämlich Reine Farbe, Weiss und Schwarz darzustellen. Auf Grund der Messungen und unter Benutzung des Gesetzes von Weber-Fechner haben sich mit einem Minimum von Willkür Normen aufstellen lassen, deren Anzahl innerhalb der praktisch notwendigen Gebiete 700 bis 1,000 beträgt. Diese Normen sind auf Papier und auf Wolle praktisch ausgeführt worden und werden in Deutschland von vielen Industrien benutzt, für Textilstoffe,

Teppiche, Tapeten, Porzellan, Buchdruck, Gärtnerei. Auch in der Wissenschaft (Psychologie, Zoologie, Mineralogie, Chemie) haben sie Anwendung gefunden. Zur Bezeichnung der Normen dienen Zeichen von der Form  $8\text{ ge}$ , wo die Ziffer den Farbton (Ort im Farbkreis), der erste Buchstabe den Gehalt an Weiss und der zweite Buchstabe den an Schwarz bezeichnet.

Der Hauptpunkt, welcher diese Fortschritte möglich gemacht hat, ist die Erkenntnis, dass die drei Variablen der Körperfarben nicht wie bisher allgemein (und auch von Herrn Raffé) angenommen wurde, Farbton, Reinheit und Helligkeit sind, sondern Farbton, Gehalt an Weiss und Gehalt an Schwarz. Die Reinheit oder der Gehalt an reiner Farbe steht mit den Gehalt an Weiss und Schwarz in gesetzlicher Beziehung, welche durch die Gleichung dargestellt wird:

$$\text{Reine Farbe} + \text{Weiss} + \text{Schwarz} = 1.$$

Diese Gleichung hat für die Farbenlehre etwa dieselbe Bedeutung, wie das Daltonsche Gesetz für die Chemie.

Ich schicke Ihnen gleichzeitig einige Drucksachen, welche Ihnen eine vollständigere Vorstellung von der neuen quantitativen Farbenlehre geben werden, als es in einem Briefe möglich ist, und hoffe, dass Sie hier den Weg sehen, auf welchem die von Herrn Raffé bezeichneten Aufgaben teils schon gelöst sind, teils unmittelbar gelöst werden können.

Ihr ganz ergebener,  
W. OSTWALD.

Grossbothen, Germany,  
August 19, 1926.

## ESSAY-REVIEW

### **THE GROWTH OF PROTOZOOLOGY AS A MEDICAL SCIENCE.**

By JOHN GORDON THOMSON, M.A., M.B., Ch.B., Director, Department of Protozoology, London School of Hygiene and Tropical Medicine, being a review of **Protozoology**, by C. M. WENYON, C.M.G., C.B.E. [Pp. xvi + 1563.] (London: Baillière, Tindall & Cox, 1926. Price 84s. net.)

OF late years it has become increasingly obvious to all those who study the literature that, with our ever-growing knowledge of medical zoology, specialisation in one or other of its branches is almost imperative. Protozoology, entomology, and helminthology have each assumed an individual importance, which has resulted in their being regarded as separate subjects for purposes of research and teaching. The justification for this specialisation is now seen in Dr. Wenyon's two recently published volumes entitled, *Protozoology, a Manual for Medical Men, Veterinarians, and Zoologists*. In these volumes the author has embodied the accumulated knowledge of a lifetime devoted to the study of the unicellular animal, the simplest form of animal life, the precursor of the more complex metazoan, and, at the same time, the parasite which may live upon and in the human body. Hence this book has a dual appeal both to scientist and medical man, and Wenyon, with the added comprehensive knowledge which is of such importance to a protozoologist engaged in the study of parasitic forms, has endowed the literature with a work which will rank as a standard text-book.

Historically it would seem that the original student of protozoology was Antoni van Leeuwenhoek (1632-1723). He, while working with a primitive microscope, discovered not only the first free living protozoon but also an intestinal parasite of man. He, accordingly, may be regarded as the father of medical protozoology. The years have rolled past since this original discovery of a parasite in man, and, just as in other sciences, so here, many workers gradually and laboriously have added to our knowledge. The greatest stimulus of all was initiated by Laveran in 1880 and Ross in 1898. The former found the causal organism of malarial fever, and the latter demonstrated the rôle of the mosquito in its transmission.

It is, therefore, over a quarter of a century since the foundation of our knowledge of human parasitic diseases was soundly laid by these two great observers. From this point onwards diseases in man and domestic stock have been studied from quite a new point of view. As a subject of investigation malaria has attracted most attention, because of its prevalence in most tropical areas in course of development by European races. Its eradication or amelioration was indeed a *sine qua non* of effective colonisation and development of the tropics. Possibly the best example of this is the building of the Panama Canal. After the French had spent millions of pounds and had lost thousands of lives, Gorgas, the famous American military surgeon, and his fellow workers, profiting by the discovery made by Ross, were able to apply hygienic measures to prevent the spread and dissemination of mosquito-borne diseases. As a result, the completion of the canal was made possible, and man triumphed over his microscopic enemies. Nevertheless, it must be remembered that many problems associated with this disease still require solution. In parenthesis it is interesting to remark that benign tertian malaria as an aid in the treatment of general paralysis of the insane has given remarkably good results, and, incidentally, the experimental inoculation of the parasite into man is furthering our knowledge regarding malaria in general.

Improved microscopes, the production of high-power objectives, and the introduction of new fixing and staining methods, have helped the modern scientist in the study of the minute detail of cell structure. The progress of knowledge has been gradual. Many brilliant and patient workers have added their observations, not hesitating "to scorn delight and live laborious days" in their endeavour to add one link to the chain of evidence. Mistakes have been made and wrong evidence brought forward, but even these errors often stimulated the final elucidation of the truth. Bit by bit the workers have added to the foundation laid by an original discovery the many bricks necessary to complete the edifice, or, rather, almost complete it, for it would seem there is little finality in research. The elucidation of one problem merely clears the way for the approach to the next. It is perhaps natural that, in the history of research in human diseases, those who laid the foundation-stone or made the initial discovery should receive the greater fame, but we would emphasise the fact that much of our accurate knowledge of the parasitic protozoa of man is due to the work of keen and able observers who have laboured in comparative obscurity.

In Wenyon's book we have unfolded, as it were, the romance of the discovery of the parasitic protozoa, their life histories,

and their relation to disease in man and other animals. When it is realised that there are ninety-seven pages of references to the literature and sixty pages giving a complete list of hosts of blood parasites of vertebrates and the trypanosomidæ of invertebrates, we can gather some idea of the enormous scope of the book.

To indicate a few of the chief discoveries of recent years, the story of amœbic dysentery may be cited first as an interesting example. Loesch, in 1875, found what was undoubtedly the causal organism of human amœbic dysentery. Previous to this, however, Lewis in 1870, and Cunningham in 1871, had found amœbæ in man which were possibly the harmless commensal *Entamœba coli*. Owing to the fact, now well known, that there are two distinct species parasitic in the human intestine, a period of confusion followed during which observers disagreed. As a result of the observations of Kartulis (1887) and others, it was found that an amœba of man was pathogenic in that it could invade the tissues of the intestine and also the liver. Schaudinn (1903) appears to have been the first to appreciate clearly the fact that man could be parasitised by two distinct amœbæ, one of which was harmless and the other pathogenic. Finally, Walker and Sellards (1913) proved conclusively in a classic experiment on human beings that Schaudinn's view was correct. Now five species of amœbæ are known in the intestine of man, but only one of them is pathogenic, viz. *Entamœba histolytica*. There is no difficulty in differentiating these with accuracy, and, further, recent observations on cultural methods evolved by Cutler, and by Boeck and Drbohlav, have added a means of studying more accurately the life histories of these amœbæ.

To pass on to another important disease, trypanosomiasis, Bruce in 1895 demonstrated that the disease of cattle in Zululand known as Nagana was caused by a trypanosome. Seven years later Forde and Dutton noted a similar parasite in the blood of man on the west coast of Africa. This latter observation startled the scientific world, and commissions were despatched to Africa to investigate sleeping sickness. It was not clearly understood what the causal organism of this disease was, but Castellani, in 1903, found trypanosomes in the cerebro-spinal fluid of certain cases, and later, Bruce and Nabarro, in the same year finally established the relationship between this organism and sleeping sickness. Kleine, 1909, demonstrated a cyclical development in the tsetse, an observation confirmed by Robertson. The current opinion points to the possibility that the trypanosomes of game and domestic animals—that is to say, the polymorphic group—and those of man are physiological variants of the same species, but this is not yet definitely established.

One of the most fascinating chapters in the history of tropical diseases is that which deals with leishmaniasis. The causal organism was discovered by Leishman in 1903, and independently by Donovan in the same year. The following year Rogers noted that it became a flagellate in citrated blood, indicating that it was probably transmitted by an insect. Until last year the method of transmission had baffled all workers. For a long time the bed bug had been incriminated as the probable vector, but is no longer regarded as the culprit. Sinton drew attention to the fact that the distribution of the disease kala azar in India and of a certain species of sandfly, *Phlebotomus argentipes*, were closely related, and, acting on this information, Knowles and his colleagues last year conducted experiments suggesting this was probably the case. As a result, a commission of medical men from the I.M.S. have shown recently that sandflies of the species quoted above can be infected by feeding on kala azar cases, and the establishment of the fact by transmission experiments is simply awaited.

It would seem also that the problems connected with the transmission of oriental sore are approaching solution. Here, again, sandflies are incriminated, the experimental observations of Adler and Theodor made in Palestine during 1925 and 1926 pointing to the fact that *Phlebotomus papatasi* is the vector. Kala azar occurs round the shores of the Mediterranean, and its distribution more or less coincides with that of a similar disease in dogs. This fact led to what is probably an erroneous conception that fleas might act as the transmitting agents. Researches were complicated by the occurrence of natural leptomonads in the gut of these insects, which were confused with the developmental forms of *Leishmania*.

It is now many years since Smith and Kilborne (1893) published an account of the transmission by ticks of Texas fever or Red Water fever in cattle. They noted that the infection could pass through the egg of the tick to another generation of ticks—a remarkable discovery. Since this event various transmitting agents of disease of man and animals have been carefully studied, with the result that the rôle of blood-sucking invertebrates in the transmission of protozoal disease has been explained accurately. Flagellates of the trypanosome family are found as natural parasites of fleas, lice, mosquitoes, etc., and these organisms were studied in relation to similar stages in the developmental forms of leishmaniasis and trypanosomiasis. Interesting leptomonads have been found in the latices of certain plants and in the gut of plant-sucking hemiptera. All this opens up the interesting problem of the evolution of parasitism.

In conclusion attention is directed to the many difficulties

associated with the examination of minute unicellular animals. Artefacts, pseudoparasites, and coprozoic organisms have been and will continue doubtless to be encountered by research workers. The presence of these protean objects necessitates long experience before the student can be considered an expert, and years of study may be devoted to intestinal organisms before the degree of skill is obtained necessary for the recognition of the various objects seen. Quite recently three species of coccidia considered even by authorities as true parasites of man have been shown simply to be parasites of fish ingested with the food. The oöcysts pass unchanged through the human intestine, and thus act as temporary foreign bodies. While parasitic protozoa of man and animals form, as it were, a group of organisms which can be specially studied, it is obvious that some knowledge of their free living relatives in water and in soil is necessary before an intelligent conception can be formed of the manner in which they gradually became parasitic. Wenyon has not overlooked this important fact, and by a judicious choice of free living types has endowed the subject as a whole with further interest.

## REVIEWS

### MATHEMATICS

**Analytic Functions of a Complex Variable.** By D. R. CURTIS. [Pp. ix + 173.] (Chicago: The Open Court Publishing Company. Price 10s. net.)

THE author says in his Preface: "What is attempted here is a presentation of fundamental principles, with sufficient details of proof and discussion to avoid the style of a mere summary or synopsis. . . . For almost every topic the reader is given several references." This programme is well performed; and though the little work contains only 173 pages, it gives one of the clearest and yet brief expositions of its theme which we know of. It will be specially useful to students who have not yet wandered in the field of "imaginary" numbers, or who have not done so with sufficient enterprise. The principal chapters are on the origin and applications of the theory, complex numbers, real functions of real variables, complex functions that have derivatives, applications in geometry and physics, integrals of analytic functions, infinite series, singularities of single-valued analytic functions, and many-valued analytic functions and index.

R. R.

**Analytic Geometry.** By MARIA M. ROBERTS and JULIA T. COLPITTS. Second Edition. [Pp. xii + 261.] (New York: John Wiley & Sons; London: Chapman & Hall, 1926. Price 9s. net.)

AN admirably printed and very clearly written elementary work, with sufficient figures and a considerable number of exercises. The most important curves are expounded in separate chapters. Chapter XII deals with the general equation of the second degree, and the next two chapters deal with transcendental and parametric equations and with solid analytic geometry. The book will be useful not only to students, but to men of science who may be called occasionally to refer to a particular curve. Some of the latter will possibly be surprised, or even offended, with the modern method of exposition—which is, not to strain the student's intelligence to the utmost by difficult rigid demonstrations inflicted on him as exercises, but to lead him easily from step to step, especially by means of frequent numerical examples. Each section consists of enunciation, proof, and exercises, and attempts only sufficient matter for the purpose of the book.

### PHYSICS

**Magnetism and Atomic Structure.** By E. C. STONER, Ph.D. [Pp. xiii + 371, with 63 diagrams.] (London: Methuen & Co., 1926. Price 18s. net.)

IN reviewing a volume of a German treatise some short time ago, the reviewer stated that he was aware of only one English text-book which gave a comprehensive and up-to-date survey of the work in a large branch of physics. It is, therefore, a pleasure to record the publication of Dr. Stoner's excellent book on magnetism and atomic structure.

Dr. Stoner commences his work with a short history of the development of our knowledge of magnetism, which is very pleasant to read. Then, following a few pages of introductory mathematics, he describes the fundamental methods of determining the magnetic susceptibilities of weakly magnetic substances, particular attention being devoted to the work of Curie and to the results of earlier experiments. Under the heading of the interpretation of magnetism as an atomic phenomenon, accounts are given of the Ewing theory of ferromagnetism, Langevin's theories of paramagnetism and diamagnetism, and the molecular field theory due to Weiss, the difficulties which these theories have to face being clearly indicated. Dr. Stoner's own work on the distribution of electrons in the atom and on the applications of the quantum theory are so well known that comment on the chapter dealing with the quantum theory is unnecessary, and his review and discussion of the more recent experimental results with paramagnetic and ferromagnetic substances, so many of which are inaccessible in the English tongue, will appeal very much to serious students of magnetism. He devotes a chapter to a discussion of the gyromagnetic effect, describing in detail the experiments of Chattock and Bates. The reviewer, however, would like to suggest that the work of Sucksmith, who employed a very neat method designed by Chattock, is worthy of greater notice; for his measurements were made by a method which, although not direct, was entirely different to that used by Chattock and Bates, and the confirmation of their results was therefore particularly valuable.

The experiments of Gerlach and Stern, the Zeeman and other magnetoptic effects are all adequately treated. A chapter on diamagnetism contains accounts of the work of Pascal, Oxley, and Soné. Galvano-magnetic and magneto-caloric effects are briefly treated, and the book finally closes with a series of chapters on the important bearing of magnetic data on the elucidation of the structure of the atom and on chemical combination. Throughout the book references are everywhere complete, and in addition, concise statements of the particular importance and the subject matter of many of the more important references are given. It is therefore clear that Dr. Stoner has given us a very important book of reference, which will undoubtedly prove invaluable to all interested in magnetic research.

L. F. BATES.

**Atomzertrümmerung.** By HANS PETTERSSON and GERHARD KIRSCH. [Pp. vii + 247, with 61 figures and 1 plate.] (Leipzig: Akademische Verlagsgesellschaft, M.B.H. Price 15 gold marks.)

THE main purposes of this book are to give in a convenient form the results of investigations which have been made on atomic disintegration in Cambridge and Vienna, and to attract the attention of other workers outside these universities in order that they may help in solving the complicated problems of the structure of the atom.

The authors describe the methods and results of all experiments recently carried out in this domain of physics. In some cases the results obtained in the Vienna Institute are not in agreement with those obtained in the Cavendish Laboratory, and the discrepancies are in part attributed to the differences in the optical arrangements used for scintillation work in these places. Pettersson has advanced the hypothesis, known as the explosion hypothesis, that an X-ray on striking the nucleus of an atom, produces an effect which results in the instability of the combined system of X-ray and nucleus, so that a kind of explosion takes place, during which a portion, or portions, of the nucleus may be expelled. The authors consider that this hypothesis explains the available experimental data more adequately than the satellite theory of nuclear structure which has been advanced by Rutherford. The

descriptions of the methods employed in Vienna are very interesting, and a description of the determination of  $e/m$  of a proton by means of a form of mass spectrograph, designed by G. Stetter, is given. It is clear, however, that a large amount of work remains to be done before we shall have any complete conception of the nature of atomic disintegration.

L. F. BATES.

**Readable Relativity : a book for Non-Specialists.** By CLEMENT V. DURELL, M.A., Senior Mathematical Master at Winchester College. [Pp. vi + 146.] (G. Bell & Sons, 1926.)

Μέγα βιβλίον, μέγα κακόν, quoted old President Routh after stubbing his toe on a divinity folio; and Mr. Durell, mindful of this axiom of Callimachus, has produced a lucid and readable treatise in the small compass of some hundred and fifty octavo pages.

A chapter on the progress of science is succeeded by one in which the life in Dr. Wm. Garnett's convex-looking-glass world is expounded in a manner which will help the common-sense reader to realise some of the bearings of that innocent word "reality." The development of the thesis, by way of the Michelson-Morley experiment and of the difficulties attendant on the synchronisation of clocks, follows normal lines, and leads to a clear discussion of the restricted theory and its implications. Two brief chapters on the General Theory and on the Einstein tests conclude the book.

Throughout the whole argument the mathematics, while higher than that mathematics for botanists which (*teste* Sir J. J. Thomson) would "not give a headache to a caterpillar," is of a simple character; and the reader who will take the trouble to work through the collection of numerical and algebraical exercises which forms a special feature of each chapter will find many of his difficulties agreeably diminished. Altogether, Mr. Durell is to be congratulated on his effort to produce a treatise *aptus virginibus puerisque*; but when he expresses the velocity of light in *luxes*, he should remind his readers that the term "lux" is used by the photometry experts in quite a different sense.

A. F.

**Reflections on the Structure of the Atom.** By FLORENCE LANGWORTHY. [Pp. xi + 260, with 35 figs.] (London: Watts & Co., 1926. Price 12s. 6d. net.)

THE reviewer feels that these reflections are adequately summarised for the benefit of serious physicists by the following quotations:

"Our atom shows us that a K X-ray—a form of light—is a hydrogen atom."

"Since X-rays have such an ill effect on the operators thereof, it is puzzling why metals are X-rayed for flaws. For it stands to reason that what is bad for the man is bad for the metal. Our atom shows us that the atoms can be turned by the X-rays into another element—which result may be more deadly than a flaw. We may, indeed, be making many flaws in trying to discover one."

L. F. BATES.

**Methods of Measuring Temperature.** By EZER GRIFFITHS, D.Sc., F.R.S. [Pp. 203 + xii, with 99 figures.] Second Edition. (London: Griffin & Co., 1925. Price 10s. 6d. net.)

WHEN it was first published in 1918 this book was the only authoritative treatise on modern pyrometry written in this country. Though shorter than the then classical work of Burgess and Le Chatelier, it contained an admirable exposition of the theoretical side of its subject and a fully informed account

of current practice. In the new edition the book has undergone a thorough and critical revision. Additions have been made to many of the chapters, notably to that which includes description of optical pyrometers. The chapters on the heat emission of metals and high temperature melting points have been largely rewritten and a really useful index has been added at the end of the book. Methods of measuring low temperatures are still dealt with inadequately and the bibliographies at the ends of the chapters have not received much attention.

Retaining its previous character both as a scientific textbook and a practical treatise, Dr. Griffiths' work remains indispensable to all those interested in the measurement of high temperatures.

D. O. W.

**Geschichte der Physik.** By E. HOPPE. [Pp. viii + 536.] (Braunschweig : F. Viewegund Sohn, 1926. Price 30 rm.)

A HISTORY of physics of the kind which is much needed in England. It covers the whole period of scientific study up to 1895, which, as the year of the discovery of X-rays, the author selects as the beginning of modern aspects of physics; and it not only is a history, but it can also serve as a textbook or reference-book of "classical" physics to anyone having some previous acquaintance with the subject. The work is divided into three parts, the first two of which deal generally with early physics up to the end of the sixteenth century. As *laudator temporis acti*, the author is inclined to let his enthusiasm ascribe too much to the ancients, as, for example, when he states that Plato was acquainted with the phenomenon of capillarity, because in the Symposium "he mentions the experiment that the water flows over from a full beaker through an overhanging thread into an empty beaker." Others had probably observed this before, but that does not mean to say that they had a knowledge of the underlying principles.

The high position which Hero holds among ancient scientists is well shown, and it is also pointed out that many of the constructions of Leonardo da Vinci and the Renaissance mechanicians are to be found in Hero's work. The author is rather severe on the Italians of that epoch, especially on poor Galilei, to whom nothing original appears to be left except the law of falling bodies. The latter, and main, portion of the book deals with individual branches: mechanics, sound, heat, optics, electricity and magnetism in detail, commencing with the seventeenth century but with references back to earlier times when necessary. The treatment adopted is to take each concept or instrument, *e.g.* diffusion, the pendulum, as a separate sub-heading, and to trace its development through the centuries. In this way interest is kept up, and the book becomes something more than a string of names and discoveries. Of special interest are the following sections: in mechanics, the history of the barometer and the air-pump, and an account of atomic theories; in heat, the development of the thermometer (another of Hero's discoveries, ascribed to Galilei), and the cause of heat; in optics, the history of the telescope and microscope, and photometry. In the long section on electricity and magnetism full credit is given to Gilbert and to Faraday, but the important contributions of Kelvin receive rather scanty treatment. The book concludes with an account of the early electro-magnetic and electron theories.

In reading such a book, one is struck by the need for care in giving a discovery or a principle a personal name. One might easily make up a list of fallacies in this connection. Such cherished phrases as "Galileian telescope," "Boyle's Law," "Gay-Lussac's Law," are shown to be misnomers. Thus "Lambert (1728-1777) undertook a careful research on the expansion of air, using Amontons's method, and hence deduced the law named after Gay-Lussac."

The book might have been presented in better style; the addition of a few prints, the relegation of references (all of which appear in the text) to foot-notes, a more careful revision of foreign quotations and names (Bacon masquerades as "Baco von Verulam") would have improved the appearance of the text. Nevertheless, this scholarly treatise fills a gap in the history of science, and is worthy to be translated. E. G. R.

**Thermodynamics for Students of Chemistry.** By C. N. HINSHELWOOD, M.A. [Pp. v + 185, with 11 diagrams.] (London: Methuen & Co., 1926. Price 6s. net.)

MR. HINSHELWOOD is to be congratulated on placing such an excellent introduction to the study of thermodynamics within the reach of students at the price of six shillings. The text shows that the author fully realises the difficulties which confront the average student, and he has taken great pains, by the inclusion of numerical examples and special explanatory paragraphs, to remove them. This is particularly evident in the case of the sections on osmotic pressure and entropy, and wherever assumptions are made in the derivation of mathematical expressions they are carefully emphasised. Although designed primarily for students of chemistry, the book will certainly meet the needs of a large number of students of physics, and we wish it every success. L. F. BATES.

**Foundations of the Universe.** By M. LUCKIESH. [Pp. viii + 245, with 17 figures.] (London: Chapman & Hall, 1926. Price 15s. net.)

THE aim of this book is to present a popular and systematic picture of the foundations of the universe, and Mr. Luckiesh certainly gives us a very comprehensive survey of our present knowledge and its development. In this case we are not confronted with an attempt to thrust another book on the structure of the atom upon us, but with a volume which endeavours to give a clear conception of the established experimental facts concerning the nature of the universe. Thus, starting with Newton's laws and their limitations, the author deals with the kinetic theory of matter, the properties of radiant energy and the velocity of light, and then gives a short treatment of the principle of relativity and its consequences, before attempting to discuss those branches of physics which are more intimately connected with the structure of the atom. The author everywhere recognises the true relation of experimental investigation to human theories, his language is always restrained and untouched by sensationalism, and the book can confidently be recommended to the general reader. One cannot help feeling, however, that the average reader would welcome a more extensive use of diagrams and illustrations, and, incidentally, some revision of the diagrams already supplied. L. F. BATES.

**A Manual of Radioactivity** By GEORGE HEVESY, Ph.D., and FRITZ PANETH, Ph.D. Translated by ROBERT W. LAWSON, D.Sc., F.Inst.P. [Pp. xix + 252, with 42 figures.] (Oxford: Oxford University Press, 1926. Price 15s. net.)

THE German edition of this book appeared in 1923, and although the English edition follows it closely, it is by no means a literal translation of its forerunner, for many improvements have been made and the text has been brought up to date by the inclusion of all important recent work up to the beginning of 1925. For example, we find a short description and a clear illustration of Blackett's work on the ejection of a proton from the nitrogen nucleus during impact with an  $\alpha$ -particle.

This edition is extremely well produced, the chapters are concise and

adequate, and in particular those on the disruption of the elements and on the constitution of the atom are very well written. It should, however, be realised that the authors have intentionally omitted the description of manipulative processes and many practical details, on the grounds that there already exists, in the English tongue, a sound textbook on practical measurements in radioactivity. The book can confidently be recommended as a convenient textbook for all students of physics, chemistry, and medicine who desire to acquire a sound knowledge of a very extensive branch of physics, of which they need have little previous knowledge, and we feel sure that this edition will meet with the success it undoubtedly deserves.

L. F. BATES.

**Handbuch der Experimentalphysik.** Band I. Mess-Methoden und Mess-Technik. Von DR. LUDWIG HOLBORN. Technik des Experiments. Von DR. ERNST VON ANGERER. [Pp. xx + 484, with 246 figures and 1 plate.] (Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1926. Price 42 gold marks.)

THE book under review is the first volume of a comprehensive treatise on experimental physics which is to be published in seven volumes. The editors (W. Wien and F. Harms) have considered it desirable to devote this first volume to the description of the principles and methods of fundamental physical measurements and to experimental technique. Presumably, this volume is designed to avoid overlapping and repetition in the remaining volumes, whose various sections will be contributed by specialists in particular branches of physics. Consequently it cannot fairly be judged without reference to the remaining portions of the treatise, and the reviewer can therefore only indicate its contents and scope.

Dr. Holborn naturally commences his treatment of fundamental physical measurements with a preliminary chapter on units. He then devotes a large section to mechanics, in which he deals with the measurement of length, mass, volume, density, time, pressure and compressibility, in separate chapters. It may be mentioned that the chapter on the measurement of length includes the evaluation of the standard metre in terms of a standard wavelength of light, that the chapter on time measurements includes a description of the Eccles valve-maintained tuning-fork, and that the chapter on the measurement of pressure contains adequate descriptions of the Langmuir quartz fibre manometre and the Knudsen and Riegger radiometers, as well as a very clear section on high-pressure technique. The next section of this contribution is devoted to heat, and opens with a treatment of thermometry, pyrometry, and thermometric accessories, followed by chapters on calorimetry, expansion, change of state, evaporation and thermal conductivity, all of which combine to give an adequate conception of the nature of fundamental measurements in this branch of physics. One notes that the references to original work are very complete and up to date, *e.g.* mention is made of the determination of the melting-point of graphite by Ryschkewitsch and Merck this year. The third section is devoted to the measurement of electric current, resistance, potential, capacity and inductance. A comprehensive survey of the measurement of the strength of a magnetic field is given in the fourth section, whilst the fifth section deals almost entirely with photometric measurements. Again, references are numerous and up to date throughout these sections.

The second part of this volume, contributed by Dr. Angerer, contains a large amount of useful information which will appeal to all physicists who are concerned with the setting up of experiments. The particular properties of materials in common use are described in the first chapter. The second chapter is devoted to soldering, sweating, and joinery, the

third chapter to glass-blowing, and the fourth chapter to the production of thin metallic films on glass surfaces by chemical or other means. Further chapters are devoted to vacuum technique, the production of metal sheets, wires and quartz fibres, the manufacture of thermoelements, photography, electric ovens, and galvanic deposition of metals. In particular, there is a valuable chapter on the preparation of high resistances. In addition, Dr. Angerer gives the names and addresses of German firms who supply special materials.

The whole volume is excellently produced and well illustrated, and we look forward to the publication of the remaining volumes of this treatise.

L. F. BATES.

## CHEMISTRY

**Colloid and Capillary Chemistry.** By PROF. HERBERT FREUNDLICH, Ph.D.  
Translated from the third German edition by H. STAFFORD HATFIELD,  
B.Sc., Ph.D. [Pp. xv + 883.] (London: Methuen & Co., 1926.  
Price 50s.)

A TRANSLATION of Freundlich's monumental text-book has long been overdue. Although a number of large collective works in English have made their appearance since the publication of the third German edition, a text-book conceived and written by a single author is far more useful to the student than a collection of articles by various authors, however distinguished. Everybody familiar with the literature is aware that among such text-books Freundlich's work occupies a very high position. The first half is a very complete account of phenomena at interfaces; although the author does ample justice to recent developments, he does not, like many recent writers dealing with these phenomena, forget that there were strong men before Agamemnon, and that a great deal of work done before the days of unimolecular and orientated films is still of fundamental importance. The second part is devoted to colloid chemistry in the narrower and accepted sense of the term; here the author is in the first instance concerned with describing experimental facts, and he develops theories only where the present state of knowledge makes the enterprise a reasonably hopeful one. He has, of course, views of his own, and the part played by adsorption in the formation and coagulation of sols is, perhaps, overestimated; the work of investigators who take a different view is, however, generally mentioned adequately and fairly, so that the student can balance the evidence for conflicting views. The study of books, however complete, is, of course, no substitute for actual familiarity with the materials and experimental methods of a discipline, facilities for acquiring which are still quite inadequate in this country; but for those who are restricted to learning colloid chemistry from books, Freundlich will be an extremely competent guide.

The translation, while not free from constructions which nobody writing English at first hand would be likely to use, is generally clear, and gives the sense of the original. The proofs appear to have been read with quite unusual care; the reviewer has detected one mistake only, obvious to anyone with an elementary knowledge of calculus (p. 694), which has been taken over from the original. The third German edition, from which the translation has been made, was an "anastatic" reprint of the second, for which reason the new matter was added in the form of an appendix; in the translation this might perhaps have been incorporated in the text, without too much trouble to the translator and considerable advantage to the reader.

Printing and binding are excellent, and the price of the English work compares favourably with that of the original.

E. H.

**An Introduction to Industrial Chemistry.** By S. I. LEVY, M.A., Ph.D., F.I.C., with an Introduction by SIR W. J. POPE, K.B.E., D.Sc., F.R.S. [Pp. xiii + 288, with 42 figures and 16 plates.] (London: G. Bell & Sons, 1926. Price 15s. net.)

THE purpose of this book is to provide a general introduction to the subject of industrial chemistry, developing the foundations which are fundamental to all chemical industries and dealing with a few selected industries in order to illustrate the general principles and defining the boundaries of the subject rather than with a view to complete treatment.

The general plan is best illustrated by noting the chapter headings: General Principles, Process Costing and Process Costs, Large Scale Operations, General Works Equipment, a General Survey of Chemical Industries (Metallurgical, Organic, and Colloid), the Fuel Industries, Sulphuric Acid, Alkali Industry, Intermediates and Explosives, and Appendices showing flow sheets for T.N.T. Manufacture, Gun Cotton Manufacture, and Nitro-glycerine Manufacture. It will be seen therefore that a student who has fully mastered the contents of this work will have laid a very secure foundation for his future career.

Particular attention is paid to the methods of plant control by means of record sheets and the costing methods summarised in Chapter II were developed by the author in conjunction with K. B. Quinan in connection with the operation of the national explosives plants during the war and is therefore a subject upon which the author is specially qualified to write.

Whilst the reader specially appealed to is the future technical chemist, his more academically minded brother will also gain much profit by a perusal of the book and will then be able to realise better why many interesting and potentially valuable laboratory discoveries are sometimes found to be difficult or impracticable to carry out on a technical scale.

The book may be recommended with confidence to those in need of a general introduction to the methods and problems of applied chemistry.

F. A. M.

**The Chemistry and Examination of Edible Oils and Fats, their Substitutes and Adulterants.** By G. D. ELSDON, B.Sc., F.I.C., [Pp. xix + 521, with 12 illustrations.] (London: E. Benn, 1926. Price 45s. net.)

ALTHOUGH Mr. Elsdon's book is by no means unwieldy it appears to contain all that needs to be known regarding the Edible Oils and Fats, and one is chiefly struck by the vast number of little-known products which have been examined and found fit for consumption, most of which, it is safe to say, the average citizen has never even heard of.

As the author points out, the examination of oils has for long suffered the disadvantage that its methods have been largely empirical, but there are signs that this is being rapidly altered; already many valuable investigations of fundamental importance have been concluded, whilst others are in progress, and Mr. Elsdon has taken full advantage of the present opportunity to incorporate the main facts of this recent work and has enhanced the value of the book by including a large number of literature references. Besides the thirty-one chapters dealing with the general properties, chemical and physical tests of animal and vegetable fats and with their analysis, there are also half a dozen useful tables given as appendices, references to recent work in 1926, author and subject indices, and a botanical index.

The book has obviously involved a very large amount of work on the part of the author and he has been assisted by the publishers in producing a valuable and well-printed monograph which should meet with general approval.

F. A. M.

**Plant Products.** By J. HOARE COLLINS, M.Sc., F.I.C., and GEORGE REDINGTON, M.Sc. Second Edition. [Pp. xiii + 262.] (London: Baillière, Tindall and Cox, 1926. Price 10s. 6d. net.)

THIS volume is one of the Industrial Chemistry Series and appears in its second edition under joint editorship but with an abbreviated title, the words "and chemical fertilisers" having been omitted, as the latter subject is treated more fully in a companion volume. In its general form the book is essentially the same as before. In the section on soils and their properties a few brief additions have been made concerning the colloidal properties and acidity of soils. The section on Photosynthesis contains a new feature in the form of a short account of the physiology of the process. The section on Foods Fed to Beasts includes mention of Food Accessories. A good many of the subjects mentioned in the book are dealt with in a very cursory manner, which makes the value of the book somewhat uneven and several omissions are noted—such as vanilla, and peppermint, among essential oils, which are dismissed with a single paragraph of twenty lines, and cocaine and atropine among the alkaloids. There is moreover a peculiar and almost studied reluctance to mention natural sources and plant names, when given, are not always correctly spelled; thus: Pulaquium for Palaquium; moreover, the term *Arachis* oil as synonymous with earth-nut oil is omitted. In any future edition no doubt more attention will be paid to the revision of names of authors, Blackman having been incorrectly quoted in both editions as Blackwood, to mention one example only. In spite of these minor blemishes the book, however, contains a quantity of interesting information of a very varied character.

P. H.

## GEOLOGY

**Catalogue of the Machæridia (*Turrilepas* and its Allies) in the Department of Geology.** British Museum (Natural History). By THOMAS HENRY WITHERS, F.G.S., Assistant in the Department. [Pp. xv + 99, with 8 plates and 23 text-figures.] (London: printed by Order of the Trustees, 1926. Price 7s. 6d.)

THE genera *Lepidocoleus*, *Turrilepas*, *Deltacoleus* nov., and *Plumuilites* form a homogeneous group to which the name *Machæridia* has been given by the author. The group comprises two families—the *Lepidocoleidæ*, consisting of the single genus *Lepidocoleus*, characterised by two columns of plates, and the *Turrilepadidæ*, having four columns of plates and comprising the other three genera of the group. The systematic descriptions and the sorting-out of the confused interpretations of the previously known genera of the group have been carefully executed by the author. An excellent feature is a page of comparative diagrams illustrating at a glance the diagnostic characters of the four genera.

The main interest of the book, however, lies in the author's suggestive discussion of the affinities of these ancient organisms. The consensus of recent opinion has placed *Turrilepas* and its allies with the cirripedes. In disposing of the arguments supporting this view and the earlier views of the molluscan and annelidan affinities of the group, the author fully justifies his combination of these genera into a homogeneous group, the *Machæridia*, of rank still indeterminate—possibly a new group, not intimately connected to any of the established phyla. An examination of the plates has revealed a reticulation of the inner surface and a condition of fossilisation—crystalline cleavage and behaviour of the calcite as a single crystal—known hitherto only in the stereom of Echinoderms, and regarded as diagnostic for fragmentary fossils. The author evidently regards this structure as of greater value in indicating the possible affinities of the group

than the somewhat superficial analogies based on the morphology of parts, on which previous theories of the affinities of these genera were elaborated.

In a very valuable and interesting preface Dr. Bather authoritatively discusses these interesting and suggestive facts of intimate structure, and estimates their potential value as indicators of the echinodermal affinities of the group, reconciling to this view the most discrepant facts in the morphology of the *Machæridia* with similar features known to him in the group of the *Heterostelea*. A discussion of the mechanism of the evolution of the general machæridian form from the hypothetical *Dipleurula* leads Dr. Bather to the tentative conclusion that the separate stocks of the *Heterostelea* and *Machæridia* are among the earliest offshoots from the Echinoderm stem of which we have knowledge.

JOHN WEIR.

**Gems and Gem Materials.** By E. H. KRAUS and E. F. HOLDEN. [Pp. 222, with 256 figures.] (London: McGraw-Hill Publication Co., 1925. Price 12s. 6d. net.)

THIS book has been prepared from a series of lectures given to jewellers and others interested in the subject, at the University of Michigan during the past ten years. It is divided into two parts: the first dealing with those properties of minerals and principles of mineralogy which are necessary for the understanding of the subject, the second being purely descriptive. The first part includes chapters on crystal forms; the physical, optical, and chemical properties; and on the origin, nomenclature, cutting and polishing, and the manufacture of gems. The second part deals with individual gems and gem materials, and closes with a number of excellent tables summarising the various properties.

Too much, perhaps, is attempted in the first part, especially in connection with the crystallographical and optical properties. But the matter is nevertheless clearly written and well arranged, and will doubtless be of use to the class of student to which the book is addressed. The chapters on cutting and polishing, the nomenclature, and the manufacture of gems and gem materials will be of the greatest value to the scientific mineralogist.

In the descriptive matter the only serious error is the statement that quartz is the most common mineral in the earth's crust, which is inexact except on the most elastic definition of the term "mineral." Some of the photographic illustrations are of little value, *e.g.* that of marble (Fig. 47, p. 21), olivine (Fig. 183, p. 135), and others. These are not really photographic subjects, at least in black and white, and are blemishes on an otherwise excellent and informative series of illustrations.

The book is well got up, comparatively free from misprints, but is printed on the heavy, shiny, and odorous paper which is favoured by some publishers. We believe that it will take its place as the foremost popular textbook on gem stones and gem materials.

G. W. T.

**Cawthron Lectures, Vol. II. No. 1. The Geology of Nelson.** By Dr. P. MARSHALL, M.A., F.G.S. **No. 2. The Coming of the Maori.** By TE RANGI HIROA (Dr. P. H. BUCK). (No. 1, pp. 1-15, 13 plates; No. 2, pp. 17-56, 6 plates, 2 maps.) (Nelson, New Zealand: R. W. Stiles, 1925.)

To judge from indirect references in the first of these lectures, the object of the Cawthron Institute, so named after its founder, is to further scientific research, partly at least through the medium of public lectures such as those published in the volume under review.

The Nelson region contains one of the most remarkable masses of ultra-

basic igneous rocks in the world, namely, that of Mt. Dun, which has provided petrology with one of its most familiar rock names, *dunite*. Dr. Marshall's lecture is a simple, clear, and most welcome résumé of the geology of this important, but still little known, region.

The second paper of the volume is a scholarly history of the coming of the Maori people to New Zealand, the material of which has been derived from the elements of tradition, culture, language, and physical anthropology. There seem to have been two waves of migration, one from the western Pacific (the Maruiwi), and the Moriori. The latter have long genealogical tables and an elaborate history of events in Hawaiki, which suggests their derivation from Eastern Polynesia. The paper is illustrated by several plates showing racial types, and by an anthropological map of New Zealand.

G. W. T.

**The Surface-History of the Earth.** By J. JOLY, Sc.D., F.R.S. [Pp. 192, 11 figures, 13 plates, 1 map.] (London: Oxford University Press, 1925. Price 8s. 6d. net.)

It is hardly too much to say that Prof. Joly's new book, embodying his illuminating theory of the connection between the geological cycle and the thermal cycle of the earth's crust, inaugurates a new era in geological science. Geological history is dominated by a rhythm, albeit a complicated one; and Prof. Joly's theory is the only one which comes anywhere near a convincing explanation of this periodicity. We believe we have now a key which will ultimately solve all sorts of riddles concerning the tectonics and igneous activity of the earth's crust.

Starting from the facts of isostasy and the data yielded by seismology and other branches of geology, Prof. Joly first seeks to establish on a firm foundation the now well-known view that the continental masses are composed of acid rocks (*sial*), and are floating, eight-ninths submerged, in a heavier basaltic substratum which constitutes the floor of the oceans. Then he turns to the thermal history of the earth, showing that if the crustal rocks contain as much radioactive material as is possessed by representative samples, a continuous generation and accumulation of heat must take place. The heat generated in the continents goes chiefly to maintain the known loss by radiation (the temperature gradient), and to raise the temperature of the continental bases to that of the fusion point of basalt; but the heat developed in the basaltic substratum must accumulate. Owing to the blanketing effects of the heated continents, that part of the basaltic substratum immediately beneath a continent must ultimately fuse; and if no means of relief are afforded, catastrophic effects must ensue. Similarly the basaltic layer must fuse at a certain depth beneath the oceans floors.

When fusion becomes sufficiently widespread, a slow westerly drift of the still solid crust ensues, according to Prof. Joly, under the action of tidal forces, with the result that the superheated basalt beneath the continents comes to underlie the cooler oceanic regions. The ocean floor is rapidly stoped away to within a few kilometres of the surface, but not sufficiently to threaten its stability, and the loss of heat by conduction finally leads to the reconsolidation of the basaltic layer.

Thus Prof. Joly provides for a periodic accumulation and discharge of heat in the basaltic substratum. The remainder of the book exhibits the geological effects of this thermal rhythm. As the substratum melts it expands and produces tension in the solid crust, plateau basalts are extruded through the cracks, the continents sink a little in the expanded melt, and consequently the oceans spill over and cause the transgressions of geological history. On consolidation contraction effects ensue; the continents rise again with resulting oceanic regressions; and the solid ex-

panded shields underlying the oceans press irresistibly on the bordering continents, producing the fold-mountain ranges which, as Prof. Joly shows, everywhere confront wide stretches of ocean. This simple and beautiful mechanism with its rhythmic pulsations thus matches the observed periodicity of geological history. It would, however, take much too long to go into all the new points raised by Prof. Joly. Prof. Holmes has recently shown that Joly's presentation of the theory is far too simple to match the complex details of geological history, and brings in the complicating effects of a larger rhythm due to the vastly slower heating and cooling of the peridotite stratum underlying the basaltic layer. While this criticism is valid, it is nevertheless certain that Joly has laid an enduring foundation upon which others are already eagerly building.

The book is written in a simple, luminous, and graphic style, which will enable its ideas to be understood by any educated reader. A valuable summary of the theory has been provided by reprinting as an appendix a lecture delivered to the Geological Society of London. The Oxford University Press are to be congratulated on producing a beautifully printed and well got-up book at such a reasonable price.

G. W. T.

**Economic Geology.** By H. RIES. Fifth edition, revised. [Pp. viii + 843, 291 figures, 75 plates.] (New York: J. Wiley & Sons, Inc.; London: Chapman & Hall, 1925. Price 25s. net.)

THE fourth edition of this standard work was reviewed in *SCIENCE PROGRESS* of July 1917. The aim of the author in preparing the present edition has been to make such changes in subject matter and statistical figures as would bring the book up-to-date without increasing its size. Actually a reduction of 13 pages has been achieved. Some new references have been added, and a number of the old ones have been replaced by new. The geographical reference of the work is still mainly to the United States, although some important Canadian mineral occurrences are described, as also a few from extra-American localities. Mistakes such as "P. Brun" for "A. Brun" (p. 441), and misspellings of "propylitisation" (p. 486), and "porphyry" (p. 518, fig. 162) remain uncorrected from the last edition, although they were pointed out in the earlier review.

While the book is essentially a compilation rather than a philosophical treatise on economic geology (as one might expect from its title), it maintains its great value as a reference work for student and specialist alike. Its usefulness is increased by a series of excellent and informative illustrations, and by copious up-to-date references to authorities at the end of each chapter.

G. W. T.

**The Vredefort Mountain Land in the Southern Transvaal and the Northern Orange Free State.** By A. L. HALL, M.A., Sc.D., and G. A. F. MOLENGRAAFF. [Pp. 183, Royal 8vo. 39 plates, and geological map.] (Shaler Memorial Series: *Verh. d. Kon. Akad. v. Wetensch. t. Amsterdam*, Tweede Sectie., Deel xxiv, No. 3, Amsterdam, 1925.)

THIS work is one of the most finished pieces of geological research we have seen for a long time; and both the authors and the American foundation which has financed the field work are to be congratulated on these brilliant results. The special problems the authors set out to solve, and their solutions, are briefly sketched in *SCIENCE PROGRESS*, Oct. 1926, p. 221. The main problem was: how could the intense contact-metamorphism of the sedimentary rocks surrounding the nearly circular outcrop of the Vredefort granite have arisen when the granite is almost certainly older than the

sediments? The solution favoured by the authors is that an immense concealed intrusion, the only surface manifestations of which are a few small bosses and dykes of alkali-granite and nepheline-syenite, was injected beneath the Vredefort granite, updoming it, and causing great internal stresses which have produced flinty-crush phenomena in enormous profusion, and the uptilting and overturning of not less than six miles' thickness of the adjacent sediments. The clue to this solution was obtained in the excentric distribution of the contact-metamorphism with respect to the outcrop of the Vredefort granite. The weak point in this explanation (in the reviewer's opinion) is the origin of the "centripetal pressure," which is supposed to have co-operated with the younger magma in the updoming of the central granite.

The memoir contains some striking petrography; the Vredefort granite itself, the peripheral basic intrusions, the small alkalic masses, the extreme types of metamorphic rocks resulting from a unique combination of load and thermal metamorphism. Most striking of all, however, is the discussion of the innumerable pseudo-tachylite or flinty-crush-rock veins, due to the partial or complete frictional fusion of the Vredefort granite and its peripheral rocks along surfaces of intense internal movement. This is by far the most complete account of the phenomenon which has appeared in the literature. It is further noteworthy for the suggestion that the great ring-dyke of enstatite-granophyre which intersects the granite is an extreme product of this mode of fusion.

The memoir is equally important because of its contribution to the newly developed field of *magma-tectonics*, and to the study of metamorphism. It should be read and re-read by every petrologist, especially by those who are taking part in the development of the newer views relating the tectonic and petrologic aspects of the geological cycle.

G. W. T.

**Text-book of Petrology, Vol. I. The Petrology of the Igneous Rocks.** By F. H. HATCH, O.B.E., Ph.D. Eighth edition. Revised with the assistance of A. K. WELLS, D.Sc. [Pp. 566, with 144 figures.] (London: G. Allen & Unwin, 1926. Price 15s. net.)

THE eighth edition of the first volume of Dr. Hatch's well-known text-book, dealing with the igneous rocks, appears after a lapse of twelve years from the date of the seventh edition. While the plan of the work differs but little from that of its predecessors, it has been largely rewritten with the aid of Dr. A. K. Wells. New features are chapters on the consolidation of igneous magmas, and on cycles of igneous activity in the British Isles. The work of the Geophysical Laboratory at Washington has been incorporated in the petrogenetic discussions.

It may be said at once that the work has been well done, although the new wine of modern facts and views (presumably poured in by the junior author) has in some parts seriously strained the old vessel. Nowhere is this more evident than in the chapter on classification, wherein an attempt has been made to express modern views on classification and nomenclature within a system which has for its limiting framework a tripartite division based on *silica percentage*. The grouping of igneous rocks into Acid, Intermediate, and Basic divisions ( $\text{SiO}_2$  percentage respectively greater than 66; between 66 and 52; and less than 52) does not express anything fundamental or genetic, as is indeed recognised by the authors (p. 196); and it is difficult for the student to apply in practice. He has always to translate "silica percentage" into terms of actual minerals present. It is consequently better to base classification on the relative proportions of minerals or groups of minerals, the actual units with which the student has to deal,

than on the elusive and immediately indeterminable silica percentage. The silica percentage classification is really a relic of an old-time and now obsolete conception of the constitution of rock magmas. One of its difficulties is illustrated by the proposal (p. 210) to make the percentage of silica the means of distinguishing the granodiorites from the diorites, the dividing line being 66 per cent. of silica. But the silica percentage of the *average* granodiorite given in the table on p. 199 is only 65.1. It is better to make the amount of quartz the criterion for the distinction of granodiorites and tonalites from quartz-diorite and diorite.

In the first chapter there is a brief mention of Goldschmidt's view of the composition of the earth's zones, but none of the more probable view of recent American workers. In Chapter II lopoliths are treated under the heading of laccoliths, which may tend to obscure the fact that the two forms are genetically quite different. There is a brief but good discussion of the consolidation of magmas, and the rock textures resulting therefrom, but there is no specific treatment of the problems of the origin and differentiation of igneous rocks. The latter topic is incidentally dealt with under the heading of petrographic provinces.

A somewhat delusive over-simplification of nomenclature is advocated, but is, fortunately, not fully carried out in practice. We are glad to see the vague term "porphyrite" being quietly dropped in favour of diorite-porphry, etc.; but the use of the prefix "crypto-" for felsitic rocks, in such terms as crypto-granite, crypto-syenite, etc., strikes us as neither necessary nor desirable when the term felsite is so well known.

The section on the igneous history of the British Isles is, in our opinion, the best feature of the book. It provides us with the most comprehensive and modern survey of the petrographical provinces and periods of this country.

There are a few minor mistakes and misprints. The term "Renfrewshire" on the map p. 480 covers far too great an area; the term *synneusis* (p. 174) was introduced by Vogt, not Sederholm; and the alkalis have got interchanged somehow in the table of analyses on p. 335.

Notwithstanding its minor defects, and the archaic basis of its system of classification, the book forms a very notable contribution to British petrographical literature, and provides the best available account of our igneous rocks. It is written in an easy style, and the illustrations are informative and well reproduced.

G. W. T.

## BOTANY

**Root Development of Field Crops.** By Prof. JOHN E. WEAVER. McGraw-Hill Publications in the Agricultural and Botanical Sciences. [Pp. xii + 291, with 113 figures.] (New York and London: McGraw-Hill Book Co., 1926. Price 15s. net.)

THE aerial growth of crop plants has always attracted a great deal of attention, and the literature dealing therewith is extensive. The subterranean systems, on the other hand, present more difficulty in observation and have consequently been much neglected till very recent times. Howard in India and Weaver and other workers in America are endeavouring to remedy this and to provide some accurate information as to the relations between root and shoot growth. The mechanical difficulties of technique render it necessary to confine the work to areas where the soil and subsoil are of such a nature as to permit satisfactory excavation of the roots without undue difficulty.

The volume under review sums up the results of a large volume of American work in which the author has taken a leading part. The roots are examined by means of careful excavating with a series of trenches, a method

which has proved remarkably successful on the soils investigated. As a preliminary, soil characteristics as correlated with plant growth are outlined, special notice being given to root habits in relation to crop production. Emphasis is laid on the point that a very finely branched root has a much greater absorbing surface, and is therefore more active in functioning than a thick, heavy root with few branches. Weaver contends that under suitable soil conditions crop plants are deep-rooted and are not chiefly confined to the top 6 or 8 inches of soil, and that consequently the nature of the subsoil plays an important part in plant nutrition and absorption of water. Root development is influenced by the supply of available plant food, and in agricultural practice this has a direct bearing upon such matters as depths of ploughing and manuring and the kinds of fertilisers used. Root habit may not be directly influenced by tillage methods, but indirectly in response to differences brought about in the physical and chemical constitution of the soil and subsoil. In areas where the water content is the chief limiting factor to growth the root habits of native plants afford a useful warning of soil conditions that are unfavourable to growth. Individual crops vary greatly in their growth, and this is also affected by different methods of cultivation, such as spring and autumn sowing in wheat.

Special notice may be drawn to the excellent and accurate illustrations, which are really maps of the roots as they appeared *in situ*, either in a vertical or horizontal plane, photographs and ordinary sketches having proved unsatisfactory for the purpose required.

W. E. BRENCHEY.

**Kalkfrage Bodenreaktion und Pflanzenwachstum.** VON O. ARRHENIUS. (Pp. vii + 148, with one Plate and 40 text figs.) (Leipzig: Akademische Verlagsgesellschaft, 1926. Price 8 marks.)

In recent years the important rôle of calcium salts in natural and artificial plant communities owing both to their chemical and physical effects has become more and more recognised. The total absence of calcium salts from soils will of course produce calcium starvation, but the much more common condition is that in which the amount is so low that although the minimal requirements of the plant with respect to this element are met, an acid condition results. This soil acidity is now known to be a real acidity due to the presence of hydrogen ions in excess of hydroxyl ions and the discovery of indicators each giving marked colour changes over a narrow range of reaction has placed the determination of hydrogen-ion concentrations within the capacity of any careful investigator. As a consequence of the realisation of the importance of soil reaction and these increased facilities for its determination a copious literature bearing on the subject has grown up, as will be realised from the useful bibliography appended to the present work, which, though containing over four hundred references, is by no means exhaustive, and indeed there are some surprising omissions. The citations in the text are of a very cursory character and constitute a somewhat misleading guide to the chief aspects with which these papers deal.

The first chapter treats of the nature and origin of acidity and alkalinity of soils, whilst the second treats of the buffer action they exhibit. The most extensive sections respectively describe the methods of investigation and the effects of reaction on plant growth. In the latter a number of curves are furnished illustrating the effect of reaction on the growth of various crop plants, and it is interesting to note that these mostly show a bimodal form similar to those to which the reviewer has called attention in the incidence of wild species. It cannot be too strongly emphasised that though cultivated crops, under normal conditions of cultivation, and feral species, under constant conditions of climate and competition, exhibit an optimum

with respect to reaction, yet the potential range of tolerance is often comparatively wide, and *in the absence of competition* may cover the whole range of reaction that the soils of a region present. Though generally in such cases the existence of an optimum is indicated by a falling off in vigour as we approach the extremes, nevertheless an increase in vigour may result from an even greater depression of some competitor or the suppression of some parasite.

The existence of such reaction optima for crop plants plays a part in determining their geographical distribution, since in general we find that the prevalent reaction of the soils of a country is less acid as we pass from the cooler to the warmer regions of the earth. Thus the prevailing reaction of Finland is pH 5.5-5.9, of Denmark 6-6.4, of Italy 7-7.4, and of Egypt 7.5-7.9.

In the last chapter an attempt is made to evaluate the practical results accruing from the study of soil reaction, a knowledge of which enables the farmer to select suitably tolerant species. In no direction, however, is the practical importance better illustrated than in the artificial increase of soil acidity by means of sulphur in order to suppress the black scab in potato culture and in the liming of acid soils to encourage nitrogen-fixing bacteria or to precipitate soluble salts of alumina or iron.

There is much useful information in this little book, which is well printed and comparatively cheap.

E. J. SALISBURY.

**Index Kewensis plantarum phanerogamarum. Supplementum sextum.**  
Ductu et consilio A. W. HILL confecerunt herbarii horti regii botanici  
Kewensis curatores. [Pp. iv + 222.] (Oxford: Clarendon Press, 1926.  
Price 10s. net.)

To all plant systematists the *Index Kewensis* is an indispensable tool which we owe to the foresight and generosity of Darwin and the industry of Dr. Daydon Jackson. In the present supplement are contained the names and synonyms of genera and species of flowering plants which have been published from the beginning of 1916 to the end of 1920.

It is difficult for anyone to appreciate the meritorious labour involved in the making of such a work or in maintaining it in an up-to-date condition, but some idea is gathered from the fact that this record of five years alone contains some 30,000 references. Unfortunately it is only too true that the number of new binomials is not an index of the increase in our taxonomic knowledge.

The extreme importance of such a work is perhaps best realised in the larger genera. Thus in the quinquennium dealt with some 170 binomials have been added to the already large number included in the genus *Hieracium*, whilst in the genus *Rhododendron*, largely owing to the studies of the late Prof. Bailey Balfour, the number of additional names in the same period is over 350, and even this number is surpassed in the genera *Rosa* and *Rubus*. With such increases in the number of species and binomials the work of the systematist would be rendered wellnigh impossible without this periodic gathering together of the newly minted currencies in the world's botanical nomenclature.

E. J. S.

## ZOOLOGY

**Animal Ecology.** By A. S. PEARSE. [Pp. ix + 417.] (London: The McGraw Publishing Co., 1926. Price 20s. net.)

"Some people," says Prof. Pearse, "affirm that there is more to an animal than matter energised by a specific and peculiar system of activities, *i.e.* that there is something necessary to complete an animal besides matter

and physical energy. Science has not yet been able to say that there is anything else." There are, he says further, two possibilities—(1) that there is nothing in an organism except matter and energy, but organisms are such complex systems that they have, so far, defied complete analysis; (2) that there is something else that has not been, and perhaps never can be, completely described and measured.

The latter view is that which most ordinary people take. Biologists and medical men generally take the former view. They investigate the organism in health and disease by physico-chemical methods, and so they must assume that the functioning animate system is a locus of physical and chemical changes. The success of physiological investigation shows that the assumption "works" and is, in a sense, "true." Still this success only proves that the *means* of organic functioning are just those physico-chemical processes that we can also observe in inorganic systems.

Ordinary non-scientific observers, such as good shepherds, fishermen, anglers, gamekeepers, sportsmen, and field naturalists, would generally tend to take the second of Prof. Pearse's standpoints. They may match their own wits against those of wild and domestic animals. They recognise, or at least believe, that animals other than man can "feel pain and pleasure, that they are capricious and may choose between alternatives." A shepherd may blame or approve the conduct of his dog, and he may reward or punish it. And so on.

Roux maintained that there were "organic causal processes" which had all the validity of chemical and physical conceptions, although they could not be reduced to chemical and physical categories. These truly organic conceptions are growth, assimilation, dissimilation, reproduction, heredity, racial and individual differentiation, and so on. The investigation of these "causal organic processes" is really the object of *biology*—what physiology does is to elucidate the ways in which growth, for instance, is effected. What are the materials utilised. Growth itself is something other than metabolising substances.

Quite indispensable to biological investigation in this sense is a complete knowledge of the acting of living organisms: Their habitats, habits; their relations to their environments, their responses and adjustments when the physical nature of the environment changes; their societies and social customs; their mode of mating; their temperaments; their life-histories in the wide sense, and so on. This is ecology, and so far it has been far better done in botanical than in zoological science. Prof. Pearse's book is therefore something rather new, and it will be most cordially received in all biological laboratories as indicating the modes of approach to the study of causal organic processes, both in the field and with the assistance of physiological laboratory methods. Much of it is, of course, a statement of what is the ground to be covered, and a formulation of the problems and the terminology of animal ecology. The arrangement is clear and concise, and the style is very agreeable. Finally there is a fine bibliography—the first thing of its kind in this department of biology. The book deserves a very hearty welcome.

J. J.

**Notes on the Game Birds of Kenya and Uganda.** By SIR FREDERICK J. JACKSON, K.C.M.G., C.B., M.B.O.U., F.Z.S. [Pp. xiv + 258, with 13 plates.] (London: Williams & Norgate, 1926. Price 25s. net.)

THERE is no one better qualified to write on the birds of Kenya and Uganda than the author of this book. Apart from being a life-long ornithologist and first-rate field naturalist, few could have the extensive knowledge of these two adjacent colonies, where he has spent over thirty years.

The reviewer will never forget conducting the author to a heronry he discovered at the north-eastern corner of Victoria Nyanza a quarter of a century ago. His enthusiasm to get at the nests and the recklessness with which he climbed the frail ambatch-trees growing out of the slimy mud kept the reviewer on tenter-hooks for longer than he trusts he will ever be kept again. It is many years since Sir Frederick Jackson first contemplated writing the present volume, and no one is more delighted to have it in his hands than the reviewer, who will even forgive him for misspelling his name—or is it merely a printer's error? All workers in the field will appreciate what the author says in his Preface about the lack of authoritative works in English on African birds. Had it not been for *Die Vögel Afrikas*, by Prof. Reichenow, we would have been wellnigh helpless. Apart from this, ornithological nomenclature during the past five-and-twenty years seems to have been the playing-field of systematists. Let us sincerely hope and trust that Mr. W. L. Sclater's *Systema Avium Ethiopicarum* will be accepted as the last word on the subject for some time to come, to say the least.

Although the author has very wisely adopted the nomenclature of Mr. Sclater's *Systema Avium Ethiopicarum*, he also gives the old familiar nomenclature of Sharpe's *Hand List*, and of Reichenow's *Die Vögel Afrikas* at the beginning of each species.

This book fills a long-felt want and is certain to find a place in the library of every East African settler and administrator, while every sportsman visiting any part of East Africa should include it as an essential part of his equipment.

Here and there, a few printer's errors will need correction in future editions.

Although most of the plates are good and all more or less familiar, the book is worthy of a completer and better series specially drawn for the work by our greatest living bird-artist, Mr. Archibald Thorburn.

R. E. DRAKE-BROCKMAN.

**Biology.** "Science for All" Series. By O. H. LATTER, M.A. [Pp. vii + 196, with 84 illustrations.] (London: John Murray. Price 3s. 6d. net.)

It is very satisfactory to note the increasing interest in biological science that is being shown in schools. We are, at present, just realising dimly what biologists have long since urged, that a thorough study of biology is the only sure way to an understanding of our sociological problems. Mr. Latter's book is well suited to provide a study of biology such as should form part of a general education. Does the argument on p. 141 give sufficient consideration to the laws of osmosis?

W. C. BROWN.

## ANTHROPOLOGY

**Race and History: An Ethnological Introduction to History.** By EUGÈNE PITTARD, Professor of Anthropology at the University of Geneva. Translated by V. C. C. COLLUM. [Pp. xxiii + 505, with 6 figures and 3 maps.] (London: Kegan Paul, Trench, Trubner & Co.; New York: Alfred A. Knopf, 1926. Price 21s. net.)

*Les Races et l'Histoire*, published in 1924, was one of the first volumes to appear of the series *L'Évolution de l'Humanité*, and the inclusion of a translation among the books selected from the French library to form part of the parallel English one, *The History of Civilisation*, was apposite, as the literature dealing with the subject in our language is particularly scanty. The sub-title is, perhaps, misleading, and in reading the book the historian

may be disappointed to find so little contact between the matter presented and his own studies. There are no theories claiming to establish a correlation between physical constitution and racial psychology, no acceptance of the hypothetical effect of environment upon either, and an almost complete rejection of the claim that historical evidence may furnish a reliable guide to racial history. The methods of the earlier writers who theorised on such questions with little or no acquaintance with physical anthropology are decried in this book. Professor Pittard's object was to present an anthropological history of the world as it is envisaged by observational science, paying little regard to the evidence of history, philology, and cultural anthropology, and, though restricted in that way, the scope of the work is immense. All regions of the world are dealt with, but the greater space is devoted to the countries of Western Europe and the Mediterranean area. Separate chapters give sketches of the racial history of the more important ones from the earliest known prehistoric times to the present day. The data considered relate to the skeletal remains of extinct populations and anthropometric measurements of existing ones, and, in general, reference is only made to the cephalic index, stature, and integumentary colours. The treatment is not unlike that used by Ripley in his well-known work on the peoples of Europe, and by Deniker in his survey of the peoples of the world. To make such an account intelligible and connected it was, perhaps, necessary to make numerous statements as of fact, for which no adequate evidence can be given in the present very imperfect state of our knowledge, but they might, at least, have been stated in a less dogmatic way. The translation of over-beliefs into definite assertions is most flagrant in the descriptions of the races of prehistoric times, and the writer's assurance becomes greater according as the material from which deductions can be drawn is scarcer. To the historian, the perusal of this book will perhaps suggest that Herodotus and Ptolemy are at last become old-fashioned anthropologists, that statements such as "The Anglo-Saxons came and exterminated the British" were better unsaid, and that it would be well to renounce all claim to be a decipherer of racial history, although that was his undisputed right not long since. He will think that brachycephalic and dolicocephalic races (which may be either tall or short) succeeded one another in Europe in a most bewildering way, and may hope that the physical anthropologist will find simpler tales to tell than one such as that recorded for the country the anthropological history of which is said to be better known than that of any other country. "Thus the French population presents a heterogeneous ethnic aspect when History is about to write its annals. It is composed in the first place of the descendants of palæolithic and neolithic Dolicocephals (genealogy: Chellean—Cro-Magnon—Langerie-Baumes-Chandes); of descendants of the tall northern Dolicocephals; and finally of descendants of the Brachycephals (Proto- and Neo-Brachycephals?) of the polished stone and bronze ages" (p. 126). While the science of physical anthropology is yet in its early infancy, the execution of a work of this kind was a bold essay. That the greater part of it will have to be re-written when the evidence is adequate enough to prove where now we can only guess darkly there can be little doubt, but there is here a clear recognition of the possibilities of the application of scientific methods to these subjects, and the record of a great deal that has already been achieved. It is to be regretted that in a book of this kind the author—a Swiss professor—should have found so many occasions on which to display the colour of his political bias. There is much tilting against Gobineau—a writer little known in this country—whose heinous crime it was to uphold the thesis that the tall dolicocephalic races of Europe have been in actuality and by natural right the ruling peoples of the continent.

**The Relation of Nature to Man in Aboriginal America.** By CLARK WISSLER.  
[Pp. xx + 248.] (New York: Oxford University Press, 1926. Price 16s. net.)

THE subject matter with which the science of anthropology is concerned may be classified under the three rather ill-defined heads of things made by man—his customs and social traits and somatic characters. Each is capable of being studied from the point of view of geographical distribution. This book deals with the application of that modern method to a most varied assortment of anthropological data, which have been culled from the specialised memoirs of a large number of writers. It presents first the distribution of several so-called "traits of material culture," such as mounds and earth-works, moccasins, and methods of arrow release from which it is concluded that all behave in the same way, in that the various forms of each are continuous and overlapping in a definite concentric way, the most primitive form having the widest distribution, and the most highly specialised the most restricted and central position. That is followed by an account of other material, such as monolithic axes and mutilations of the lip and nose by piercing them with sticks, showing similar distributions (whether in the world as a whole or only in America) in areas which are widely separated from one another. In the same way examples are given of a number of social traits, including "mother-in-law avoidance" and blood sacrifice, which have either unique distributions or, as Professor Wissler supposes, independent centres of distribution in different areas, and he concludes that all true culture complexes are distributed in one and the same way. The second half of the book is devoted to an application of these methods to the somatic characters of races of mankind. A difficulty arises here which is not faced or even commented on. All the examples previously considered were of single artifacts or well-defined customs which show various forms from a simple or basic to a complex one, and the conclusions arrived at concern the traits themselves and nothing more: it is not suggested that the study of the distribution of a single trait can lead to any conclusions relating to society as a whole or to anything other than the trait itself. But the quantitative groupings of somatic characters do not correspond at all to different steps in an evolutionary sequence, as far as we know, and they most certainly cannot do so for all characters, and in the second place the conclusions we want to arrive at do not concern the characters themselves so much as the racial entities which they represent. Professor Wissler assumes without hesitation that maps showing continuous distributions of the concentric type for cephalic indices are *by themselves* going to teach us a great deal about the relations and descent of racial types. But the inter-racial correlations of physical characters are known to be small, and it is evident that if any other trait, such as stature, is plotted, the distributions will be entirely different in shape and extent though, perhaps, still predominantly of the continuous and concentric form. The conclusions, then, will surely be different, and why should one character be chosen rather than another? The ones actually taken (being stature, chest circumference, cephalic index, and eye colour) were evidently selected because suitable data happened to be available. Does not the history of physical anthropology show clearly enough the futility of basing any theories of relation and descent on the study of single somatic characters? By using other methods the writer arrives at several far-reaching conclusions from data which seems to be quite inadequate for the purpose. All statistical constants are quoted without probable errors, and most without any indication of the size of the population from which they were deduced. The numerous maps are of great interest, but we fail to see that the ones giving the distributions of somatic characters have been shown to prove anything beyond the fact that such distributions are normally continuous and of a zoned type. B.

**The Southern New Hebrides. An Ethnological Record.** By C. B. HUMPHREYS. [Pp. xvi+214.] (Cambridge University Press, 1926. Price 12s. 6d. net.)

WITH the heritage of Hakluyt, Purchas, Anson, and Cook, among other early records of travel, the English-speaking peoples may claim to have provided many of the earliest surveys of barbarous parts of the world at times when the known facts concerning them were usually confused with the wildest fiction. The books of those pioneers are of great value to the modern science of cultural anthropology, as they provide the only source of evidence relating to many primitive civilisations before they had been modified by contact with European overlordship. The writer of this modern record attests to the general accuracy of the observations made by Captain Cook. Several accounts of the New Hebrides have been written since his day—either mere travellers' notes or quasi-scientific descriptions—but this claims to be the first to set down in English the cultural conditions of the southern islands. As its title implies, the object of the book is to furnish a scientific record, and no concessions are made to make it more attractive to the general reader, although he may find much matter of interest in its pages. The islands are dealt with in turn, and for the two larger ones there are anthropometric data, sociological and economic surveys, accounts of the things made and body adornments and mutilations, pedigrees and notes on the languages. The detailed information relating to Tanna and Eromanga was collected by the writer during visits to them, but the scanty material for the smaller islands was gathered from records of earlier observers. In reading through this valuable mass of evidence, one gathers the impression that Mr. Humphreys was chiefly interested in the customs and social traits, and that the carefully prepared descriptions he has given of them will provide the most enduring part of the book. The anthropometric data (comprising 10 measurements of 247 individuals) are not ample enough to solve any of the problems of such a racially heterogeneous area and the descriptions of artifacts and body decorations are aided by no photographs and by only a few drawings. This treatise is essentially an observational record, and only passing comparisons are made between the cultures of the area studied and those of neighbouring districts. It is to be regretted, perhaps, that the author did not provide an additional comparative study of the islands of Tanna and Eromanga, for it is by no means easy to construct composite pictures of those cultural units from the detailed information given relating to so many traits which are apparently independent of one another. In a short chapter of conclusions at the end of the book, the writer does broach the wider questions of racial and cultural origins and distributions, but the treatment is too hasty to carry convictions, as the problems are admittedly complex ones.

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**The Upper Palaeolithic Age in Britain.** By D. A. E. GARROD, B.Sc. Oxon. [Pp. xxi + 190, with 3 plates and 49 figures.] (Oxford: The Clarendon Press, 1926. Price 10s. 6d. net.)

THERE is little doubt that the subject dealt with by Miss Garrod, with success, in this book, is of considerable importance to all those who make a study of the remains of man of Upper Palaeolithic times. Until the appearance of this volume our knowledge of the period under discussion, so far as it related to Britain, was, without question, very scanty. Numerous investigations had, indeed, been made by various archaeologists, in the British caves; but the literature dealing with the relics discovered was somewhat scattered, and often contained in the publications of learned, but obscure societies, not easily accessible to the student. Miss Garrod has now—after, no doubt,

considerable trouble—brought together all these accounts of cavern researches into one volume, and further, by means of her extensive knowledge of the cultures of Upper Palæolithic times, has placed a new and an enlightening interpretation upon many of the specimens found in these researches. The book is divided into three parts, viz. (1) The Upper Palæolithic Cave Sites of Britain, (2) Open-air Finds Referable to the Upper Palæolithic, and (3) Epipalæolithic Cultures. Each of these three divisions is dealt with in much detail, and Miss Garrod concludes, after a survey of the whole evidence, that while Britain in Upper Palæolithic times was “a north-west cape, remote and inhospitable,” and not exactly a favourite place of residence, yet the people of this period did, in some cases, take their courage in both hands, and decide to reside here, both in caves and at certain “stations” in the open country. The author believes that the most abundant remains of Upper Palæolithic man in Britain are referable to the Aurignacian culture—the succeeding Solutrean phase is represented, but not abundantly—while there is reason to believe that the still later Magdalenian civilisation flourished to a small extent—or, in any case, “influenced” by some unspecified means—the development of the prehistoric industries being carried on in Britain at that particular time. Lastly, a number of flint implements, and other relics, linking up the latest Palæolithic Cultures with those of the earliest Neolithic, have been found in some northern caves, and at open-air settlements in different parts of the country. The book is well and lucidly written, and contains few, if any, misprints, thus distinguishing it from some other treatises on prehistoric archæology that it is necessary, at times, to read. The volume, which is well illustrated, has a foreword by Prof. Henri Breuil, and should find a place on the library shelves of all those interested in man’s past.

J. REID MOIR.

## MEDICINE

**The Secretion of the Urine.** By ARTHUR R. CUSHNY, M.A., M.D., LL.D., F.R.S. Second Edition. (Monographs on Physiology. [Pp. xi + 283.] (London: Longmans, Green & Co., Ltd., 1926. Price 16s. net.)

THE recent death of Professor Cushny has robbed physiological science of a great investigator and an inspiring teacher. It is fortunate indeed that he was able before he died to leave us in this book his considered views on a subject of which he was perhaps the greatest living exponent. This second edition of his monograph bears on every page evidence of careful revision. The nine years which have elapsed since the publication of the first edition have seen no one outstanding advance in this subject, but new and improved techniques have rendered possible a more accurate expression of the facts, and a less hesitant expression of the theory, to be found in the original work.

In Chapters V and VI, dealing with direct evidence on the functions of the tubules and glomeruli, considerable weight is given to the recent work of Richards and Wearn whose technique for examining glomerular filtrates has yielded confirmation tantamount to proof of the modern theory respecting the activity of the glomeruli, and to the studies of Starling and Verney on the heart-lung-kidney preparation as influenced by cyanide. The author criticises the interpretation placed by Starling and Verney upon their results, and endeavours to explain these results along the lines of his “modern” theory.

With characteristic reticence Professor Cushny has made but little direct allusion to his own work in this field, but a glance at the bibliography (greatly enlarged in this edition) reveals the fact that he was by no means inactive in this last decade. Chapter XV, on renal disorders, has been notably changed. The work of Dr. Mayrs and of Professor Dunn on the pathology of nephritis appears to give important confirmation to the “modern” theory, which had hitherto been based purely on physiological studies. Diabetes

insipidus, no longer considered a disease of the kidney, has been removed from the chapter on renal pathology. The author inclines to the view that the disease is associated with a lesion in the hypothalamus near to the pituitary rather than in that body itself. Be that as it may, the water diuresis characteristic of this condition has been shown to be the result, not the cause, of the excessive intake of water: the kidneys are normal.

The pituitary body continues to be the focus of much attention in renal studies, to judge by the enlargement of Chapter XI, but it is clear that no satisfactory explanation is yet forthcoming of its erratic influence on the kidney.

The publishers are to be complimented on the clearness of the type and the accuracy of the setting.

P. E.

**Muscular Activity.** The Johns Hopkins University School of Medicine, Lectures on the Herter Foundation, Sixteenth Course, 1924. By A. V. HILL, M.A., Sc.D., F.R.S. [Pp. viii + 115, with 11 figures in text.] (Baltimore: The Williams and Wilkins Co.; London: Baillière, Tindal & Cox. Price 12s. 6d. net.)

In these lectures Prof. A. V. Hill summarises the current conceptions of muscular function, to the formation of which he and his fellow workers have so materially contributed. He does not deal with the whole province of muscular activity, but with certain newer findings in the mechanical, thermal and chemical aspects of the subject, towards the elucidation of which his own work has been chiefly directed. The lectures provide a very welcome, clear, and concise statement of these newer conceptions. They will be of interest not only to the physiologist and the medical man, but also to that immensely large public interested in the physical performances of the body, whether in sport or work. These latter readers will no doubt enjoy most the third and fourth lectures, and particularly the fourth, in which the results of the scientific and laboratory work are applied to man. Prof. Hill, himself an international runner, makes no secret of his interest in athletics, and defends his choice of the athlete as his subject of experimentation on the very just ground that normally he must be a very healthy person and therefore one likely to give a clean experiment.

As a result of observations by Hill and other workers in the same field, we have been provided with some very startling figures showing the wonderful capacity of the heart to adapt itself to the needs of the body. To supply 59 litres of oxygen a minute (a consumption actually achieved by one of Hill's subjects doing "standing running") it is necessary for the heart to transmit 37.5 litres of blood a minute from the right side of the heart to the left and out into the tissues. This means therefore that in such a case the heart would pump out every minute 75 litres of blood (about 17 gallons), or some 120 times its own volume. Such figures make it very clear that the heart's efficiency is a factor of the highest importance in determining the limit of an individual's physical performance. Having read the last two lectures, the general reader will feel more disposed to grapple with the two first, which, though giving some of the essential facts on which the final conclusions are based, are not in their condensed form so easily followed by those not familiar with the subject.

In deference to a wish expressed by Prof. Hill at the end of his lectures that such tribute should be paid, his statement is repeated that a great part of the work described in these lectures was made possible by the devotion and enterprise of his colleague, Hartley Lupton. Lupton's early death at the age of 32 not only robbed Prof. Hill of a devoted fellow-worker, but also deprived physiological science of an enthusiastic and successful researcher.

W. C. CULLIS.

## BOOKS RECEIVED

*(Publishers are requested to notify prices)*

- Modern Algebraic Theories. By Leonard E. Dickson, Ph.D., Professor of Mathematics in the University of Chicago. Chicago: Beng. H. Samborn & Co. (Pp. ix + 276.)
- The Thirteen Books of Euclid's Elements. Translated from the Text of Heiberg, with Introduction and Commentary. By Sir Thomas L. Heath, K.C.B., K.C.V.O., F.R.S. Second Edition. Revised with Additions. Cambridge: at the University Press, 1926. (Pp. Vol. I, xii + 432; Vol. II, 436; Vol. III, 546.) Price 70s. net.
- Mathematics for Engineers. By Raymond W. Dull. London: McGraw-Hill Publishing Co., 6 Bouverie St., E.C.4. (Pp. xvii + 780.) Price 25s. net.
- Traité du Calcul des Probabilités et de ses Applications. Tome II. Les Applications de la Théorie des Probabilités aux Sciences Mathématiques et aux Sciences Physiques. Fascicule I. Applications à l'Arithmétique et à la Théorie des Fonctions. Leçons professées à la Faculté des Sciences de Paris. Par Émile Borel, rédigées par Paul Dubreil. (Pp. 100.) Price 22 frs. Tome IV. Applications Diverses et Conclusion. Fascicule I. Applications au Tir. Par J. Haag. (Pp. 184.) Price 25 frs. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926.
- Elements of Mathematics. For Students of Economics and Statistics. By D. Caradog Jones, M.A., Senior Lecturer in Social Statistics, University of Liverpool, and G. W. Daniels, M.A., M.Com. Professor of Commerce and Administration, University of Manchester. Liverpool: at the University Press; London: Hodder & Stoughton, 1926. (Pp. viii + 240.) Price 8s. 6d. net.
- Mathematical and Physical Papers, 1903-1913. By Benjamin Osgood Peirce. Late Hollis Professor of Mathematics and Natural Philosophy in Harvard University. Cambridge, U.S.A.: Harvard University Press, 1926. (Pp. viii + 444.) Price 21s. net.
- Compound Interest and Annuity Tables. Values of all Functions to Ten Decimal Places, for 1-100, 1-200, 1-300 years. Rates of Interest  $\frac{1}{2}$  of 1 Per Cent. to  $10\frac{1}{2}$  Per Cent. Conversion Factors and Logarithms. By Frederick C. Kent, Associate Professor of Mathematics, Oregon State College, and Maude E. Kent. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. viii + 214.) Price 20s. net.
- Popular Experiments in Dynamics. By George C. Sherrin. London: George Philip & Son, 32 Fleet St.; Liverpool: Philip, Son & Nephew, 20 Church Street, 1926. (Pp. viii + 64, with 29 plates.) Price 2s. net.
- Matter and Gravity in Newton's Physical Philosophy. A Study in the Natural Philosophy of Newton's Time. By A. J. Snow. Lecturer in Psychology, Northwestern University. London: Oxford University Press, 1926. (Pp. 256.) Price 7s. 6d. net.

- Physics in Industry.** Lectures delivered before the Institute of Physics. By Walter MaKower, M.A., D.Sc., F.Inst.P., and Bernard A. Keen, D.Sc., F.Inst.P. With Introductions by the Hon. Sir Charles Parsons, K.C.B., LL.D., D.Sc., F.R.S., and Sir Daniel Hall, K.C.B., LL.D., F.R.S. Vol. IV. London: Oxford University Press, 1926. (Pp. 63.) Price 3s. net.
- Three Lectures on Atomic Physics.** By Arnold Sommerfeld, F.R.S., Professor of Theoretical Physics at the University of Munich. Translated by Dr. Henry L. Brose. London: Methuen & Co., 36 Essex Street, W.C. (Pp. 70.) Price 2s. 6d. net.
- Practical Physics.** By T. G. Bedford, M.A., F.Inst.P., Demonstrator of Experimental Physics in the Cavendish Laboratory, Cambridge. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. x + 425, with 225 figures.) Price 18s. 6d. net.
- Organic Syntheses.** An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. Editor-in-Chief, Henry Gilman. Vol. VI. New York: John Wiley & Sons; London: Chapman & Hall. (Pp. vii + 120.) Price 7s. 6d. net.
- Potentiometric Titrations.** A Theoretical and Practical Treatise. By Dr. I. M. Kolthoff, Conservator of the Pharmaceutical Laboratory of the State University of Utrecht, Holland, and N. Howell Furman, Ph.D., Assistant Professor of Chemistry, Princeton University, Princeton, N.J. New York: John Wiley & Sons; London: Chapman & Hall, 1926. (Pp. xii + 345.) Price 22s. 6d. net.
- The Atmospheric Nitrogen Industry.** With Special Consideration of the Production of Ammonia and Nitric Acid. In Two Volumes. By Dr. Bruno Waeser. Translated by Ernest Fyleman, B.Sc., Ph.D., F.I.C., Chief Chemist to Messrs. F. H. Crowley & Partners, Consulting Engineers, Westminster. With a Foreword by J. F. Crowley, D.Sc., B.A., M.I.S.E. London: J. & A. Churchill, 7 Great Marlborough Street, 1926. (Pp. xxvi + 646, with 72 illustrations.) Price £2 2s. net.
- Das Polarisationsmikroskop.** Seine Anwendung in der Kolloidforschung und in der Färberei. Von Hermann Ambromm und Albert Frey. Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1926. (Pp. x + 192, with 48 figures.) Price 12 marks, or 13.50 marks bound.
- Surface Equilibria of Biological and Organic Colloids.** By P. Lecomte du Noüy, D.Sc., Introductions by Dr. Alexis Carrel and Prof. Robert A. Millikan. American Chemical Society Monograph Series. New York: The Chemical Catalog Company, 19 East 24th Street, 1926. (Pp. 212, with 74 figures.) Price, \$4.50.
- The Aspergilli.** By Charles Thom and Margaret B. Church of the Microbiological Laboratory, Bureau of Chemistry, United States Department of Agriculture. Photomicrographs by C. L. Keenan. London: Baillière, Tindall & Cox; Baltimore: The Williams & Wilkins Company. (Pp. ix + 272, with 17 figures and 4 plates.) Price 22s. 6d. net.
- Theoretical Organic Chemistry (Part I).** By Francis Arnall, Ph.D., M.Sc., F.I.C., Lecturer in Chemistry, Chelsea Polytechnic, London, and Francis W. Hodges, M.Sc., Senior Science Master at Coopers' Company School, London. London: J. & A. Churchill, 7 Great Marlborough Street, 1926. (Pp. xi + 372, with 30 figures.) Price 10s. 6d. net.
- Practical Organic and Bio-Chemistry.** By R. H. A. Plimmer, D.Sc., Professor of Chemistry in the University of London, at St. Thomas's Hospital Medical School. New Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. x + 568.) Price 21s. net.

- Chemistry in the World's Work.** By Harrison E. Howe, Editor, Industrial and Engineering Technological Series. London: 11 Henrietta Street, Covent Garden, 1926. (Pp. vii + 244, illustrated.) Price 15s. net.
- Pyrosote: Das Kolloide Phänomen in der Glühend Flüssigen Materia und seine Erstarrungs-Zustände.** Unter Berücksichtigung des Latenten Photographischen Bildes. Von Richard Lorenz und Wilhelm Eitel. Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1926. (Pp. ix + 290, with 64 figures and 20 plates.) Price 18 marks, bound, 20 marks.
- The Chemistry of the Natural and Synthetic Resins.** By T. Hedley Barry, Alan A. Drummond, M.Sc., A.I.C., and R. S. Morrell, M.A., Ph.D., F.I.C. London: Ernest Benn, 1926. (Pp. vii + 196.) Price 21s. net.
- Practical Colloid Chemistry.** By Wolfgang Ostwald, Professor of the University of Leipzig, with the collaboration of Dr. P. Wolski and Dr. A. Kuhn. Translated by I. Newton Kugelmass, M.D., Ph.D., Sc.D., Yale University School of Medicine, and Theodore K. Cleveland, Ph.D. (London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvi + 191.) Price 7s. 6d. net.
- A Student's Manual of Organic Chemical Analysis, Qualitative and Quantitative.** By Jocelyn Field Thorpe, C.B.E., D.Sc., Ph.D., F.R.S., F.I.C., Professor of Organic Chemistry, Imperial College of Science and Technology, London, and Martha Annie Whitely, O.B.E., D.Sc., A.R.C.S., F.I.C., Assistant Professor, Organic Chemistry Department, Imperial College of Science and Technology, London. Reissue with Appendix on New Methods of Organic Analysis. By H. Ter Meulen and J. Heslinga. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. x + 250, with 22 diagrams.) Price 9s. net.
- The Problem of Physico-Chemical Periodicity.** By E. S. Hedges, M.Sc., Ph.D., Demonstrator in Chemistry in Bedford College, London, and J. E. Myers, O.B.E., D.Sc., A.I.C., Senior Lecturer in Chemistry in the Victoria University of Manchester. With a Foreword by Professor F. G. Donnan, C.B.E., M.A., Ph.D., D.Sc., F.R.S. London: Edward Arnold & Co., 1926. (Pp. 95.) Price 7s. 6d. net.
- Chemistry for Agricultural Students.** By R. H. Adie, M.A., B.Sc., Lecturer in Physics and Chemistry, School of Agriculture, Cambridge. London: University Tutorial Press, High Street, New Oxford Street, W.C., 1926. (Pp. viii + 357.) Price 5s. 6d. net.
- New Conceptions in Colloid Chemistry.** By Herbert Freundlich, Ph.D., Professor of the Kaiser Wilhelm Institute for Physical Chemistry, Berlin. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 147, with 47 diagrams and 20 tables.) Price 6s. net.
- An Introduction to Physical Chemistry.** By F. B. Finter, M.A., Assistant Science Master at Clifton College. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1926. (Pp. xv + 276.) Price 6s. net.
- The Essential Oils.** By Horace Finckmore, B.Sc., F.I.C. London: Ernest Benn, Bouverie House, Fleet Street. (Pp. xv + 880.) Price 70s. net.
- The Hydrous Oxides.** By Harry Boyer Weiser, Professor of Chemistry at the Price Institute. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. x + 452, with 31 figures.) Price 25s. net.
- The Chemistry of Cellulose and Wood.** By A. W. Schorger, Ph.D. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xiv + 596.) Price 30s. net.

**The Electron in Oxidation-Reduction.** By DeWitt T. Keach, Assistant Professor of Chemistry, Yale University. London: Ginn & Company. (Pp. v + 58.) Price 2s. 6d. net.

**General Inorganic Chemistry.** By M. Cannon Sneed, Professor of Chemistry in the School of Chemistry, University of Minnesota. London: Ginn & Company. (Pp. vi + 674, with 129 figures.) Price 12s. 6d. net.

**Treatise on Sedimentation.** Prepared under the Auspices of the Committee on Sedimentation, Division of Geology and Geography, National Research Council of the National Academy of Sciences. By William H. Twenhofel and Collaborators. The University of Wisconsin, Department of Geology and Geography, Madison, Wisconsin. London: Baillière, Tindall & Cox, 1926. (Pp. xxv + 661, with 61 figures.) Price 34s. net.

**Supplement to an Introduction to Sedimentary Petrography.** With Special Reference to Loose Detrital Deposits and their Correlation by Petrographic Methods. By Henry B. Milner, M.A., D.I.C., F.G.S., M.Inst.P.T., Lecturer in Petroleum Technology, Imperial College of Science and Technology, London. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. 156.) Price 9s. 6d. net.

**Rocks and Minerals. A Manual of the Elements of Petrology without the Use of the Microscope.** By Louis V. Pirsson, late Professor of Physical Geology in the Sheffield Scientific School of Yale University. Second Edition, Revised by Adolph Knopf, Professor of Physical Geology in Yale University. New York: John Wiley & Sons; London: Chapman & Hall, 1926. (Pp. vii + 426, with 36 plates.) Price 16s. 6d. net.

**The Principles of Petrology. An Introduction to the Science of Rocks.** By G. W. Tyrrell, A.R.C.Sc., F.G.S., F.R.S.E., Ph.D., Lecturer in Geology in the University of Glasgow. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xii + 349, with 78 diagrams.) Price 10s. net.

**Handbook of Non-Ferrous Metallurgy.** Prepared by a Staff of Specialists, Donald M. Liddell, Editor-in-Chief. In Two Volumes. London: McGraw Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. Vol. I. xi + 692; Vol. II, 693-1440.) Price £3.

**Tungsten. A Treatise on its Metallurgy, Properties, and Application.** By Colin J. Smithells, M.C., D.Sc., Member of the Research Staff of the General Electric Co. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. viii + 167, with 154 figures.) Price 21s. net.

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# SCIENCE PROGRESS

## RECENT ADVANCES IN SCIENCE

**PURE MATHEMATICS.** By F. PURYER WHITE, St. John's College, Cambridge.

*The Equation of the Ninth Degree.*—At the second International Congress of Mathematicians, held in Paris in 1900, Hilbert delivered a most interesting address on the future problems of mathematics. He now (*Math. Ann.*, **97**, 1926, 243–50) returns to some of the problems there indicated which have not been solved, remarking that a large number have been fruitfully attacked since 1900. In particular he is concerned with the 13th problem in his lecture, suggested by the science of nomography. The question is whether a function of an arbitrary number of arguments can be compounded from functions of a certain fixed number of arguments. For example, any rational function of any number of arguments can be constructed by addition, subtraction, multiplication, and division, and each of these operations represents a function of two arguments; hence a rational function of any number of arguments belongs to the class of functions of two arguments. Similarly, the roots of all equations soluble by radicals belong to the same class; we have only to add the operation of extraction of roots, which represents a function of one argument. If we notice that subtraction, multiplication, and division can be compounded of functions of *one* argument and a summation:  $u - v = u + (-v)$ ,  $u.v = \frac{1}{4} \{ (u+v)^2 - (u-v)^2 \}$ ,  $\frac{u}{v} = u.\frac{1}{v}$ , we get the result that rational functions belong to the class obtained by adjoining the sum  $u + v$  to the class of functions of one argument. The question now arises: are there besides the sum other analytic functions of essentially two arguments, *i.e.* analytic functions which cannot be expressed by functions of one argument and the sum? Ostrowski has shown that the function of two variables

$$\zeta(u, v) = \sum_{n=1, 2, \dots} \frac{u^n}{n^v}$$

is such a function as cannot be compounded of analytic functions of one argument and algebraic functions of any number of argu-

ments. But a further question is as to the existence of *algebraic* functions of this kind. Some light is thrown on this by the method of Tschirnhaus transformations. Let there be given an equation of degree  $n$ ,

$$x^n + u_1 x^{n-1} + u_2 x^{n-2} + \dots + u_n = 0.$$

To express the root  $x$  as a function of the  $u$  variables  $u_1, u_2, \dots, u_n$ , we put  $\chi = x^{n-1} + t_1 x^{n-2} + \dots + t_{n-1}$ , the  $t$ 's being undetermined. This leads to the equation

$$\chi^n + U_1 \chi^{n-1} + \dots + U_h = 0,$$

where  $U_h$  is a function of degree  $h$  in  $t_1, \dots, t_{n-1}$ . If we determine the parameters so that

$$U_1 = U_2 = U_3 = 0,$$

which can be done by rational processes and extraction of roots, we shall have reduced the equation to the form

$$\chi^n + U_4 \chi^{n-4} + \dots + U_n = 0.$$

Finally, putting  $\chi = \sqrt[n]{U_n} \gamma$ , we get an equation in  $\gamma$  of the  $n$ th degree, in which not only are the coefficients of  $\gamma^{n-1}, \gamma^{n-2}, \gamma^{n-3}$  zero, but the first and last coefficients are equal to 1.

The normal forms of the equations from the fifth to the ninth degrees are thus :

$$\begin{aligned} x^5 + ux + 1 &= 0, \\ x^6 + ux^2 + vx + 1 &= 0, \\ x^7 + ux^3 + vx^2 + wx + 1 &= 0, \\ x^8 + ux^4 + vx^3 + wx^2 + px + 1 &= 0, \\ x^9 + ux^5 + vx^4 + wx^3 + qx + 1 &= 0. \end{aligned}$$

It is clear that the reduction to these normal forms requires only functions of one argument and the sum. The equation of the fifth degree cannot therefore give an algebraic function of essentially two arguments in the sense explained ; the normal form contains a single parameter  $u$ , and hence the general equation of the fifth degree is soluble by functions of one argument with the help of the sum operation.

For the normal form of the equation of the sixth degree we have two parameters  $u, v$ , and therefore it appears plausible that the root of the equation of the sixth degree is a function of the required property. Similarly, it appears likely, as Hilbert stated in 1900, that the equation of the seventh degree is not soluble with the help of continuous functions of two arguments, and likewise that the equation of the eighth degree is, in the sense explained, essentially a function of the four arguments

$u, v, w, p$ . None of these guesses, however, have been proved. But it is remarkable, and this gives the title to the paper quoted, that the equation of the ninth degree only requires functions of four arguments in its solution, the five parameters  $u, v, w, p, q$  of its normal form being reducible to four variables. To see this, let us return to the equation for  $\chi$ :

$$\chi^9 + U_1 \chi^8 + U_2 \chi^7 + \dots + U_9 = 0,$$

where the  $U$ 's are polynomials in  $t_1, \dots, t_9$ . By means of the linear equation  $U_1 = 0$ , we express  $t_8$  by the others  $t_1, \dots, t_7$ , and thus get  $U_2^1, U_3^1, U_4^1$  as polynomials of the 2nd, 3rd, and 4th degrees in  $t_1, \dots, t_7$ . We express  $U_2^1$  as the sum of eight squares of linear functions,  $U_2^1 = L_1^2 + L_2^2 + \dots + L_8^2$ , and remark that the equation  $U_2^1 = 0$  can be satisfied by  $L_1 + iL_2 = 0$ ,  $L_3 + iL_4 = 0$ ,  $L_5 + iL_6 = 0$ ,  $L_7 + iL_8 = 0$ . These are four linear equations for the parameters  $t_1, \dots, t_7$ . Express, by means of them,  $t_4, t_5, t_6, t_7$  in terms of  $t_1, t_2, t_3$  and we get  $U_3''(t_1, t_2, t_3)$  and  $U_4''(t_1, t_2, t_3)$ . The equation  $U_3'' = 0$  represents a cubic surface, containing twenty-seven straight lines. To find one of these we have to solve an equation of degree 27, whose coefficients are rational in the coefficients of  $U_3'' = 0$ . Now it is known that  $U_3''$  can be expressed uniquely as the sum of five cubes:  $U_3'' = M_1^3 + M_2^3 + M_3^3 + M_4^3 + M_5^3$ , the  $M$ 's being linear in  $t_1, t_2, t_3$ ; the cubes are roots of an equation of degree 5 with coefficients rational in the coefficients of  $U_3''$ . Thus to express the  $M$ 's we need, besides the nine operations, only functions of one argument. Using  $m_i = M_i/M_4$  ( $i = 1, 2, 3$ ) as new variables,  $U_3'' = 0$  is expressed as

$$m_1^3 + m_2^3 + m_3^3 + 1 + (V_1 m_1 + V_2 m_2 + V_3 m_3 + V_4)^3 = 0,$$

containing only *four* parameters in its coefficients. If then  $t_1 = \rho_1 s + \sigma_1$ ,  $t_2 = \rho_2 s + \sigma_2$ ,  $t_3 = \rho_3 s + \sigma_3$ ,  $s$  being variable, represents one of the 27 straight lines of the surface, the coefficients  $\rho, \sigma$  are equally algebraic functions of these four parameters. Substituting these expressions in  $U_4'' = 0$ , we get a biquadratic equation  $U_4'''(s) = 0$ , whose solution involves only the sum operation and functions of one argument. It is thus clear that using functions of four arguments we can remove the coefficients of  $\chi^8, \chi^7, \chi^6, \chi^5$  and thus, by the usual final step  $\chi = \sqrt[9]{U_9 \cdot y}$ , we get an equation in which only four parameters enter.

*Conformal Representation.*—The principal result of Schwarz's investigations on conformal representation was the proof of Riemann's theorem that every simply-connected simple region bounded by a finite number of analytical arcs, cutting at angles different from zero, can be one-one, continuously represented (including the boundary) upon a circle, so that angles are pre-

served for inner points. To define the representation uniquely we may assign two corresponding oriented line elements of the interiors, *i.e.* two points and two directions through them. Poincaré, however, showed later that the conformal representation of two connected regions, two corresponding line-elements inside them being assigned, is determined uniquely if we make no supposition with regard to the correspondence of the boundaries or to the existence of such a correspondence. Schwarz's problem may thus be regarded as a special case of a more general problem in which the only requirement is a one-one, continuous, conformal representation for inner points; the two problems are equivalent if the first has a solution, but there may be cases in which this does not happen. This remark is responsible for the remarkable new development of the subject in the hands of Osgood, Hilbert, and Koebe, and, in particular, of C. Carathéodory (*Matt. Ann.* **72**, 1912, 107-44, **73**, 1913, 305-20, 322-70).

In the first of the papers mentioned he considers an infinite sequence of regions  $G_1, G_2, \dots$  in the  $u$ -plane, all containing the point  $u = 0$  as an inner point and all lying within the circle  $|u| = M$ . If  $f_n(z)$  is the analytic function which maps  $G_n$  on  $|z| < 1$ , so that the points  $z = 0, u = 0$  and the positive directions of the real axes correspond, he investigates necessary and sufficient conditions for the regions  $G_n$  in order that  $f_n(z)$  may tend to a limit function, and he examines the properties of this limit function. He proves that, if for any  $|z| < 1$  the limit function  $f(z)$  exists, then either  $f(z)$  is identically zero or the function  $u = f(z)$  for  $|z| < 1$  gives the conformal representation of a certain region  $\Gamma$  of the  $u$ -plane, which contains no inner winding-points and nowhere overlaps itself. The region  $\Gamma$  is the greatest region with the property that any closed region contained in it and containing the point  $u = 0$  lies wholly inside all the regions  $G_n$  from a certain value of  $n$  onwards. Defining then, for any sequence  $G_n$  (whether  $f(z)$  exists or not), the kernel  $K$  as the greatest region with this property, or as the point  $u = 0$  if there is no circle with centre  $u = 0$  which lies in all  $G_n$  from a certain  $n$  onwards, it is proved that the necessary and sufficient condition for the existence of the limit function  $f(z)$  is that the sequence  $G_n$  should converge to its kernel, in the sense that any arbitrary partial sequence  $G_{n_1}, G_{n_2}, \dots$  has the same kernel as the original sequence. As an application, Carathéodory shows that the inside of an arbitrary Jordan curve can be conformally represented on the inside of a circle by taking the representation of polygons which approximate to the curve from outside and proceeding to the limit. He also gives a new proof of the existence of the conformal representation of a general region, not depending on the boundary problem

for the equation  $\Delta u = 0$ , and not only proving the possibility but giving a recurrent process for finding the function required.

In the second paper cited above Carathéodory examines how the boundaries of two regions are related by a conformal representation between their inner points. What are necessary and sufficient conditions for a region in order that, when it is conformally represented on unit circle, there may be a one-one, continuous correspondence between its boundary and the circumference of the circle, which links up continuously with the conformal representation of the inside? He succeeds in proving a guess of Osgood's that this occurs for a region bounded by an arbitrary Jordan curve. In other words, if it is in any way possible to represent the boundary of a simply-connected region one-one, continuously upon a closed circumference of a circle, then for every conformal representation of the inside on the inside of a circle such a representation of the boundary is realised. The proof depends upon Lebesgue integrals and, in particular, upon a theorem proved by Fatou with their help, namely, that if the single-valued analytic function  $f(z)$  is regular and bounded for  $|z| < 1$  then there are points on the unit circle  $|z| = 1$ , everywhere dense, approaching which along a radius the function  $f(z)$  converges to a definite value. Moreover, if  $f(z)$  is not constant, then on any arc the aggregate of limiting values so obtained must contain at least three different values.

If the region is not bounded by a Jordan curve, then it may happen that to a point of the circumference of the circle there corresponds a perfect, connected aggregate of infinitely many points of the boundary of the region. Carathéodory was thus led, in the third paper under discussion, to consider the boundary of a region as made up of more general constructs than points, constructs which are definable in terms of the theory of aggregates. They are called "Primende," and their study forms an important chapter in analysis situs. See, for instance, Kerékjártó: *Topologie*, Berlin, 1923, pp. 108-120.

In the first paper above mentioned an important part is played by what is known as Schwarz's lemma, which runs as follows: Suppose that  $f(z)$  is an analytic function of the complex variable  $z$  which is regular for  $|z| < 1$  and suppose that  $f(0) = 0$  and that  $|f(z)| \leq 1$  for all points of this region. Then for any  $z$  satisfying the conditions  $0 < |z| < 1$ , we must have  $|f(z)| < |z|$  unless  $f(z)$  is a linear function  $e^{i\theta}z$ , in which case  $|f(z)| = |z|$  everywhere. A simple proof is given by Carathéodory (*l.c.* 72 (1912) 110). G. Pick (*Math. Ann.*, 77, 1916, 1-6) gave this theorem an invariantive form by laying down a metric on the unit circle, in which its orthogonal circles are the shortest lines and the non-Euclidean "distance" is defined as the logarithm of a cross-ratio. In this case the "distance" of a

point of radius vector  $\rho$  from the origin is  $\log \{(1 + \rho)/(1 - \rho)\}$  and it is clear that Schwarz's lemma is equivalent to the statement that by the conformal representation effected by the function  $w = f(z)$  "distances" from the origin are shortened, except in the linear case. If now the  $z$ -plane and the  $w$ -plane are subjected to independent linear transformations, unit circles become circles and "distances," referred of course to boundaries of the new circles, are unchanged. We can make arbitrary points in the new circles correspond to the origins and hence we have the theorem: If the function  $w$  of  $z$  is free from essential singularities inside a circle  $K_z$  and only takes values which lie inside a circle  $K_w$ , then all "distances" are shortened in the conformal transformation, unless  $w$  is linear in  $z$ , when "distances" are unchanged. The advantage of this formulation is that it lends itself to extension to the case of two or more complex variables. This extension is the subject of a recent paper by C. Carathéodory (*Math. Ann.* **97**, 1926, 76-98), which has appeared in the special volume published as a Riemann memorial; he was born on September 17, 1826. Carathéodory, confining himself to the case of two complex variables, considers functions which are regular in a general four-dimensional region  $G$  and whose moduli are everywhere less than one. If  $z_A$  and  $z_B$  are the values which a function of this family takes in two points  $A, B$ , then the two complex numbers  $z_A, z_B$  are represented by two points in the  $z$ -plane which lie inside the unit-circle. Conformal transformations of the unit circle into itself can be regarded as movements in a non-Euclidean plane in which the positive function

$$E(z_A, z_B) = \log \frac{|z_A - z_B| + |1 - \bar{z}_A z_B|}{\sqrt{(1 - \bar{z}_A z_A)(1 - \bar{z}_B z_B)}}$$

plays the part of the "distance" between  $z_A$  and  $z_B$ . The upper limit of these distances  $E(z_A, z_B)$  for all functions of the family is denoted by  $D_G(A, B)$  and is called the "distance" of the points  $A$  and  $B$  in the region  $G$ . This formulation is independent of the number of variables and it is at once seen that for functions of one variable Schwarz's lemma gives

$$D_G(A, B) = E(A, B),$$

where  $G$  is taken to be the unit circle. The properties of this distance function, which is invariant for analytic transformations, are then investigated; it is then calculated for the "dicylinder," given by  $|x| < 1, |y| < 1$ , and for the hypersphere  $x\bar{x} + y\bar{y} < 1$ , and a result proved by K. Reinhardt (*Math. Ann.* **83**, 1921, 211-55) becomes almost obvious, namely, that it is impossible to obtain an analytic transformation of the interior of a dicylinder into the interior of a hypersphere. Carathéodory further

considers "metrical" transformations, *i.e.* one-one, continuous transformations which leave the distance function unaltered; their group includes that of analytic transformations as a subgroup. He extends Reinhardt's result by showing that a corresponding metric transformation is impossible, and he finally obtains all metric transformations of a hypersphere into itself and of a dicylinder into itself.

*Fourier Constants.*—In the Riemann memorial volume above referred to there is one paper in English; it is by Hardy and Littlewood, and is entitled "Some New Properties of Fourier Constants" (*Math. Ann.* 97, 1926, 159–209). The authors remark that the theory of Fourier constants, as distinct from the theory of the convergence of Fourier series, may be said to date from Riemann, the theorem that the Fourier constants of any integrable function tend to zero having been proved by Riemann for bounded functions integrable in accordance with his definition; it was proved in full generality by Lebesgue. Supposing for simplicity of statement that  $f(\theta)$  is a real, even function, that the fundamental interval is  $(-\pi, \pi)$  and that the mean value of  $f(\theta)$  over this interval is zero, so that the Fourier series of  $f(\theta)$  is a pure cosine series  $\sum a_n \cos n\theta$  without constant term, then what are known as Parseval's Theorem and the Riesz-Fischer Theorem together assert that  $\sum a_n^2$  is convergent if, and only if, the square of  $f(\theta)$  is integrable. Generalisations of these theorems have been given by W. H. Young and Hausdorff; if  $1 < p \leq 2$  and  $p' = p/(p-1)$ , then Hausdorff's final result is that  $\sum |a_n|^{p'}$  is convergent whenever  $|f|^p$  is integrable and that  $|f|^{p'}$  is integrable whenever  $\sum |a_n|^p$  is convergent. Hardy and Littlewood give a new proof of this theorem, involving as little as possible of "existence-theory," and then they go on to investigations which provide a complete answer to the question: If  $r > 1$  and  $f$  and  $|f|^r$  are integrable, for what values of  $s$  and  $\kappa$  does it follow that the series  $\sum n^{-\kappa} |a_n|^s$  is convergent?

*Normal Families of Functions.*—In studying the values taken by a function  $f(z)$  in the neighbourhood of an isolated essential singularity, which we may suppose to be at the origin  $O$ , we may take a number  $\sigma$  of modulus  $\rho$  less than 1 and consider the behaviour of  $f(z)$  in a series of annuli bounded by circles of centre  $O$  and of radii  $R, R\rho, R\rho^2, \dots, R\rho^n, \dots$ . If we now put  $f_n(z) = f(\sigma^n z)$ , then since when  $\sigma^n z$  is in the annulus between the circle of radii  $R\rho^n$  and  $R\rho^{n+1}$ , say  $\Gamma_n$ ,  $z$  is in the annulus  $\Gamma_0$ , we may replace the study of  $f(z)$  in the neighbourhood of  $O$  by that of the family of functions  $f(z), f_1(z), f_2(z), \dots, f_n(z), \dots$  in the annulus  $\Gamma_0$ . Historically the study of families of functions preceded the special applications here indicated. Suppose then we have a sequence of functions

$f_1(z), f_2(z), \dots, f_n(z), \dots$ , holomorphic in a connected domain  $\Delta$ , limited by one or more contours  $C$ . Then it is known that if the sequence converges uniformly on the contour  $C$  it converges in  $\Delta$  to a holomorphic function; an immediate consequence is that if the sequence converges uniformly in any domain  $\Delta^1$  interior to  $\Delta$  the limit is a function holomorphic in  $\Delta$ , but not necessarily on  $C$ . Further results are that if the moduli of the functions  $f_n(z)$  are bounded in their aggregate, the convergence of the sequence in an infinity of points having at least one limit point in the interior of  $\Delta$  suffices to ensure the uniform convergence in any area  $\Delta^1$  interior to  $\Delta$ ; and finally that the condition of boundedness may be replaced by the condition that in  $\Delta$  no one of the functions  $f_n(z)$  assumes certain two values  $a$  and  $b$  (Landau and Carathéodory).

A family of functions is said to form a *normal family*, in the terminology of Montel, if from any infinite sequence of functions belonging to the family we can pick out a new sequence converging uniformly in the interior of  $\Delta$ . It may converge to a finite limit, which will then be a holomorphic function; or it may tend uniformly to infinity. Various criteria have been given in order that a family may be normal; in particular "equal continuity" of the functions suffices, *i.e.* if the  $\eta$  which one has to take so that, when  $|z - z'| < \eta$ ,  $|f(z) - f(z')|$  is arbitrarily small can be the same for all functions of the family; and also if no one of the functions takes two distinct finite values  $a, b$ , they form a normal family. The theorem of Landau and Carathéodory quoted above follows immediately from this last result. Remarking that conformal representation conserves normality, it is then easy to deduce the theorem on integral functions due to Schottky, that if the function  $f(x) = a_0 + a_1 x + \dots + a_n x^n + \dots$  is holomorphic in a circle of radius  $R$  and does not take the value 0 or 1, then, for any  $\theta$  between 0 and 1 and for  $|x| \leq \theta R$ , we have  $|f(x)| \leq M(a_0, \theta)$ . Picard's theorem, that, for a function  $f(x)$  with an isolated essential singularity at the point  $x_0$ , there are *at most* two values  $a, b$  such that the equation

$$f(x) = a, f(x) = b$$

have no roots in a circle of centre  $x_0$  and suitable radius, can be likewise deduced.

An account of normal families of functions of one variable is to be found in the Borel tract by G. Julia: *Leçons sur les fonctions uniformes à point singulier essentiel isolé*, Paris, 1924; more recently the same author (*Acta Math.* **47**, 1926, 53-115) has begun the study of normal families of functions of several variables, hoping that, as in the case of one variable, the study of the points where the family ceases to be normal would

throw light upon the essential singularities of the limit functions of the family. He obtains the result that, roughly, the points where a family ceases to be normal cannot be isolated, a result completely analogous to those of Hartogs and Levi on the poles and essential singularities of uniform functions of several variables. Julia goes on to examine the properties of the aggregate  $E$  of points where the family is not normal, and applies them to the study of the associated radii of convergence of a double power series.

**ASTRONOMY.** By W. M. H. GREAVES, M.A., Royal Observatory, Greenwich.

*B-Type Stars with Bright Lines.*—It is well known that many stars of type B show in their spectra bright or emission lines as contrasted with the usual dark or absorption lines. During recent years Dr. W. J. S. Lockyer, Director of the Norman Lockyer Observatory at Sidmouth, has devoted a great deal of attention to the study of these stars, and as a result of his work it appears that in many cases the appearance of the lines is variable.

In *Monthly Notices, R.A.S.*, 73, 5, pp. 326–32, March 1923, Lockyer summarises photographs taken (with the same instrument) of the spectrum of  $\zeta$  Tauri, and he finds changes in the appearance of the line  $H_\beta$ . In February 1896  $H_\beta$  was a dark broad line, but in January 1915 it was a thin dark line with strong bright components on the violet side, and in January and February 1923  $H_\beta$  appeared as a thin sharp dark line with strong bright components on each side. The line is clearly variable.

In a subsequent paper (*Monthly Notices, R.A.S.*, 84, 6, pp. 409–25, April 1924) Lockyer presents a study of 21 stars of class B whose spectra contain bright hydrogen lines. These spectra had been photographed and examined at Sidmouth during the period September 1923 and March 1924. Many of these stars had been examined by Merrill during 1911 and 1912 with the Lick 36-inch refractor and some by other observers at various epochs. Lockyer's study leads him to the conclusion that in many cases changes have taken place in the appearance of the hydrogen lines. Thus in the case of 17 Tauri, the Sidmouth plates show  $H_\beta$  and  $H_\gamma$  as double bright lines superposed on broad absorption bands. The red component of  $H_\gamma$  is brighter than the violet. Merrill at the earlier epoch had noted that  $H_\alpha$  was a fine sharp bright line on wide absorption and that  $H_\beta$  was strong absorption. In the case of  $\eta$  Tauri the Sidmouth plates show  $H_\beta$  as a clear double bright line on a broad absorption band. Adams in 1903 had found no traces of a bright line at  $H_\beta$  and Merrill

describes  $H_{\beta}$  as a very broad absorption band with a very faint bright portion within. For other examples the reader is referred to the paper.

In two later papers (*Monthly Notices, R.A.S.*, 85, 7, pp. 580-607, May 1925, and 86, 7, pp. 474-97, May 1926) Lockyer discusses the star  $\phi$  Persei of spectral type B0 p e. In the first paper 55 photographs of the spectrum of this star, taken between September 10, 1923, and April 10, 1925, are discussed, and the second paper deals with 106 photographs covering the period September 9, 1925, to January 21, 1926. Each of the hydrogen lines in the spectrum consists of a broad absorption band on which are superposed two bright lines symmetrically placed with regard to it, and separated from each other by a strong sharp absorption line. Lockyer, from a careful and detailed measurement and analysis of the material, finds that both in the case of  $H_{\beta}$  and  $H_{\gamma}$  the relative intensities of the bright components vary in a cyclical fashion, the period being 126.8 days, the violet component of each line being sometimes stronger and sometimes weaker than the red component. He also finds other cyclical changes in the spectrum. For example, the intensity of the ionised calcium line at  $\lambda$  3933.8 is found to vary and the same is true of the helium lines at  $\lambda$  4471.7 and  $\lambda$  4026.3. For fuller details Lockyer's paper must be consulted.

A similar periodicity is suggested by a study of the bright components of  $H_{\beta}$  in the star H.D.C. 20336 in Camelopardalis (spectral type B3 p e). This star is discussed by Lockyer in *Monthly Notices*, 86, 8, pp. 617-17, June 1926.

*B-Type Stars with Bright Lines in the Cluster  $\chi$  Persei.*—In the *Publications of the Astronomical Society of the Pacific*, 38, 226, p. 350, December 1926, R. J. Trumpler presents the results of a study of this cluster. Nearly all the stars in the cluster down to the thirteenth magnitude are of type B. Trumpler examines 16 B-type stars ranging in photographic magnitude from 8.3 to 10.9 and he finds that 9 of these show indications of hydrogen emission lines. These 9 stars all lie between photographic magnitudes 9.5 and 10.8, and furthermore there is only one star (of photographic magnitude 10.7) lying in this range of magnitude which presents a normal absorption spectrum. As far as this cluster is concerned it would appear that the presence of bright hydrogen lines in the spectrum is associated with the above range of magnitude, and it only needs a knowledge of the parallax of the cluster to convert the range into a range of absolute magnitude. By assuming that the mean absolute magnitude of the A-type stars in the cluster is the same as the mean absolute magnitude of the nearer A-type stars whose parallaxes have been

determined, Trumpler deduces a parallax of  $0''.00044$  and in terms of absolute magnitude the above range becomes  $-2^m.3$  to  $-1^m.0$ . Now the absolute magnitude of a star is known to be very closely correlated with its mass, and Trumpler suggests that "the causes of hydrogen emission become more active for a certain value of the mass, which although large is intermediate in the scale of stellar masses."

*Absolute Magnitudes of Early-Type Stars.*—An account has already been given in these pages of the work carried out at Lick by Ch'ing-Sung Yü, on the continuous absorption in the ultra-violet exhibited in the spectra of stars of early type. It may be recalled that in addition to and associated with the well-known Balmer series of hydrogen lines, there is a region of continuous absorption beginning on the red side of the limit of the Balmer series at  $\lambda$  3646 and extending downwards into the ultra-violet. In *Lick Observatory Bulletin* 380, 1926, Ch'ing-Sung Yü carries his researches further. In the course of his previous measures of the amount of the hydrogen absorption he had incidentally measured the effective temperatures of the stars concerned, and from a study of 39 of these stars for which determinations of absolute magnitude were available, he deduces a relation between the absolute magnitude of a star, its temperature, and the amount of the continuous absorption referred to above. This relation is as follows :

$$M = 11.15 R - \frac{T}{1600} + 3.25.$$

where  $M$  = absolute magnitude.

$T$  = effective temperature measured relatively to  $\zeta$  Ophiuchi, for which a temperature of  $22000^\circ$  K is assumed.

$R$  = percentage absorption immediately to the violet side of the head of the Balmer series.

From the measures of  $T$  and  $R$  values of  $M$  are deduced for 63 stars, and by comparison with the apparent magnitudes, parallaxes for these stars are obtained.

**ORGANIC CHEMISTRY.** By J. N. E. DAY, M.Sc., A.I.C., University College, London.

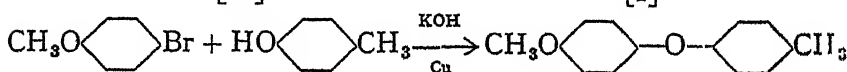
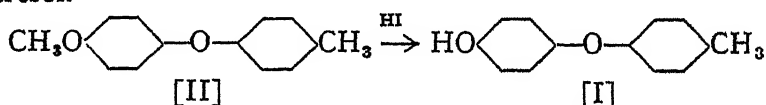
*Thyroxine.*—Two important papers have been published recently (*Biochem. J.*, 1926, **20**, 293, 300) by C. R. Harington on thyroxine, the active principle of the thyroid gland.

The first paper gives a greatly improved method for the extraction of thyroxine from the thyroid gland, by means of

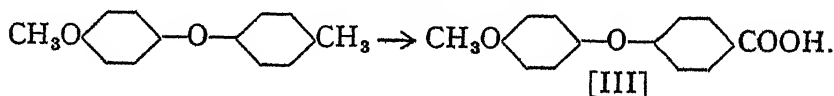
a solution of barium hydroxide, in a yield of about 0.12 per cent. of the dried gland. It is shown, by physical properties and physiological tests, to be the same substance as previously obtained. Previous work on thyroxine, suggesting that it was an indole derivative of the empirical formula  $C_{11}H_{10}O_3NI_3$ , is discussed and dismissed, and the empirical formula  $C_{15}H_{11}O_4NI_4$  is shown to agree with the analytical results now obtained.

The second paper deals with the structure of the substance, named desiodo-thyroxine, obtained by the removal of the iodine from thyroxine. This iodine was removed by shaking a 1 per cent. solution in N. potassium hydroxide in an atmosphere of hydrogen in the presence of a palladium catalyst. The empirical formula of the desiodo-thyroxine so obtained was found to be  $C_{15}H_{15}O_4N$ , and its reactions suggested that it was an  $\alpha$ -amino acid. The amount of hydrogen absorbed was found to be 4 mols. per mol. of thyroxine. These facts indicate that the iodine had been replaced by hydrogen without further changes taking place.

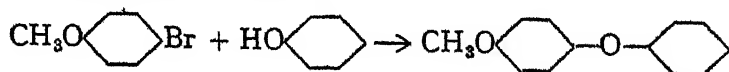
When desiodo-thyroxine was fused with potassium hydroxide the products isolated were *p*-hydroxy benzoic acid, a small amount of quinol, ammonia, oxalic acid, and a substance  $C_{13}H_{12}O_2$ , which has been shown to be 4-(4'-hydroxyphenoxy) toluene (I), which was also prepared from 4-(4'-methoxyphenoxy) toluene (II), the condensation product of *p*-bromo anisol and *p*-cresol.



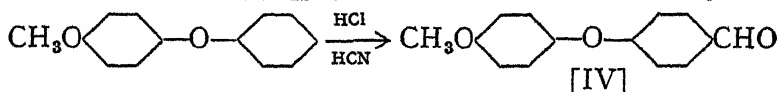
On exhaustive methylation and removal of trimethylamine, an unsaturated acid  $C_{16}H_{14}O_4$  [V], containing one methoxy group, was obtained. On oxidation this gave a neutral substance  $C_{14}H_{12}O_3$  [IV], and on further oxidation an acid  $C_{14}H_{12}O_4$  [III]. The substance [III] was proved to be 4-(4'-methoxyphenoxy) benzoic acid, also obtained by the oxidation of the 4-(4'-methoxyphenoxy) toluene.



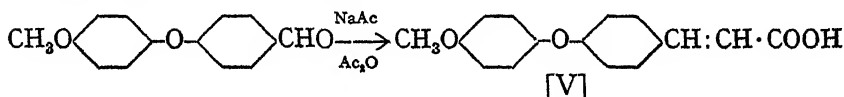
Condensation of *p*-bromo anisol with phenol gave 4'-methoxyphenoxy benzene.



From this compound 4-(4'-methoxyphenoxy) benzaldehyde [IV] was prepared, and was proved to be identical with the neutral substance  $C_{14}H_{19}O_3$  obtained from desiodo-thyroxine.

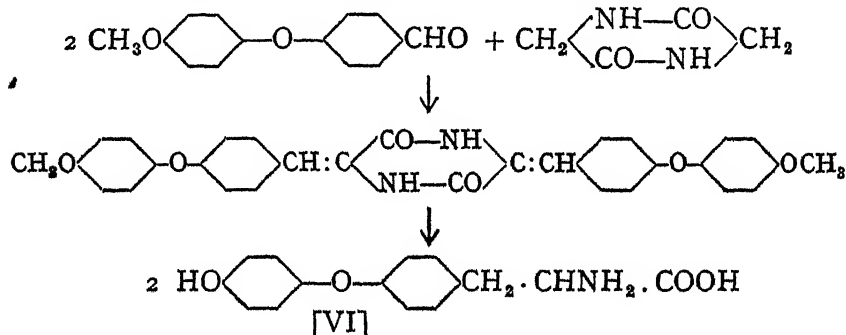


On oxidation it gave 4-(4'-methoxyphenoxy) benzoic acid [III]. From this aldehyde 4-(4'-methoxyphenoxy) cinnamic acid [V] was obtained, and shown to be identical with the unsaturated acid  $C_{16}H_{14}O_4$ .

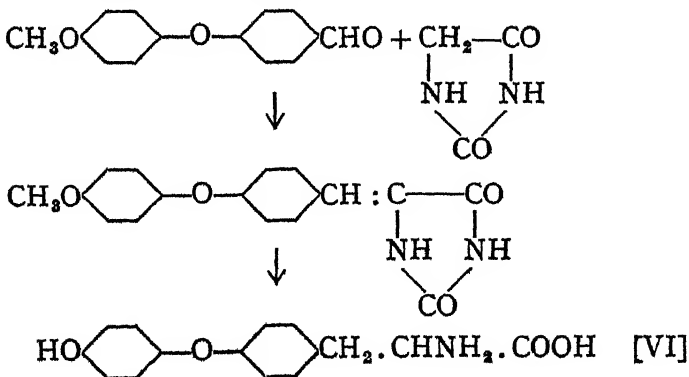


Desido-thyroxine [VI] has been synthesised by two methods from the aldehyde [IV].

I. By condensation with glycine anhydride in presence of acetic anhydride and sodium acetate, and boiling the condensation product with red phosphorus and hydriodic acid :

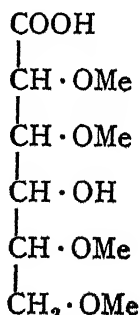
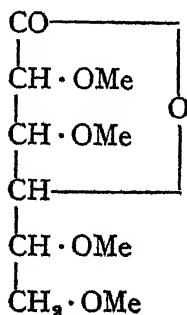
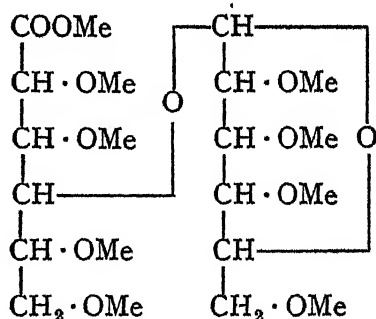
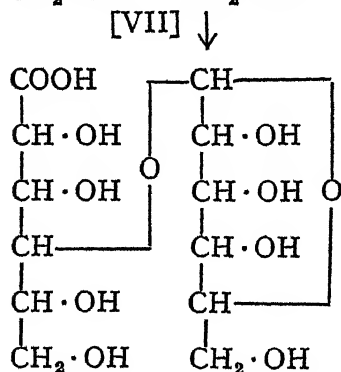
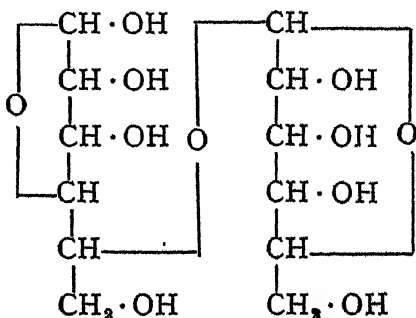
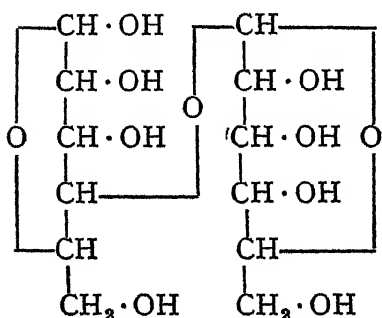


II. By condensation with hydantoin and subsequent boiling with hydriodic acid and red phosphorus :



Desiodo-thyroxine is therefore  $\beta$ -[4-(4'-hydroxyphenoxy) phenyl]- $\alpha$ -amino propionic acid, and thyroxine the tetra iodo derivative. The author states that incomplete experiments indicate that the iodine atoms are in the 3,5,3',5', positions.

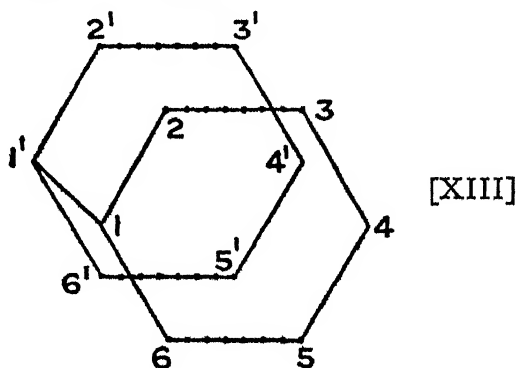
*Maltose*.—A further paper has appeared on the constitution of maltose (Haworth and Peat, *J.C.S.*, 1926, 3094). Up to the time of publication of this paper the structure had been narrowed down to either [VII] or [VIII]. See this journal, 21, 210; for formula [XIV] on p. 213 read formula [VIII] below.



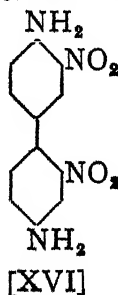
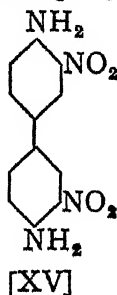
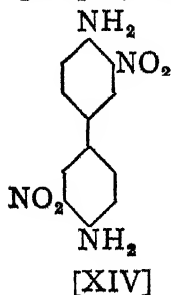
Haworth and Peat have now decided in favour of [VII], as by the following series of reactions they finally obtained a  $\gamma$ -lactone, and formula [VIII] would give a  $\delta$ -lactone : maltose on oxidation gave maltobionic acid [IX], and this on methylation gave methyl octamethylmaltobionate [X]. This on hydrolysis gave a tetramethyl gluconic acid [XI], together with crystalline tetramethyl glucose. The acid on heating gave tetramethyl gluconolactone [XII]. This lactone was identified as a  $\gamma$ -lactone by the determination of its rate of hydrolysis and comparison with known  $\gamma$ - and  $\delta$ -lactones, and was found to be 2 : 3 : 5 : 6 tetramethyl  $\gamma$ -gluconolactone.

The phenylhydrazide was prepared from the lactone, and was found to be identical with a specimen of the phenylhydrazide prepared from 2 : 3 : 5 : 6-tetramethyl gluconic acid. Thus, assuming that the position of the biose linkage is not altered during methylation or oxidation, when compounds [IX] and [X] were prepared, the structure of maltose will be given by [VII].

*Diphenyl.*—Several authors have published recently papers dealing with the structure of diphenyl. The Kaufler formula [XIII] has been much scrutinised.

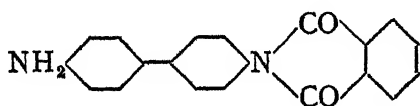


Strakosch (*Ber.*, 1872, 5, 236), by nitrating diacetylbenzidine and hydrolysis, obtained a dinitrobenzidine [XIV], in which both nitro groups have been shown to be ortho to the amino groups (Brady and McHugh, *J.C.S.*, 1923, 123, 2047).

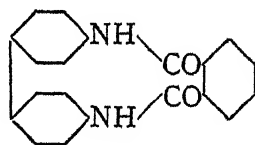


Bandrowski (*Monatsh.*, 1887, **8**, 471), by nitrating diphtalylbenzidine and hydrolysis, also prepared a dinitrobenzidine, which was later shown by Cain, Coulthard, and Micklethwait (*J.C.S.*, 1912, **101**, 2298; 1913, **103**, 586, 2074; 1914, **105**, 1437) to be different from the first one. These workers represented the compound prepared by Strakosch as 3 : 5'-dinitrobenzidine [XIV], and that prepared by Bandrowski as 3 : 3'-dinitrobenzidine [XV]. It was shown by Brady and McHugh (*J.C.S.*, 1923, **123**, 2048) that the two tetra-amines obtained by reduction of the nitro compounds were different, but that, as stated by Cain, the diquinoxalines prepared from them appeared to be the same, if the evidence of a mixed melting-point could be relied upon.

Dennett and Turner (*J.C.S.*, 1926, 476) showed that in the case of 4 : 4'-dibromodiphenyl, unsymmetrical nitration takes place, the product of nitration being the 2 : 3'-dinitro compound. The presence of one nitro group in the 2 position was proved by the synthesis of this compound from 2-nitro benzidine. Following this result, Le Fèvre and Turner (*J.C.S.*, 1926, 1759) prepared 2 : 3'-dinitro benzidine from the acetyl derivative of 2-nitro-benzidine. This dinitro benzidine was converted into the dibromodinitrodiphenyl, which was shown to be identical with the 4 : 4'-dibromo-2-3'-dinitrodiphenyl previously prepared, and it has thus been shown to have the 2 : 3' structure [XVI]. This dinitro compound has now been shown to be identical with Bandrowski's dinitrobenzidine. In the latter case therefore the nitro groups must be in the 2 : 3' positions. This is of interest, showing as it does that unsymmetrical nitration can take place with phthalyl derivatives (compare Brady, Quick, and Welling, *J.C.S.*, 1925, **127**, 2264). Reference may also be made to two papers by Bell and Kenyon (*J.C.S.*, 1926, 2705, 3044) on investigations in the diphenyl series. In connection with monophthalylbenzidine, it is suggested by Le Fèvre and Turner (*J.C.S.*, 1926, 2476) that this compound should be represented by [XVII] in place of [XVIII] which had been given to it previously, and that this will enable one to account for the formation of the 2-nitro derivative.



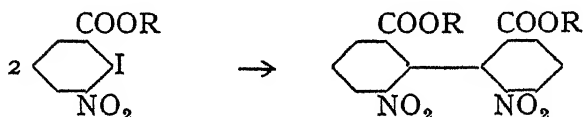
[XVII]



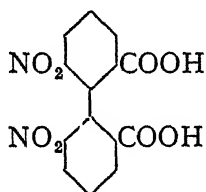
[XVIII]

Kenner and Stubbings (*J. C. S.*, 1921, **119**, 593) prepared 6 : 6'-dinitrodiphenic acid by treating the ester of 2-iodo-3-

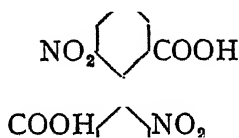
nitrobenzoic acid with copper powder, and hydrolysing the product.



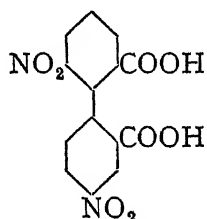
They found that this acid was different from the dinitrodiphenic acid prepared by Schultz (*Annalen*, 1880, **203**, 95), in which the nitro groups were also given the 6 : 6' positions. The evidence for this structure was given by Schmidt (*Ber.*, 1903, **36**, 3745), who stated that he obtained 2 : 2'-dinitrodiphenyl from it. Kenner and Stubbings therefore gave these acids the following structures [XIX] and [XX].



[XIX]



[XX]



[XXI]

Christie, Holderness, and Kenner (*J.C.S.*, 1926, 671) have now re-examined Schultz's acid, and have obtained 2 : 4'-dinitrodiphenyl from it, thus showing that Schultz's acid will be 4 : 6'-dinitrodiphenic acid [XXI].

**RADIOACTIVITY AND GEOLOGY.** By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow.

THE discussion excited by Prof. J. Joly's remarkable theory connecting the geological cycle with the thermal cycle controlled by the radioactivity of the rocks still continues with great vigour (*SCIENCE PROGRESS*, Jan. 1924, pp. 382-3; April, 1926, pp. 580-1; Jan. 1927, pp. 544-5). Dr. H. Jeffreys has criticised some of the fundamental data of the theory and the conclusions Prof. Joly has drawn from them (*Phil. Mag.* (7), 1, 1926, pp. 923-31). He says that Prof. Joly's theory requires : (a) that the granitic layer of the continents should be about 30 km. thick, leading to fusion below it on account of excess radioactivity ; (b) that if fusion occurs, the upper solid layer should be much thinner below the oceans than in the continents ; (c) that tidal friction in such conditions should make the outer shell rotate with respect to the interior ; (d) that the result of such rotation should be to restore solidity in the liquefied layer. Reasons are given for believing that the estimate of

crustal thickness (*a*) is probably excessive ; that the arguments for (*b*) are erroneous ; that tidal friction is not adequate to produce the required rotation of the outer shell (*c*) ; and that fusion, once initiated, is likely to be permanent (*d*).

In his answer to this criticism (*Phil. Mag.* (7), 1, 1926, pp. 932-9), Prof. Joly points out that his object is to show that there is a periodic accumulation and discharge of heat within the crust, and not that there is any periodic variation of temperature. The heat involved is latent, and there is little actual variation of temperature. The latent heat of the basaltic substratum is conveyed upward mainly by convective circulation. Incidentally in this reply Prof. Joly introduces some ideas which the writer thinks will help to explain the variations in composition of "plateau-basalts" in different parts of the crust.

In his Presidential Address to the Geological Society of London on "Regions of Compression," Dr. J. W. Evans (*Quart. Journ. Geol. Soc.*, 82, pt. 3, 1926, *Proc.*, pp. lx-cii) points out that a considerable part of the energy liberated by radioactive elements occurring in rock constituents may be used up in promoting physical, chemical, or atomic changes (*e.g.* pleochroic haloes) in the surrounding minerals, and that only a small proportion may thus be converted into heat. Prof. A. Holmes and Dr. R. W. Lawson, however, have supplied an answer to this criticism in a recent paper on the radioactivity of potassium and its geological significance (*postea*). They conclude that the energy of the radioactive rays expended in the manner suggested by Dr. Evans does not amount to 1 per cent. of the total energy, and is probably not more than one-tenth of this value.

G. R. MacCarthy (*Geol. Mag.*, 63, 1926, pp. 301-5) has criticised the theory of Joly and Holmes chiefly on the ground that any mechanism of the periodic dissipation of accumulated heat through the alternate fusion and solidification of a basaltic substratum would involve the foundering of the ocean floor at the climax of each period of fusion. He believes that the escape of heat from even a 10-km. thick zone of basalt would be sufficient to raise the temperature of the present oceans to boiling-point. Alternatively no periodic climax occurs at all, and any small accumulation of heat is dissipated by the escape of magma through fissures on to the ocean floor.

Prof. A. Holmes and Dr. R. W. Lawson have shown that a modification of the Joly theory, especially in regard to the thickness of the continents, is necessitated by the radioactivity of potassium (*Phil. Mag.* (7), 2, 1926, pp. 1218-33). Potassium is vastly less active than uranium or thorium, but because it is enormously more abundant in the rocks its aggregate heat

production increases the total amount of heat supplied by about one-third of that due to uranium and thorium combined. This necessitates the reduction of the continental thickness from 30 km. (on the old assumptions) to less than 20 km., with 15 km. as the most reasonable average figure. With this thickness the temperature at the bases of the continents is hundreds of degrees lower than any possible fusion-point, and consequently cooling of the substratum by conduction through the continents becomes an important process. The application of the principle of isostasy under the new conditions leads to another important inference, namely, that the density of the substratum must be 3.4 or 3.5. This higher figure is required in order to provide the continents with the necessary buoyancy to stand as high as they do. As a corollary the material of the substratum can hardly be basalt as we know it. There is only one known kind of rock of the composition of basalt or gabbro which has a density of 3.4 or 3.5, and that is *eclogite*, which may be regarded as a high-pressure facies of basalt. The assumption that the substratum consists of eclogite satisfies the isostatic, seismic, thermal, and petrologic data.

That a presumed ultrabasic layer (peridotite) beneath the basaltic (eclogitic) substratum may complicate the simple thermal cycle by superimposing a rhythm of longer period upon those due to the accumulation and dissipation of radioactive heat in the basaltic substratum was fully recognised by Prof. Joly in his Halley Lecture on "Radioactivity and the Surface History of the Earth" (Oxford: 1924, pp. 40); and the implication in the writer's review of *The Surface-History of the Earth* (SCIENCE PROGRESS, Jan. 1927, p. 545), that Prof. Joly had omitted to take the peridotite layer into account, is incorrect. This subject, already treated in some detail by Prof. A. Holmes (see SCIENCE PROGRESS, April 1926, pp. 580-1), is further dealt with by him in a new paper entitled "Contribution to the Theory of Magmatic Cycles" (*Geol. Mag.*, 63, 1926, pp. 306-29), which is largely based on the new data provided by the recognition of the radioactivity of potassium, and the reduced thickness of the continents.

Dr. Holmes first of all answers the criticisms of G. R. MacCarthy. He shows that there is no possibility of fissures permitting egress of basalt reaching down to the zone of accumulating magma. The heated rocks immediately above the expanding substratum accommodate the tension by flowage; and the zone of flowage must stretch outwards and become thinner, like the stretched rubber of a slowly-inflated balloon. On this view geosynclines are especially weak belts riddled with the products of fusion, which may be compared with the thinner and weaker spots in an inflated balloon. The rate of

expansion is so extremely slow that atomic or molecular mobility should be amply sufficient to prevent the zone of flowage from general bursting, and permitting the escape of magma.

For suboceanic regions the newer interpretations of seismic data reveal a surface layer from 4 to 8 km. in thickness. As the substratum is now believed to have a density of 3.4 (eclogite), Dr. Holmes finds it necessary to postulate the occurrence of a column 20 km. thick of material of density 2.7, in order to maintain the isostatic balance of oceanic islands like Hawaii. The assumption that the suboceanic surface layer consists of alkali-syenite accords well with this view. The present writer thinks that if the column of rock beneath oceanic islands were made of basalt of D. 2.9, no drastic increase of its thickness would be necessitated for isostatic balance. Similarly would not a suboceanic surface layer of basalt on top of material in the eclogite phase be in accordance with seismic evidence? The occurrence of trachyte, trachyandesite, etc., in oceanic islands is only on a very small scale compared with the amount of basalt; and these rocks are best interpreted as differentiation products of the basaltic magma, of which the complementary ultrabasic types are ankaramite and oceanite.

Dr. Holmes then goes on to outline some interesting and important speculations as to the mechanism by which the regional ascent of magma is accomplished. He imagines a process of "crystal stoping," whereby advancing fusion operates along crystal boundaries producing a film-like network of liquid. At a later stage crystals are liberated and sink into the growing magma beneath. It is shown that by a repetition of this process low-temperature magmatic components will be concentrated in upper layers, and further invasions of magmatic conditions will no longer be possible. Successive granites of the Pre-Cambrian shields of Mozambique and Finland show increasing proportions of potash-felspar and radium. There is here a clue to the reason why there has been no widespread granitic invasion since the Pre-Cambrian; and why diorites and andesites, the complementary products, are abundant in later zones of mountain folding.

The growth, ascent, and consolidation of basaltic magma are then dealt with: and finally the effects due to a peridotite cycle are further analysed. Holmes believes that the effects due to basaltic cycles are feeble compared with those due to a peridotite cycle; and, moreover, they may not be widespread or fully completed. He divides the peridotite cycle very tentatively into four stages, each of which is correlated with certain tectonic and petrologic effects, and its interactions with concurrent basaltic cycles elucidated. The view that the earth

is now at the end of a major cycle, and that solidity prevails on the whole in the depths of the crust, is supported by the distribution of modern earthquakes in depth, which shows, according to Oldham, that changes of bulk are affecting the substratum through a great thickness.

Dr. H. Jeffreys returns to the attack on the Joly-Holmes theory in a paper on "The Earth's Thermal History and Some Related Problems" (*Geol. Mag.*, 63, 1926, pp. 516-25). He denies that the theory of the continuous cooling of the earth fails to explain the tectonic and petrologic phenomena; but his main weapon is the proposition, based on general physical grounds, that permanent alternations of temperature in the earth's crust are not possible. "... whenever a physical system of finite extent, free to lose heat by radiation from an outer boundary, is affected by a steady internal source of heat, the temperature at any point will approach steadily towards some permanent value as a limit, and there will be no possibility of a permanent oscillation of temperature." Fusion and rotation of the outer crust may import some complication, but the general result remains unchanged. Dr. Jeffreys also discusses the thickness of the granitic layer, and the upward transfer of radioactive material by the concentrating action of volatile constituents, wherein he finds himself in substantial agreement with Holmes.

The seismic data relating to the structure and thickness of the continents have been re-examined by Dr. Jeffreys by the method of least squares (*Nature*, Sept. 25, 1926, p. 443). The results show that there is an upper layer of the crust that transmits compressional waves with a V. 5.6 km. sec., and a lower one with V. 7.8 km. sec. The Taucrn earthquake gave rise to a wave of V. 6.2 km. sec., which must have travelled within an intermediate layer. The result for the upper layer corresponds to that found for granite. According to L. H. Adams and R. E. Gibson, in a paper on the compressibilities of dunite and of basalt glass and their bearing on the composition of the earth (*Proc. Nat. Acad. Sci.*, 12, 1926, pp. 275-83), the velocity of waves in basaltic glass at ordinary temperatures and under a pressure corresponding to a depth of some tens of kilometres, is 6.4 km. sec., whilst in dunite the V. is 8.4 km. sec. If allowance be made for higher temperature in depth, the layer beneath the granite may thus well be basaltic glass, as suggested by Daly, and a lower layer may be dunite. Hence the downward distribution of rock types according to Jeffreys may be granite—basalt glass—dunite; according to Holmes it is granite—diorite—eclogite—dunite.

Prof. A. Holmes has recently discussed the question of estimates of geological time, with special reference to thorium

minerals and uranium haloes (*Phil. Mag.*, May 1926, pp. 1055-74). He dismisses Joly's sodium method on account of a lack of reliable data, and lack of adequate knowledge of the geochemical cycle of sodium salts. Lead ratios from thorium minerals give highly variable estimates of geological age. Holmes shows that there is no reason to accept the lower estimates by this method, as has been done by Joly. On the contrary, the higher estimates, which agree fairly well with the figures derived from uranium minerals, are the more reliable.

Holmes suggests that the lead in thorium minerals is in the form of an oxide or silicate, a relatively soluble form as compared with the highly insoluble uranate in which it is probable that the lead exists in uranium minerals. Hence lead in thorium minerals is more likely to be leached out by circulating waters and lost, than in uranium minerals. Further, pleochroic haloes due to uranium provide no evidence that uranium has disintegrated more rapidly in the geological past, as Joly has contended. Consequently the shorter estimates of geological time favoured by Joly on this basis are invalid. Some of the points here raised are further discussed by Holmes in a letter on "Rock-lead, Ore-lead, and the Age of the Earth" (*Nature*, April 3, 1926, p. 482), and in a criticism of Prof. Joly's Boyle Lecture on "The Geological Age of the Earth" (*Nature*, April 24, 1926, pp. 592-4).

In a paper on "Radioactive Minerals as Geological Age Indicators" H. N. Ellsworth (*Amer. Journ. Sci.*, 9, 1925, pp. 127-44) also decides against Joly in the matter of the relative reliability of age estimates derived from thorium minerals. He thinks that the results from uranium minerals are, up to date, the most reliable and consistent. Age estimates derived from Canadian Pre-Cambrian uraninites are in remarkable mutual accord, giving figures averaging about 1,150 million years.

Extraordinarily detailed analyses of uraninites from Katanga (Central Africa) and South Dakota, and of asphaltite from Utah, have been made by C. W. Davis (*Amer. Journ. Sci.*, 11, 1926, pp. 201-17). The age of the Katanga uraninites is estimated from the Pb/Ur ratio as 665 million years. The South Dakota examples, however, are 1,667 million years old, and these are the oldest minerals yet noted. The Utah asphaltite gives 20.5 million years for its age. As shown by Holmes, the factor 7,900 used in these calculations by Ellsworth and others should now be replaced by 6,600, which would, of course, reduce the above estimates in an equivalent proportion.

By the same method, and using the same factor as the American investigators, L. A. Cotton has estimated the ages of a number of Australian uranium and thorium minerals

(*Amer. Journ. Sci.*, 12, 1926, pp. 41-6). Apart from two altered minerals, which gave abnormal values, the figures obtained were in good agreement with those for Pre-Cambrian minerals from other continents.

**ENTOMOLOGY.** By J. DAVIDSON, D.Sc., Rothamsted Experimental Station, Harpenden.

NOTE.—The literature on Entomology has grown so large of recent years that new contributions can only be briefly noted in this short review. Papers dealing with the applied aspect of the subject are not included here, except in those cases in which the contents may be of general interest, as this aspect of Entomology is so admirably dealt with in the *Review of Applied Entomology, Series A and B*, by the Imperial Bureau of Entomology.

*General Entomology.*—*The Natural History of Wicken Fen*, pt. ii (1925), pt. iii (1926), edited by J. Stanley Gardiner, contain some interesting observations on the insect fauna of the fen. The British Museum (Nat. Hist.) have issued a 4th edition of the *Guide to the Exhibited Series of Insects* (price 1s.), which is a necessary guide to understanding the classification, forms, and habits of the chief groups; the main collection of insects at the Museum is now estimated at 3,500,000 specimens of about 250,000 named species. R. N. Chapman has brought out a valuable mimeographed volume entitled *Animal Ecology, with Special Reference to Insects* (Minneapolis: Burgess-Brooke Inc., 1926). A large mass of data has been accumulated, the volume representing the outline of a course of Ecology of Insects which has been given for some years at the University of Minnesota. E. A. Cockayne (*Trans. Entom. Soc., London*, 74, 203-29) describes some interesting cases of homœosis and heteromorphosis culled from literature on Entomology. Bateson in 1894 defined homœosis as a variation which consists in the assumption by one member of a meristic series, of the form and character proper to other members of the series, such variations constituting a distinct group of phenomena; heteromorphoses are tissues or organs formed before development has been completed, built up of normal cells, but situated in an anomalous position. E. O. Essig has written a well-illustrated work on the *Insects of Western North America* (New York: The Macmillan Co., 1935 pp.); the book is particularly important from the economic and technical aspect of the subject. Another important book is entitled *Insects of Australia and New Zealand*, by R. J. Tillyard (Sydney: Angus & Robertson, Ltd., 1926, 560 pp., price 42s.). The subject-matter is arranged in a systematic manner, and illustrations are given

of the more important insect types of these countries ; the illustrations and figures are 1,251 in number, the great majority of them being original. The *Verhandlungen des III Internat. Entomologen-Kongresses*, Zurich, 1925 (Weimar: G. Uschmann, 1926, Bd. I & II, 72 + 646 pp.), contains numerous papers of interest. A comprehensive volume of 601 pp. and 1,088 figs., comprising the results of seventeen years' studies on plant galls found in the Dutch East Indies and caused by nematodes, mites, and insects, is the work of J. Docters van Leeuwen and W. M. Docters van Leeuwen ; 1,536 plant galls are described, the causal organism being identified in some 300 cases. Another important addition to the literature is vol. iv of the well-known *Handbuch der Pflanzen Krankheiten* (Berlin: Paul Parey, 1925, 483 pp., 218 figs.) ; this volume is the work of Dr. L. Reh, of Hamburg.

The systematist will be interested in the additions in various insect groups in *Fauna Bruana* (*Treubia*, 1926, 7, 217-330), as well as the *Catalogus Insectorum Jamaicaensis*, by C. C. Gowdey (*Dept. Agric. Jamaica*, Entom. Bull. No. 4, 1-114). An excellent review of leaf-mining insect larvæ, compiled from widely scattered literature and many years' special studies by the author, is given by M. Hering in a work entitled *Die Ökologie der blattminierenden Insekten larven* (Berlin: Gebrüder Borntraeger, 1926, pp. iv + 253, price 18 gold Marks).

It has long been known that the mid-intestine of insects performs the dual function of digestion and absorption. Osamu Shinoda (*Mem. Coll. of Science*, Kyoto University, 1926, Ser. B, 2, no. 2), from studies carried out, chiefly on the larva of the Saturniid moth, *Dictyoploca japonica*, finds that the epithelial layer is composed of goblet cells and ciliated cylindrical cells, which he considers are homorphous at different phases of their activity: the cylindrical cells are those in the phase of secretion and absorption, while the goblet cells are only a resting phase of the cylindrical cells.

The introduction of natural enemies for the control of insect pests is the chief method employed in Hawaii and some well-established successful results have been obtained. O. H. Swezey (*Jl. Econ. Entom.*, 19, 714-20) discusses the recent introductions of beneficial insects into the island, which includes *Cyrtorhinus mundulus*, a mirid bug from Australia and Fiji, which feeds on the eggs of the sugar-cane leaf hopper ; this insect, which was introduced in 1920, has successfully controlled the leaf hopper.

F. V. Theobald (*Jl. Royal Hort. Soc.*, 51, 314-23) has an interesting paper on insects caught in light-traps, the result of two years' records being given ; the author concludes, from the records obtained with the *Medusa* type of lamp, that much

good can be done with light-traps in destroying Tortrix moths, when used in plantations which are infested with these insects.

C. B. Williams (*Trans. Entom. Soc., London*, **74**, 193-202) has some further records on insect migration; and in another paper (*The Entomologist*, **59**, 281-8) discusses the question of the voluntary or involuntary migration of butterflies.

*Orthoptera*.—The values for the hydrogen-ion concentration in the blood of certain species of grasshoppers have been estimated by J. H. Bodine (*Biol. Bull.*, 1926, **51**, 363-9). The pH values were determined by micro-electrometric method; considerable variation was found, and no correlation appears to exist between blood pH and age or sex. The average pH values for the blood of 4 species of grasshoppers examined were found to be 6.73; 6.68; 6.79; 6.73.

A. A. Granovsky (*Jl. Econ. Entom.*, 1926, **19**, 791-5) gives a brief review of Russian literature concerned with the control of migrating grasshoppers by airplane dusting. In *Treubia*, 1926, **9**, livraison 1-3, a lengthy account of the Orthoptera of Malay is given by H. H. Karny.

*Coleoptera*.—The occurrence in the Bordeaux area of France after the war of the well-known Colorado beetle (*Leptinotarsa decemlineata*) has necessitated strong measures being taken since 1922, in order to exterminate this destructive potato pest; these control measures have successfully restricted the spread of the insect, and J. Feytaud (*Rev. Zool. Agricole*, 1926, No. 5, 65-77) reviews the actual position regarding the infested area at the beginning of the campaign of 1926.

An important paper on British Bark Beetles (*Forestry Commission*, London, 1926, Bull. No. 8, 77 pp.) by J. W. Munro deals with their general biology, importance in forestry, structure and classification, and brief accounts of the genera and species; useful identification keys are also given. G. B. Walsh (*Entom. Mo. Mag.*, 1926, **62**, 221-31, 257-61) discusses the origin and distribution of the coast Coleoptera of the British Isles.

*Lepidoptera*.—The importance of the characters of the male genitalia of Microlepidoptera from the systematic point of view is clearly recognised, and R. J. Eyer (*Ann. Entom. Soc., America*, 1926, **19**, 237-44) discusses the significance of these characters in relation to family and sub-family groups. R. W. Glaser (*Ann. Entom. Soc., America*, 1926, **19**, 180-9) describes the symptoms of diseased silkworms attacked by the green muscardine disease, which is due to a fungus (*Metarrhizium anisopliae*); the author found that silkworms do not acquire the disease by swallowing the spores, but, when the spores lodge on the skin, germination and infection soon occur.

An important volume from the British Museum (Nat. Hist.) by G. F. Hampson is entitled *Descriptions of New Genera*

and *Species of Lepidoptera (Phalæne) of Fam. Noctuidæ*, and contains 641 pages. Another important work is a *Monograph of the Tribe Hesperiidæ* (European species), by B. C. S. Warren (*Trans. Entom. Soc., London*, 1926, 74, pt. 1).

J. Heslop-Harrison (*Journ. of Genetics*, 1926, 17, 1-9) shows that in interspecific crosses between *Tephrosia crepuscularia* and *T. bistorta*, melanism introduced by *T. bistorta* remains as within the limits of the species, a Mendelian recessive.

Biochemical investigations on insects is a phase of entomology which has not received much attention, and increased knowledge would certainly help us to a better understanding of the problems of insect physiology, ecology, and insect control; in this respect the paper by H. S. Swingle (*Ohio Jl. Sci.*, 1925, 5, 209-18) on the digestive enzymes of an insect is of interest, as also is the paper by W. Rudolfs (*Jl. New York Entom. Soc.*, 1926, 34, 249-56) on the chemical changes which take place during the life-cycle of the tent caterpillar (*Mala-cosoma americana* F.). Rudolfs has made chemical analyses of this caterpillar during various stages of its life, particularly with reference to moisture-content and the presence of fats; moisture was found to be lowest at the time of hatching (39.4 per cent.) and highest during the larval instars 3-5 (83-85 per cent.); fat, calculated on a dry basis, was lowest upon hatching (0.66 per cent.) and highest when the larvæ had just pupated (28.8 per cent.).

B. Trouvelot (*Ann. des Épiphyties*, 1926, 10, 136 pp.), dealing with the potato-moth (*Phthorimæa operculella* Zell.), a species which the author states was introduced into France about fifteen years ago, discusses in some detail the factors concerned in favouring or limiting the increase of an injurious insect.

*Hemiptera*.—H. Spencer (*Ann. Entom. Soc., America*, 1926, 19, 119-53) has an interesting account of the biological relationships existing between certain species of aphids and their parasites and hyper-parasites. P. Vayssièrre (*Ann. des Épiphyties*, 1926, Nos. 4 and 5, 197-382) contributes an important biological and systematic study of the sub-family Monophlebinae (Coccidæ), and Inokichi Kuwana (*Japan Imperial Plant Quarantine Service Techn. Bull.*, No. 4, 1-44) deals with the Diaspine Coccidæ of Japan, Part IV.

*Diptera*.—A. E. Cameron (*Bull. Entom. Res.*, 1926, 17, 1-42) has an important paper on the Tabanidæ of the Canadian prairie: the bionomics of Tabanidæ are next to the Culicidæ in economic importance as blood-sucking insects, and the information contained in this paper is a welcome addition to the literature; data regarding oviposition, seasonal occurrence of the broods, description of species, and hymenopterous parasites of the early

stages are given. Another important work on Tabanids is by J. H. Schaurmans Stekhoven (*Treubia*, 1926, 6, Supplement, pp. 1-551, Pls. 18) entitled "The Tabanids of the Dutch East Indian Archipelago"; it would appear that one of the chief reasons for the preparation of this important monograph is the fact that some of these insects, particularly *Tabanus striatus*, act as vectors of the disease known as Surra: 266 species are here described, many being new to science. F. W. Edwards (*Trans. Entom. Soc., London* (1926), 74, 389-426) gives a useful systematic account of the British biting midges (Ceratopogonidæ) with identification keys.

D. E. Minnich (*Biol. Bull.*, 1926, 51, 166-78), who has previously shown that the tarsi of certain butterflies possess sense-organs which are sensitive to chemical stimuli, has also experimented with the muscid flies *Phormia regina* Meigen, *P. terræ-novæ* R.D., and *Lucilia serricata* Meigen: Minnich shows that these flies extend the proboscis under appropriate chemical stimuli of the tarsi; by means of these reactions it can be shown that the chemoreceptors in the tarsi serve as organs of taste: the oral lobes of the proboscis are also equipped with organs of taste.

R. A. Wardle and E. A. Taylor (*Proc. Zool. Soc.*, 1926, pt. i, 1-23) describe the cephalic skeleton of contrasting types of crane-fly larvæ; the larvæ of these insects exhibit great variety of habitat: the authors consider that the Pedicine type of larva is more generalised as regards its cephalic morphology than the Tipuliine types. A further paper by the senior author in the same journal (pp. 25-48) deals with the respiratory structures of two larval members of the Tipulidæ, *Tipula flavolineata* Meig. and *Pedicia rivosa* L., which offer great contrasts in habits of larval life.

*Other Orders.*—G. C. Crampton (*Psyche*, 1926, 33, 78-85) discusses the affinities of *Grylloblatta* as indicated by a study of the head and its appendages in *G. campodeiformis* Walk. and *G. barberi* Caud.; the author concludes that this evidence supports the previous views proposed by him regarding the inter-relationships of the Orthopteroid and Isopteroid insects. Two papers on Thysanoptera of interest to students of these insects are by H. Priesner (*Treubia*, 1926, 8, Supplement, 1-264, 16 plates) on Thysanoptera of Malay, and H. H. Karny (*Mem. Dept. Agric. India, Entom. Series*, IX, No. 6, 187-239) on some Indian Thysanoptera.

**PEDOLOGY.** By Prof. G. W. ROBINSON, M.A., University College of North Wales, Bangor.

*International Society of Soil Science.*—In last year's "Recent Advances" reference was made to the activities of this body.

The Society now includes practically all workers in the subject in all countries and affords a striking example of international scientific co-operation, for it has already succeeded in securing co-ordinated action with regard to those branches of pedological investigation which specially demand this type of attack. The Society is divided into a number of commissions which unite workers interested in the principal branches of the subject. Apart from the full meetings of the Society, to be held every few years, meetings of the different commissions are to be held in the intervals. During the past year the First Commission (Soil Physics) met at Rothamsted to arrange for concerted action in the matter of mechanical analysis of soils. The Second Commission (Chemical Soil Analysis) held a meeting at Groningen, Holland, for the discussion of Base Exchange and Soil Acidity. The Third Commission (Soil Classification and Cartography) held a meeting in Hungary which afforded workers from different countries an opportunity of discussing certain important European soil-types in the field.

The several commissions in their meetings formulated certain general conclusions to be presented to the full congress at Washington, U.S.A., in June 1927. The American Committee in charge of the arrangements has raised a considerable sum of money which will make it possible for European delegates to attend the conference and join the soil excursion in the United States and Canada at a cost which will be little greater than the steamer passages.

The *Proceedings* of the Prague Conference (1922) and the Rome Conference (1924) have both appeared this year. As the papers which appear in these *Proceedings* relate to work carried out more than three years ago, we shall not refer to them except to remark that they form admirable records of the principal directions along which advances in pedology are at present being made.

*Books on Pedology.*—H. Stremme has published an important work on the field-study of soils (*Grundzüge der praktischen Bodenkunde*, Borntraeger, 1926). Stremme's work gives an excellent presentation of the principles of soil classification and mapping. K. Gedroiz (*Chemische Bodenanalyse*, Borntraeger, 1926) has published a useful compendium of chemical methods used in the examination of soils. One must, however, regard such a work as largely provisional, since, owing to the rapid developments and changes in our conceptions of the soil, there are comparatively few analytical procedures which can be regarded as permanent routine methods. A similar opinion may be expressed with regard to the more ambitious work of J. Stoklasa and E. G. Doerell (*Handbuch der biophysikalische und biochemische Durchforschung des Bodens*, Parey, Berlin,

1926). This book contains some features of great interest; in particular, the sections dealing with the biological examination of soils and radioactivity of soils.

The chapters on soils in A. G. Tansley and T. F. Chipp's book on *Aims and Methods in the Study of Vegetation* (British Empire Vegetation Committee, 1926) fulfil admirably the purpose of bringing before ecologists the essential principles of the classification of soils.

*Classification of Soils.*—The somewhat inconclusive discussion on soil classification at the British Association at Oxford is typical of the confusion which exists with regard to this important question. Whilst our ideas on soil classification are generally dominated by the ideas of Glinka and the Russian school, it cannot be said that any single system has succeeded in obtaining universal approval. The problem is twofold. In the first place a world-wide classification is needed to furnish the great soil groups. In the second place it is necessary to arrive at principles by which the soils shall be classified within the great groups. With regard to the first problem, Prof. Stebbut of Belgrade has made some useful suggestions in a communication to the meeting of the Third Commission in Hungary. He suggests that, as a primary classification, the type of soil-forming process should be used, rather than the actual type itself, since, owing to topographical and other reasons, it is often the exception rather than the rule to find mature soil profiles. A region is thus to be judged by the type of soil which is found where the conditions are ideal for a mature profile.

D. Vilensky (*Proc. Int. Soc. Soil Sci.*, 1926, 1, 224) makes certain suggestions for a scheme of soil classification which are somewhat difficult to criticise until they have been more clearly presented. He proposes four great primary divisions, namely, *thermogenic*, *phytogenic*, *hydrogenic*, and *halogenic*, according to whether the dominant factor is temperature, vegetation, water-supply, or salt content respectively. Within each of these divisions he recognises types which progress to a stage of maximum expression and then exhibit successive stages of degradation. In the phytogenic division, the progressive types are desert, semi-desert, grey soils, chestnut soils, black earths. The last type represents the maximum expression of the phytogenic division and is succeeded by successive stages of degradation which end in the extreme podsol type, into which the other great divisions also degrade. The ideas of Vilensky merit consideration, but it cannot be too strongly emphasised that before any universally acceptable system of soil classification can be devised, much more information from all parts of the world must be secured. The systems

proposed up to the present are largely based on Russian and Central European data.

*Laterite.*—The formation of laterite is a problem which has been actively discussed for many years. Much of the confusion which at present exists would have been avoided if accurate profile studies of such soils had been available. Whilst laterite is considered by some to represent an eluviated or A horizon, Stremme regards laterite as an illuvial or B horizon from which the A horizon has been removed by erosion. Such an explanation would bring laterites into line with the podsoles of cool and temperate climates. A. Eichinger (*Z. Pflanz. Düng.*, 1926, 8A, 1) narrows the laterite problem down to the differential stability of negative and positive sols. As a consequence of the hydrolytic decomposition of silicates, negative sols of silicic acid and positive sols of the sesquioxides are formed. The former are more stable and can percolate through the capillary spaces of the soil without precipitation. The positive sesquioxide sols, on the other hand, are precipitated by movement through capillaries, a phenomenon well known in capillary analysis. It is also known that silicic acid sols are more stable to changes in temperature than sesquioxide sols.

H. H. Bennett (*Soil Sci.*, 1925, 21, 349) presents some interesting data relating to the chemical composition of clays from the humid regions of tropical and temperate North America. Two types of clay are distinguished, namely, the non-plastic lateritic type, in which the ratio of silica to sesquioxides is less than 2, and the plastic type, to which doubtless most of the clays of cool and temperate regions belong in which the silica : sesquioxide ratio is greater than 2.

*Soil Colloids.*—E. B. Powell (*Soil Sci.*, 1925, 19, 407), in experiments on the effect of temperature on the viscosity of suspensions of soil colloid extracted from a Putnam Silt Loam, found no evidence of colloidal swelling, and has concluded that the soil colloids are of a suspenoid character. G. J. Bouyoucos (*Soil Sci.*, 1926, 21, 481) argues that the colloidal matter of soils is not to be regarded as existing simply as a coating on the surface of non-colloidal particles. He has found that the material passing a fine sieve may contain actually less colloid than the material retained. A considerable proportion of the soil colloid must exist as an independent component. The same author (*Soil Sci.*, 1925, 19, 447 ; 20, 67) has also shown that a characteristic property of soil colloids, namely, heat of wetting, is practically absent from the separated colloid and is, thus, closely dependent on physical condition. L. Smolik (*Vest. Českoslov. Akad. Zemedelski*, 1926, 221) also finds that the colloidal property of the same soil is subject to variation. The total surface as measured by hygroscopicity or by catalase

effect is increased by leaching out of electrolytes and decreased by drying the soil at high temperatures. W. W. Pate (*Soil Sci.*, 1925, **20**, 329) has found that saturation of the soil with univalent bases decreases the heat of wetting.

*Flocculation.*—The so-called anomalous flocculation of clay suspensions by solutions of calcium hydroxide first described by Comber has given rise to considerable discussion. F. Hardy (*J. Phys. Chem.*, 1926, **30**, 254) makes an interesting contribution on the theoretical side of the question, in which he discusses separately the colloidal behaviour of hydrous alumina, hydrous ferric oxide, hydrous silica, and hydrous aluminosilicates. Hardy supposes that the hydrous sesquioxides present in laterites are ampholytic and have isoelectric points near neutrality. The theory, which may be extensible to the other types of colloidal clay, admits a reasonable explanation of the phenomena of anomalous flocculation. The evidence for the ampholytic character of aluminosilicate clays is, however, far from complete. For example, W. C. Dayhuff and D. R. Hoagland (*Soil Sci.*, 1924, **18**, 401) found clay particles to be electro-negative from pH 2.1 to 12.7.

A. F. Joseph and H. B. Oakley (*Nature*, 1926, **117**, 624) failed to obtain anomalous flocculation with highly purified clay suspensions; but W. O. Kermack and W. T. H. Williamson (*Nature*, 1926, **117**, 824) have pointed out that the clay used may have lost its gel coating, and allude to experiments in which kaolin suspensions could be made to show anomalous flocculation by sensitising them with colloidal silica.

*Soil Organic Matter.*—S. A. Waksman, in a series of papers on the origin and nature of soil organic matter or humus (*Soil Sci.*, 1926, **22**, 123, 221, 323, 395, 421), has presented an exhaustive historical and critical account of our present knowledge of the nature of humus and of its determination. He outlines a method for the determination of humus (which we must here take as equivalent to Grandeau's *matière noire*) depending upon extraction with 5 per cent. sodium hydroxide and subsequent separation into  $\alpha$ - and  $\beta$ -fractions. It is somewhat surprising to read that preliminary treatment of the soil with dilute acid is unnecessary and may actually reduce the amount of humus extracted. With regard to the organic substances giving rise to humus, Waksman suggests that lignins, being unattacked by most micro-organisms, persist and accumulate in the soil, where they form, in consequence, a considerable portion of the humus extracted by alkalis. The lignins of straw are supposed to retard the microbial decomposition of celluloses and similar substances by soil organisms. A proportion of the original carbon, perhaps 10

to 20 per cent. of that originally present, may be expected to pass into the form of microbial protoplasm, which, of course, undergoes further changes and contributes to the humus reserves. Humus, according to Waksman, is thus divisible into two categories, namely (*a*) residual lignin from added plant materials, and (*b*) microbial protoplasm and its decomposition products. The substances in the second category contain nitrogen. It will be noticed that this generalisation is made with regard to humus soluble in alkalies. There may, however, be organic matter present in the soil which, though humified, is insoluble in alkalies.

An important contribution to the field study of organic matter has been made by Hesselman (*Medd. fran Ståttens Skögsforsöksanstalt*, 1926, 22, no. 5, 169). The author has added a fairly full German summary of his work, which is likely to rank as a classical contribution to the subject. The investigation is an exceedingly detailed examination of the soil characters associated with different types of forest growth in Sweden, together with data collected from other parts of Europe. The nature of the soil organic matter and the extent to which it accumulates in forest soils depend on climate, surface topography, including drainage, and also on the character of the forest vegetation and leaf fall. Two types of decomposition may be contrasted, namely, that which occurs in the presence of an adequate supply of bases, and leads to the formation of the so-called mild humus ("mull"), and that which occurs under more acid conditions and gives rise to acid peaty humus ("torf"), which accumulates as a sharply defined layer.

Great emphasis is laid on the effect of the mineral composition of the leaf fall and on the base status of the soil in controlling the type of decomposition which takes place. With a leaf fall relatively poor in lime, such as is furnished by typical conifers, and with a soil also poor in bases, organic matter will tend to accumulate as a peaty layer, even though the conditions are aerobic. With a leaf fall rich in lime, such as that from beeches, the tendency is towards the formation of mild humus which is well distributed throughout the soil layers. The effect of the ash composition of the leaf fall on the formation of humus in forest soils is also discussed by G. Krauss (*Forstwissenschaftliche Zentr.*, 1926, 401) in an investigation of the variations of the calcium content of beech leaves in different localities.

A. Nemec (*Proc. Int. Soc. Soil Sci.*, 1926, 2, 255) has used the peroxide method of Robinson and Jones (*J. Agric. Sci.*, 1925, 15, 26) for the determination of the degree of humification of the organic matter of forest soils. He finds a good correlation between this ratio and nitrifying power.

*Soil Physics.*—The hygroscopic coefficient is a "constant" which has been so frequently used in equations devised to indicate soil-water relationships that there is a danger that it may sometimes be regarded uncritically. A. N. Puri (*J. Agric. Sci.*, 1925, **15**, 272) discusses the methods of determining hygroscopicity in which use is made of a saturated atmosphere. Results thus obtained are subject to certain inevitable errors and the employment of an atmosphere with a standard degree of saturation is to be recommended.

The effect of colloids on the evaporation of water from soils has been much discussed during recent years. B. A. Keen, E. M. Crowther, and J. R. H. Coutts (*J. Agric. Sci.*, 1926, **16**, 105) have examined the technique involved in the determination of evaporation and show that the results obtained are influenced by two groups of factors, namely, the soil-water relationships and the environmental factors. In order to secure significant and comparable results, the latter group of factors must be rigidly controlled.

The cohesiveness of soils under natural conditions is reflected in the force required to draw a cultivating implement. B. A. Keen and W. B. Haines (*J. Agric. Sci.*, 1925, **15**, 387, 395) have found that the drawbar pull shown by a dynamometer plough varies greatly over an apparently uniform field, a fact which again emphasises the prime importance of structure in controlling the physical properties of soils in the field.

In addition to the above, the following papers may be mentioned: F. Hardy, "Cohesion in Colloidal Soils," *J. Agric. Sci.*, 1925, **15**, 420; "Percolation in Colloidal Soils in Relation to Swelling and Cohesiveness," *ibid.*, 1925, **15**, 434; W. B. Haines, "Cohesion developed by Capillary Forces in an Ideal Soil," *ibid.*, 1925, **15**, 529; W. B. Haines, "On the Capillary Forces in an Ideal Soil," *ibid.*, 1926, **16**, 492; C. H. Wright, "The Relations between Certain Soil Moisture Constants and the Determination of the Vesicular Coefficient of Soils," *ibid.*, 1926, **16**, 18; M. D. Thomas and K. Harris, "The Moisture Equivalent of Soils," *Soil Sci.*, 1926, **21**, 411.

*Soil Acidity and Base Exchange.*—In April 1926 a meeting of the Second Commission (Chemical Soil Analysis) of the International Society of Soil Science was held at Groningen, Holland. The subject of the conference was Soil Acidity and Base Exchange. The collected papers are published as the *Transactions of the Second Commission* (Hoitsema, Groningen, 1926). The contributions group themselves around a few main aspects of the general problem. The determination of the pH of soils is dealt with by Ch. Brioux and J. Pien and by D. J. Hissink and J. van der Spek. The quinhydrone electrode appears to be generally desirable for soil work, and it was

pointed out by E. Bijlman during the discussion that the platinum electrode should be actually in the soil and not partly in the soil and partly in the supernatant liquid. Certain details of technique remain to be worked out by co-operative effort.

The principal question, namely, the general nature of soil acidity, formed the subject of a keen discussion. There are two views as to the nature of soil acidity. According to one view, the soil contains inorganic and organic constituents which have the character of weak acids or, using the convenient term of Michaelis, *acidoids*. They are only dissociated at their surfaces and the anions produced are colloidal. These weak acids are present partly as such and partly as salts, principally calcium salts. The acidity of the soil depends on the extent to which the acidoid constituents are unsaturated with bases. Owing to the weak acid character of these substances, complete saturation is never attained under natural conditions. Hissink's work suggests that even in soils which have attained equilibrium with excess of calcium carbonate, the degree of saturation is never more than about 55 per cent.

According to the other view, there are four stages of soil acidity. In its first stage it appears as hydrolytic acidity, the acidity developed by reaction of a soil with solutions of salts of weak acids, due to adsorption of hydroxyl ions. With a higher degree of base unsaturation, exchange acidity may develop. This is manifested when a soil reacts with the solution of a neutral salt and is due to the liberation of aluminium ions. With still greater acidity, the stage of neutral salt decomposition is reached, in which the acidity developed with a neutral salt solution exceeds that attributable to aluminium ions. Finally, there is active acidity in which the soil contains free mineral acids or is so unsaturated with bases as to give titratable acidity in an aqueous extract. The two views are not diametrically opposed, since they both regard acidity in terms of base unsaturation. The second view, however, which is widely accepted in Central Europe, appears to introduce unnecessary distinctions which only exist in the laboratory.

Since the Groningen meeting, H. Kappen and J. Breidenfeld (*Z. Pflanz. Düng.*, 1926, 7A, 174) have published some results on the acid effect of silica and certain silicates which should receive careful consideration. Although with pure silica no appreciable titratable acidity was developed by reaction with a neutral salt, this type of acidity was induced to a marked degree when silica was first treated with barium hydroxide, and the resultant product, after removal of excess of barium hydroxide, treated with aluminium chloride or ferrous chloride.

Where ferric chloride was used, no appreciable exchange acidity was conferred, a result attributed to the greater decomposition of the barium silicate in this case by the hydrochloric acid produced by hydrolysis. These results and those of other experiments along similar lines are adduced in support of the view that exchange acidity is primarily due to the liberation of aluminium ions. The views of Ganssen as to the molecular ratios of the zeolitic silicates of acid soils are supported by analyses of the complex silicates produced.

Of recent years much attention has been given to the problem of aluminium in physiological soil acidity. J. Line (*J. Agric. Sci.*, 1926, **16**, 335) shows that, even in very acid soils, aluminium ions can scarcely be present in appreciable concentration, although 0.001 to 0.006 per cent. of aluminium may be present as hydrosols. The similar conclusions of Magistad (*Soil. Sci.*, 1926, **20**, 181), namely, that aluminium is not present in soluble form between pH 4.7 and 8.5, are criticised by Hardy (*J. Agric. Sci.*, 1926, **16**, 616) on the ground that in the experiments which formed the basis of such conclusions, no account was taken of the effect of dialysis on aluminium sols.

## ARTICLES

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### RECENT DEVELOPMENTS IN MAGNETISM

By EDMUND C. STONER, Ph.D. (Cambridge)  
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DURING the last few years remarkable progress has been made in the understanding of magnetic phenomena. The significance of the recent advances, however, cannot be fully appreciated unless they are considered in relation to the historical background. A brief survey of this will therefore be given here first before proceeding to the discussion of special topics.

#### GENERAL SURVEY

From early ages there have been men who wondered at the properties of the magnet stone, and speculated as to their origin; and fantastic legends of massive statues suspended in mid-air and of magnetic mountains which drew the iron nails from passing ships have come down from classical antiquity. Peregrinus of Maricourt, a "master of experiment," was probably the first investigator who consciously applied the experimental method to magnetism, and in a famous letter, written in 1269, possibly to while away time during the siege of Lucca, he describes his results. He used a globe of lodestone, and introduced the term "poles" for the regions in which the magnetic power was concentrated. He found that unlike poles attracted, like repelled each other; and that parts of a broken magnet behaved like complete magnets. Little was added to Peregrinus's discoveries for some two hundred years. In 1600 Gilbert of Colchester, Court Physician to Queen Elizabeth, published *De Magnete*, which gave an account of his investigations on electricity and magnetism over a period of many years. Gilbert, the "Galileo of Magnetism," must certainly be numbered among the greatest of experimental investigators. His book is written in a vivid and fascinating manner. Gilbert does not hide his scorn of "hearsay" scientists, and he refutes idle speculations by appeal to experiment; but he recognises to the full previous sincere and honest work. Gilbert extended Peregrinus's observations; distinguished clearly between magnetic and electric forces; examined the effects of heat on

iron; and showed that the earth itself behaves as a great magnet.

With the work of Mitchell (1724-93) and Coulomb (1736-1806), who showed that the force between poles varied as the inverse square of the distance between them, magnetism passed to the quantitative stage; and the mathematical theory was developed very completely by Poisson (1820), whose treatment requires little modification—as far as macroscopic phenomena are concerned—even at the present time. Poisson did, indeed, assume the existence of two magnetic fluids, which could be separated to opposite ends of a molecule, but his results are independent of this particular hypothesis.

A year earlier Oersted had made the fundamental discovery which linked together magnetism and electricity, that a current-bearing wire is surrounded by a magnetic field. Ampère carried out further experiments, and on the basis of these he developed the mathematical theory in a memoir (1825) “perfect in form and unassailable in accuracy.” He obtained expressions for the forces between current elements, and he showed that in its magnetic effects a current circuit was equivalent to a magnetic shell whose boundary coincided with the circuit. His speculative suggestion that molecular currents might give rise to permanent molecular magnets is worthy of note. It is unnecessary to dwell on Faraday’s work on electromagnetic induction, or on the development of the electromagnetic theory by Maxwell. In 1845 Faraday discovered that all substances were weakly magnetic, and that they could be divided into paramagnetics and diamagnetics. Paramagnetics are attracted by a magnet, and in general tend to set themselves along the lines of force; diamagnetics are repelled, and tend to set themselves at right angles to the lines of force. Ferromagnetics, of which iron, nickel, and cobalt and some of their compounds are the outstanding examples, behave like strong paramagnetics, but they have other peculiarities which will be referred to later. In a comprehensive research, Curie, in 1895, measured the susceptibilities of a large number of substances over a wide range of temperatures. For most diamagnetics the mass susceptibility  $\chi$  (the magnetic moment per unit mass, divided by the field) was found to be practically independent of the temperature, while many paramagnetics obeyed approximately Curie’s law—

$$\chi = \frac{C}{T} \quad (1)$$

the susceptibility being proportional to the absolute temperature.

The ground was now prepared for a theoretical treatment which should explain the main features of the magnetic pro-

perties of substances. This was supplied by Langevin in a classical paper "Magnétisme et théorie des électrons" (1905). The electron had been discovered, and Langevin's theory applies essentially to a monatomic gas, in which, as far as their magnetic properties are concerned, the molecules consist of electrons rotating in orbits about positive centres of force. The picture was very general, and the treatment is applicable, in its main lines, to atoms of a structure suggested by later developments in atomic theory.

A single electron rotating in an orbit may be regarded as constituting a current circuit, and at points not too close it will give rise to a mean magnetic field similar to that produced by the equivalent magnet. (It should be noted that to ensure stability the electron must be assumed not to lose energy by radiation, so that its properties cannot be identical with those of the "theoretical" Lorentz electron.) Let  $\mu_e$  be the magnetic moment corresponding to the orbit of the electron

$$\mu_e = \frac{eS}{c\tau} \quad (2)$$

where  $S$  is the orbital area, and  $\tau$  the periodic time.

If  $r$  is the radius of the orbit and  $v$  the velocity at right angles to  $r$ , remembering that  $rv$  is constant

$$\mu_e = \frac{e}{2c}rv = \frac{e}{2mc}j \quad (2')$$

where  $j$  is the angular momentum.

In an atomic system the resultant moment will be the (vectorial) sum of the moments of the electronic orbits; if these balance out, the resultant will be zero; when the resultant differs from zero, the system will be paramagnetic. In either case an external magnetic field will modify the orbital motion in such a way as to produce a diamagnetic effect; but when there is a resultant magnetic moment, changes in orientation will occur, producing a paramagnetic effect which will, in general, mask the initial diamagnetism.

The variation in the magnetic flux through an orbit will produce a change in the electric force round it. It has been shown by Larmor that the effect of a magnetic field  $H$  is equivalent, as a first approximation, to an increase  $o$  in the angular velocity of the electron given by

$$o = -\frac{eH}{2mc} \quad (3)$$

Let  $\bar{r}^2$  be the mean square radius of the orbit of an electron projected on a plane at right angles to the field. For an atom with  $N$  electronic orbits, and initial resultant moment zero,

the moment induced by the field  $H$  may be readily calculated from (2) and (3); the expression obtained for the atomic diamagnetic susceptibility is

$$\chi_{At} = -\frac{e^2}{4mc^2} \sum_N \overline{r^2} \quad (4)$$

This suggests that there should be no variation in susceptibility with temperature; and it gives a method of estimating

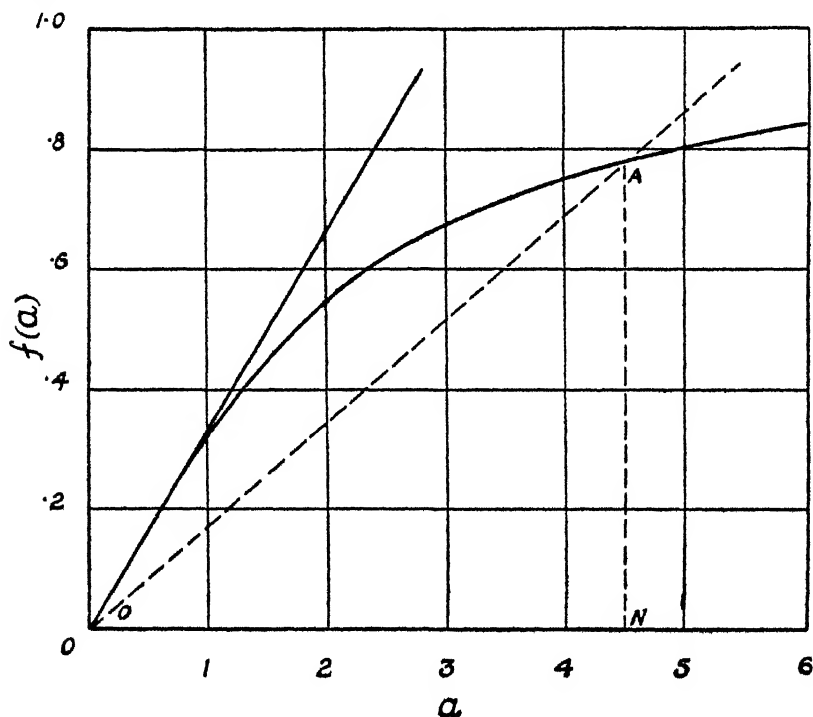


FIG. 1.—For the curve  $f(a) = \coth a - \frac{1}{a}$   
For the tangent  $f(a) = \frac{a}{3}$

atomic and molecular dimensions which, as will be seen, leads to reasonable results.

Atoms or molecules with a resultant moment will tend to turn with their axes in the direction of an applied field. In the absence of a field (or in a very small field) Langevin assumes that any orientation is equally probable; as the field is increased the tendency to orientate in the field direction is opposed by the temperature movements (in particular, the rotations); for a given field and temperature an equilibrium state is reached. Let  $\mu$  be the magnetic moment of the molecule,  $\theta$  the angle the magnetic axis makes with the field,  $n$  the number of molecules

in the mass considered,  $M$  the resultant magnetic moment. Langevin shows that

$$\frac{M}{n\mu} = \frac{\bar{\mu}}{\mu} = \coth a - \frac{1}{a} \quad (5)$$

where  $a = \frac{\mu H}{kT}$ ,  $k$  being Boltzman's constant.  $\mu$  is the mean resultant molecular magnetic moment in the field direction. The saturation moment,  $n\mu$ , corresponding to complete alignment, could only be obtained for  $H/T$  very large. The function  $(\coth a - \frac{1}{a})$  is plotted against  $a$  in Fig. 1.

When  $a$  is small, (5) reduces to

$$\frac{\bar{\mu}}{\mu} = \frac{a}{3} = \frac{\mu H}{3kT} \quad (5')$$

The molecular susceptibility is then given by

$$\chi = \frac{\bar{\mu}}{H} = \frac{\mu^2}{3kT} \quad (6)$$

and the gram molecular susceptibility, using  $n'$  for Avogadro's number, by

$$\chi_M = \frac{(n'\mu)^2}{3n'kT} = \frac{\mu_M^2}{3RT} \quad (6')$$

If very large fields and low temperatures could be employed, it would be possible to deduce the molecular moments from the intensity of magnetisation at saturation; but in practice, it is generally only possible to investigate the magnetisation over a range of  $a$  values corresponding to the initial part of the Langevin curve. The susceptibility is then independent of the field, and according to (6) inversely proportional to the temperature. (Gadolinium sulphate has been investigated at temperatures as low as  $1.3^\circ$  Abs. Values of  $a$  approaching 8 could be obtained, and the results were in agreement with the complete Langevin expression (5).) The great value of Langevin's theory lies in the fact that molecular moments can be deduced from ordinary susceptibility measurements. For paramagnetics which obey Curie's law (1), from (6')

$$C_M = \frac{\mu_M^2}{3R} \text{ or } \mu_M = \sqrt{3RC_M} \quad (7)$$

where  $C_M$  is the Curie constant per gram molecule.

Many solid paramagnetics do not behave in accordance with Curie's law, but over considerable ranges of temperature closely obey the relation

$$\chi = \frac{C}{T - \theta} \quad (8)$$

Langevin had assumed that the magnetic molecules exerted no mutual influence on each other due to their orientation; Weiss extended the theory by supposing that orientation of the molecules gave rise to an additional effective field proportional to the intensity of magnetisation, so that if  $H$  is the total effective field,  $H_e$  the external field :

$$H = H_e + H_i = H_e + NI \quad (9)$$

where  $N$  is a constant. If this additional field were purely magnetic,  $N$  should be approximately equal to  $\frac{4\pi}{3}$ , but as it is frequently negative, and its numerical value may be very great (of the order of 10,000 for nickel, for example), the molecular field  $NI$  cannot be due to mutual magnetic action of the molecules. The assumption involved in (9) leads to the expression (8) for the susceptibility,  $\theta$  having the value

$$\theta = \frac{\mu_M^2 N \rho}{3mR} = \frac{N C_M}{m} \quad (10)$$

where  $\rho$  is the density,  $m$  the molecular weight.

Although the physical nature of the Weiss molecular field is obscure, many properties of magnetic substances may be satisfactorily correlated by assuming the existence of such a field, proportional to the magnetisation and equivalent, in its magnetic effects, to an actual magnetic field. Thus the general law (8) for paramagnetics is obtained, this applying to ferromagnetics above the Curie temperature  $\theta$ . Below the Curie temperature there may be "spontaneous" magnetisation in the absence of an external field. This possibility arises when

$$\frac{\bar{\mu}_M}{\mu_M} < \frac{a}{3} \quad (\text{see the dotted line in Fig. 1}).$$

Now  $a = \mu_M H / 3RT$  and for  $H = NI = N \frac{\bar{\mu}_M \rho}{m}$

this gives, as the condition for spontaneous magnetisation,

$$T < \frac{\mu_M^2 N \rho}{3mR} \quad (11)$$

$$\text{or } T < \theta$$

Since a bar of iron, say, may ordinarily be unmagnetised, Weiss supposes that the spontaneous unidirectional magnetisation appropriate to the temperature (given by the point A in Fig. 1, where the line of slope  $\frac{a}{3} \cdot \frac{T}{\theta}$  cuts the Langevin curve) only occurs throughout small domains (which may or may not be

identified with the individual microcrystals). These domains may have their directions of magnetisation orientated at random, so that the resultant magnetisation of the whole bar may be zero. The application of a field results first in the reversal of the direction of magnetisation where its resolved component is opposed to the field, and then in the gradual alignment of all the elementary magnets in the field direction. Weiss has found considerable evidence for the general theory from experiments on pyrrhotite, and on nickel, but there are many difficulties as to details.

In 1911 Weiss concluded that magnetic moments were all integral multiples of a fundamental unit, having a value  $1.85 \times 10^{-21}$  erg/gauss (corresponding to 1,123.5 erg/gauss per gram molecule). Magnetic moments are usually expressed as multiples  $p$  of this unit Weiss magneton. The hypothesis has certainly stimulated research, and the empirical unit is of convenient magnitude; but later work has shown that  $p$  is frequently non-integral, and it now seems highly improbable that the Weiss magneton has any fundamental significance.

The application of the quantum theory to the problem of atomic structure by Bohr (1913) has revolutionised the outlook on the question of elementary magnetic moments. An essential feature of the theory is that the angular momentum of an electron rotating in an orbit can only assume certain discrete values defined by the azimuthal quantum number  $k$ , so that, from (2')

$$\mu_e = \frac{e}{2mc} j = \frac{e}{2mc} k \frac{h}{2\pi} \quad (12)$$

There is thus a theoretical unit magnetic moment associated with an orbital electron equal to

$$\mu_1 = \frac{eh}{4\pi mc} = 9.23 \times 10^{-21} \quad (12')$$

or, per gram molecule

$$M_1 = 5,593 \quad (12'')$$

This unit is approximately five times the Weiss empirical unit.

The study of the splitting of spectral lines in the Zeeman effect has led to the further conclusion that the resolved magnetic moment of an atom in the field direction can also only assume certain discrete values—that only certain definite orientations of the atom are possible. Moreover, the possible resolved magnetic moments of an atom (known as the  $mg$  values,  $mg$  giving the resolved moment in terms of the Bohr unit) can be deduced for any state of the atom for which the values of the azimuthal quantum number  $k$  (or for atoms in

which a group of electrons are involved in the production of the lines, the corresponding group quantum number  $l$ ) and the inner quantum number  $j$  (not to be confused with the  $j$  used above for the angular momentum) are known.

In Langevin's theory it was assumed that in weak fields any orientation of the atom or molecules was possible. If  $\theta$  is the angle between the magnetic axis and the field, using a bar to denote a spatial mean value, for weak fields

$$\frac{\bar{\mu}}{\mu} = \frac{\mu H}{kT} \overline{\cos^2 \theta} \quad (13)$$

If any value for  $\theta$  is equally probable,  $\overline{\cos^2 \theta} = \frac{1}{3}$  and (5') is obtained; in the quantum case, however, there are only certain discrete orientations, possible values for  $\frac{\mu \cos \theta}{\mu_1}$  being given by the possible values of  $mg$  ( $m$  assumes values symmetrical about 0, all integral or half integral, and differing by 1;  $g$  is the Landé splitting factor). The magnetic moment as calculated from (6) or (7) will then be incorrect; but it may be shown that the value calculated for  $p$  from the measurements should be given by

$$p = 5g\sqrt{j(j+1)} \quad (14)$$

If the normal spectroscopic state (the "ground" state) of an ion is known, the  $p$  value, which measurements on the ionic susceptibility may be expected to yield, may be calculated from (14).

Some of the more recent work may now be briefly considered in the light of the general outlook. Passing from the simpler to the more complex, atoms and ions, molecules, and crystals will be taken in turn, and some special problems and difficulties will then be discussed.

## ATOMS AND IONS

*Atomic States and Spectral Terms.*—The spectral state of an atom or ion (a mononuclear system) may be conveniently specified by a symbol of the type  $^rL_j$ , where  $r$  indicates the multiplicity (1, 2, 3 . . . for singlet, doublet, triplet . . . terms),  $L$  the term type (S, P, D . . .), and  $j$  the inner quantum number. With  $L$  may be associated the quantum number  $l$  ( $\frac{1}{2}$ ,  $\frac{3}{2}$ ,  $\frac{5}{2}$  . . . for S, P, D . . . terms); for one valence electron systems,  $l$  may be identified with the azimuthal quantum number  $k$  for this electron, but in the systematisation  $k$  is assigned a value smaller by  $\frac{1}{2}$  than in the original Bohr scheme. Landé has given a general formula in which the splitting factor

$g$  is expressed as a function of  $r$ ,  $l$ , and  $j$ . Thus if the 'L' state of an atom is known, the corresponding Weiss magneton number  $p$  to be anticipated from susceptibility measurements may be calculated from (14); moreover, the actual possible resolved magnetic moments  $mg$  are known. The scheme, it must be emphasised, is simply a convenient empirical method of correlating the observations on the multiplicity of lines and on their splitting under the influence of a magnetic field in the Zeeman effect. There is at present no completely satisfactory physical interpretation of the scheme in terms of a "model" atom.

*Magnetic Deflection of Atomic Rays.*—The most direct method of investigating the magnetic moments of atoms is that employed in the now classical experiments of Gerlach and Stern. The element under investigation was heated in an oven (see Fig. 2), and the issuing stream of atoms, delimited by slits

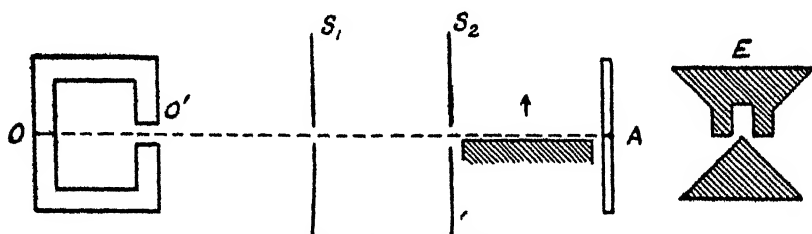


FIG. 2.—Diagram of Gerlach and Stern apparatus.

$S_1$  and  $S_2$ , passed through an inhomogeneous magnetic field (obtained by wedge-slit pole pieces, shown in cross-section on the right of the figure), and was received on a plate A.

The whole apparatus was in an enclosure in which a high vacuum could be maintained. To give an idea of dimensions it may be mentioned that the receiving glass plate A was some 3 mm. square. In the absence of a field a trace consisting of a single line was obtained. In general this line had to be "developed" by immersion in a hydroquinone silver nitrate solution; the deposition of silver being necessary to render the trace visible.

When the magnetic field is applied, the atoms will be deflected, the deflection depending on the resolved magnetic moment,  $\mu$ , the velocity of the atoms (so on the oven temperature  $T$ ), the inhomogeneity  $\frac{\partial H}{\partial s}$  and the length of the magnetic field.

If  $s$  is the deflection, it may be shown that, with A as an apparatus constant,

$$A \frac{\mu}{T} \left( \frac{\partial H}{\partial s} \right) \quad (15)$$

$\mu$  may be conveniently expressed as a multiple of the Bohr magneton. If  $\mu_0$ , the atomic magnetic moment, makes an angle  $\theta$  with the field,

$$\mu = \mu_0 \cos \theta \quad (16)$$

The results to be anticipated on the classical and quantum theories for atoms with unit magnetic moment may now be considered. On the classical theory any orientation of the atoms in the field is possible, so from (16) and (17) the single line trace should broaden out into a band when a field is applied. On the quantum theory, for an atom such as silver or sodium in the normal  $^2S$  state there are only two possible settings (both with the plane of the valence electron orbit at right angles to the field, but with the electron rotating in opposite directions); when the field is applied the trace should split up into two, and there should be no undeflected rays.

The experiment was tried with silver. At first the results were inconclusive, but eventually the apparatus was so refined that not only was the splitting clearly shown, but quantitative measurements could be made. The experimental magnetic moment of the silver atom was found to be equal, within the limits of error, to one Bohr magneton. A much enlarged schematic representation of the silver trace is shown in Fig. 3. The attracted beam is unsuitable for measurements, owing to the strong and varying inhomogeneity of the field near the wedge pole piece. The deviation is most conveniently obtained from the repelled beam, being given by the distance  $b$ , a mean of  $a$  and  $c$ .



FIG. 3.  
Diagram of  
Silver Trace in  
the Magnetic  
Deviation  
Experiments.

For copper and gold similar results were obtained. For these, as for silver,  $\mu = \pm 1$  (in Bohr units) in agreement with the fact that the spectroscopic ground term is  $^2S$  (corresponding to  $mg$  values of  $\pm 1$ ).

Zinc, cadmium, and mercury, and also tin and lead, have closed configurations—that is, the sub-groups in the atoms contain their full complement of electrons—and a  $^1S_0$  spectroscopic ground state. Correspondingly the atomic rays were found to be undeflected.

For thallium (normal state  $^2P_1$ , for which  $mg = \pm \frac{1}{2}$ ) there was a small deflection, and no undeviated trace. With bismuth and antimony the results were difficult to interpret, probably owing to the presence of molecular rays. For nickel a multiple trace was obtained, some of the rays being undeviated. This is not in disaccord with the spectroscopic evidence ( $^3F$  ground term), but further experiments are needed.

The Gerlach and Stern method has recently been applied by Taylor to the alkali metals sodium and potassium, the traces being developed by immersing the plates in hydrochloric gas, so as to produce an opaque deposit of the chloride. The atoms have one valence electron moving in a  $k = 1$  orbit (besides those in the closed inert gas-like configuration) and were found to have a moment of one Bohr magneton.

The hydrogen atom is of particular interest owing to its very simple structure and to the fact that the spectrum has now been shown (by Slater, and Sommerfeld and Unsöld) to be completely analogous to that of the alkali metal atoms. Phipps and Taylor have given a preliminary account of magnetic deviation experiments with hydrogen. The rays were received on a plate smoked with molybdenum oxide, which was reduced by atomic hydrogen. Two deviated traces were obtained, corresponding to about one Bohr magneton. (There was also a less pronounced undeviated trace possibly of molecular origin.) The hydrogen atom in its normal (1, 1) state would seem then to behave exactly as the original Bohr theory would predict.

In general, the magnetic deviation experiments confirm the quantum spectroscopic theory; they show, in particular, that the  $mg$  values deduced from the Zeeman effect do really give the actual resolved magnetic moments of the atoms. They form, perhaps, the most striking direct experimental confirmation of a quantum conception of atomic structure.

*Inert Gas Atoms.*—The inert gases are practically the only substances consisting of atoms in a free state whose susceptibility can be conveniently measured. The accurate measurement of the small volume susceptibilities presents a difficult problem, which, however, has been satisfactorily solved in a neat balance method devised by Wills and Hector. A modified Quincke U-tube method is employed, with a weak solution of  $\text{NiCl}_2$  as the liquid; a balance is obtained by varying the temperature, or the pressure of the gas.

Neon and argon have closed configurations (having 2 and 6 electrons in the outer  $k = 1$  and  $k = 2$  Bohr subgroups) with a  $^1S_0$  ground state. As anticipated, they are diamagnetic, and their susceptibility leads to a value for the mean square radius of the outer electronic orbits (on the basis of Eq. 4) which is in rough agreement with other evidence. Helium is also diamagnetic, but the susceptibility is rather high—for the gram atomic susceptibility  $\chi_A = -1.7 \times 10^{-6}$ . From the spectroscopic data, assuming both electrons to move in circular orbits, and making the most favourable assumptions as to orientation, a value of the order  $-1.3 \times 10^{-6}$  is obtained; while a model in which the electrons move in opposite directions in elliptical orbits for which  $k = \frac{1}{2}$  gives a value much too small, about

$-3 \times 10^{-4}$ . Models are not to be taken too seriously, but they serve to show how far different results can be correlated. For helium, the experimental results may be somewhat inaccurate, and theory is still very indefinite.

*Ions.*—There is a large class of typical polar compounds which may most simply be regarded as aggregates of more or less separate ions. The susceptibility of a solution of sodium chloride, for example, will depend on the magnetic properties of the  $\text{Na}^+$  and  $\text{Cl}^-$  ions; while in a crystal of salt, the ions will occupy the points of the lattice. It is found that solutions and solid salts in which the ions possess closed configurations are diamagnetic, and the same considerations hold for them as have already been advanced generally and applied to inert gas atoms. Of more particular interest are those ions in which the electron groups and subgroups are incomplete, as in the transition series of elements. It is these which display paramagnetic properties, the significance of which was so beautifully revealed by Bohr's theory of the periodic system. It will be convenient to consider the ions of the first transition series of elements, potassium to copper. The ionic susceptibility may be determined from measurements on solutions—for weak solutions the Curie law (1) holds, or on solid salts, for which the Weiss law (8) usually holds over considerable ranges of temperature.

The fact that the Weiss law holds for many solids is very surprising, for on the basis of the Langevin theory, or its quantum modification, it necessarily seems to involve that the ions, or the electronic orbits responsible for their magnetic properties, are free to change their orientation even in a solid.

When the ionic susceptibility has been found over a range of temperatures, the magnetic moment of the ion expressed as a multiple  $p$  of the Weiss empirical magneton can be readily deduced. Some results are given in Table I, the  $p$  values being given to the nearest half integer, for, although individual determinations may be very accurate, there is disagreement in separate experiments to an amount of the order of 1 unit. The right-hand index indicates the positive charge on the ion.

TABLE I  
IONIC MOMENTS

Number of Electrons in Ion .	18	19	20	21	22	23	24	25	26	27	28
Examples .	$\text{K}^+\text{Ca}^2$	$\text{Ti}^3$	—	$\text{Cr}^3\text{Mn}^4$	$\text{Cr}^2\text{Mn}^3$	$\text{Mn}^2\text{Fe}^3$	$\text{Fe}^3$	$\text{Co}^3$	$\text{Ni}^3$	$\text{Cu}^3$	$\text{Cu}^1\text{Zn}^2$
$p$ .	0	8.5	—	19	24	29.5	26	24.5	16	9	0

Now in the series of ions here considered (3, 3), electrons (constituting the M IV and M V subgroups) are being added to the completed argon configuration of 18 electrons. Ten electrons are required to complete the M group, and the ions with 28 electrons are diamagnetic like those with 18. For the ions in between, the possible spectroscopic states may be deduced on the basis of the Heisenberg-Hund system from the number of "unbalanced"  $k = 3$  (or  $k = \frac{5}{2}$ ) electrons, and the probable ground state, which should give the normal state of the ion, may be predicted. In general it may be said that the magnetic properties are not in accordance with the spectroscopic predictions. At present there is no satisfactory explanation of the discrepancies. The run of  $p$  values is actually fairly close to what should be expected if all the ions were in S states, as the following values show. (Actually, the theory predicts that some of the ions should be in P, D and F states.)

TABLE II  
QUANTUM MAGNETON NUMBERS

Spectroscopic State .	<sup>1</sup> S	<sup>3</sup> S	<sup>5</sup> S	<sup>4</sup> S	<sup>6</sup> S	<sup>8</sup> S
$p$ calc. . . . .	0	8.6	14.1	19.3	25.4	29.5

The agreement even with this purely empirical scheme, however, is very imperfect, especially in the case of Co<sup>3</sup>.

For the rare earths the spectroscopically predicted and the observed magnetic moments are in good agreement (Hund).

While, then, the order of magnitude of the ionic moments is such as the quantum theory would suggest, there are at least three outstanding problems. It is very difficult to account for the differences found in the moments of ions from measurements on different salts (or even on the same salt). The apparent freedom of the ions in solids to reorientate at present has received no adequate explanation. It is probably connected with the wider problem of the nature of the Weiss molecular field. Finally, there is the lack of correlation between the spectroscopic and magnetic properties of the ions.

## MOLECULES

Typical non-polar molecules are of particular interest as being "self-contained" electronic systems intermediate in degree of complexity between mono-nuclear systems (atoms and ions) and the "giant-molecules" constituting crystals. Here only the simpler molecules will be considered. From a magnetic point of view, many problems are raised both by the

diamagnetic and paramagnetic configurations. Most molecules contain an even number of electrons and are diamagnetic. Paramagnetic oxygen (with 16 electrons) is exceptional. The paramagnetism of "odd molecules" ( $\text{NO}$ ,  $\text{ClO}_2$ , and  $\text{NO}_2$ ) may be attributed to their possessing an unbalanced electronic orbit.

The sequence of molecules  $\text{N}_2$ ,  $\text{NO}$ , and  $\text{O}_2$  (with 14, 15, and 16 electrons) will serve to illustrate the correlation possible between the magnetic, chemical, and spectroscopic data.  $\text{N}_2$  is diamagnetic,  $\text{NO}$  and  $\text{O}_2$  paramagnetic. The Weiss magneton values for  $\text{NO}$  and  $\text{O}_2$  are 9.20 and 14.12 respectively (Bauer and Piccard), in fair agreement with the values calculated on the quantum theory for  $^2\text{S}$  and  $^3\text{S}$  systems (Table II), while the zero moment of  $\text{N}_2$  corresponds to a  $^1\text{S}$  system.

Magnetically the three molecules behave like *atomic* systems in which 0, 1, and 2 electrons move in  $k = 1$  orbits outside a closed configuration. The chemical data may be conveniently summarised by reference to Langmuir's octet models. (Dynamically, an octet may be taken to represent an inert gas-like configuration of the Bohr type.) In Langmuir's scheme  $\text{N}_2$  was regarded as a single octet, within which was a pair of electrons in addition to the two K pairs. Evidence in support of this model is provided by the chemical inertness and the difficulty of dissociation.  $\text{NO}$  has 1 additional external electron, but in  $\text{O}_2$  it was supposed that two octets were joined face to face. A dynamical analogue of this static model is difficult to imagine, and, indeed, it seems more in harmony with the chemical data to regard  $\text{O}_2$  as a single octet with 2 external electrons, treating  $\text{O}_2$  as a "divalent molecule" just as  $\text{NO}$  is monovalent.

The analysis of molecular band spectra shows that the energy levels are of three types, electronic, rotational, and vibrational. If the view adopted above is correct, it suggests that in the  $\text{N}_2$  molecule, the two N nuclei each retain a pair of K electrons (which accompany the nuclei in their rotational and vibrational movements) and that the remaining 10 electrons form a closed configuration, consisting of 2 molecular K and 8 molecular L electrons. The extra electron in  $\text{NO}$  is analogous to the valence electron in an alkali metal atom, and the two in  $\text{O}_2$  to the two valence electron in alkaline earth atoms (when these are in a  $^3\text{S}$  state). It would seem that the electronic configuration common to the whole molecule can take up orientations in a magnetic field (just as an atom or ion) independently of the rotational or vibrational state of the nuclei. The nuclei with their associated K electrons act as a common centre for all the outer electrons. With this outlook there is a far-reaching coherence between the chemical and magnetic properties of the molecules.

Difficulties are encountered when the attempt is made to explain the structure of the band spectra on the assumption that ten outer electrons are required to form a closed inert gas-like configuration. According to Mulliken and Birge, the multiplicity of the electronic levels of the molecules of the type here considered (as examples CN, BO ;  $N_2$ , CO ; NO,  $O_2^+$ ) can be satisfactorily accounted for by assuming that eight outer electrons are required.  $N_2$  will then have two valence electrons, and will be analogous to an alkaline earth atom in a  $^1S$  state, while NO and  $O_2$  would be expected normally to be in  $^2P$  and  $^3P$  states. On this basis, the magnetic properties of NO and  $O_2$  cannot be satisfactorily explained. (The difficulties are similar to those found for ions.) Until the band spectra are more completely analysed, and the different types of multiplicity clearly differentiated, the question must be left open. It may, however, be noted incidentally that pairs of atoms such as CC, BN (with eight electrons in addition to the K pairs), do not form self-contained molecular systems of the inert gas-atom type as would be expected on the Mulliken-Birge views, but the atoms aggregate into crystals of a tetrahedral type, as emphasised by Grimm and Sommerfeld.

*The Glaser Anomaly.*—Some peculiar results obtained by Glaser in the measurements on the susceptibility of a number of diamagnetic gases at low pressures have recently given rise to a great deal of discussion.  $H_2$ ,  $N_2$ , and  $CO_2$  were studied, and it was found that the relation between the volume susceptibility and the pressure was linear at the higher and lower pressures, but that at the lower pressures the slope of the curve was three times as great, apparently indicating a threefold increase in the molecular susceptibility. This was attributed to a change from random orientation of the molecules to a definite quantum orientation with a corresponding increase in the resolved areas of the electronic orbits (see Eq. 4). The ordinary theory, however, would indicate an increase of only  $\frac{2}{3}$ , and further the enhanced susceptibilities lead to values for the orbital radii which are far too large to be compatible with other evidence. Moreover, since diamagnetic molecules may be regarded as  $^1S$  systems, and it may be supposed that they take up orientations in a field just as do  $^1S$  atomic systems, it would be expected that there would be a breakdown in the spatial quantisation of paramagnetic systems such as  $O_2$  at sufficiently high pressures. This has not been observed.

The fundamental interest of the problem raised by Glaser's work led to a repetition of the susceptibility measurements, for  $H_2$  and  $CO_2$ , by a different method, by Lehrer. Down to the lowest pressures no anomaly was found. Recently Hammar has carried out further work, using virtually the same method

as Glaser, and discovered no magnetic peculiarity, but with imperfectly dried gases he obtained curves somewhat similar to those of Glaser. The Glaser anomaly, therefore, though it may exist, has its origin in secondary experimental effects, and does not provide another unsolved problem in magnetism.

#### CRYSTALS—FERROMAGNETICS

The susceptibility of paramagnetic salts at low temperatures frequently does not obey Weiss's law (8), there being departures of various types from linearity in the  $\frac{I}{\chi}$ , T curves. The work of Jackson has shown that these "cryomagnetic anomalies" still exist in the susceptibilities measured along the principal axes of individual crystals (*e.g.* hydrated nickel sulphate and cobalt ammonium sulphate). In general, for the different axes, from the linear part of the curves the Curie coefficient is approximately constant, but the Weiss coefficient varies. The anomalies are to be attributed to the joint action of some intrinsic crystalline field and a Weiss molecular field, but so far no simple relations have been found, and there is no satisfactory quantitative theory.

X-ray analysis has shown that practically all solids consist of aggregates of large or small crystals, so that the measured physical constants for ordinary solids are mean values for the different directions in the individual crystals. In the case of ferromagnetics it would certainly be expected that the study of single crystals would reveal simple relationships which would serve to test Weiss's theory, or, at least, to indicate how it might be improved. Before considering the single crystal work, however, the question of the atomic moments of ferromagnetics will be briefly discussed.

*Atomic Moments of Ferromagnetics.*—The special characteristic of ferromagnetics is that they attain saturation (appropriate to the temperature) in readily realisable magnetic fields (according to the theory, owing to the high value of the Weiss molecular field). The mean atomic moment may therefore be deduced from the saturation intensity of magnetisation over a range of temperatures, for at low temperatures the intensity corresponds to complete alignment of the elementary magnets. An estimate of the atomic moment may also be obtained from measurements of the susceptibility above the Curie temperature (using Eqs. 7 and 8), when ferromagnetics behave like ordinary paramagnetics. The moments deduced by the two methods, however, are not in agreement. Thus the magneton value deduced for nickel from saturation measurements is about 3, that deduced from measurements above the Curie temperature

about 8. According to Weiss, this is due to a change in the relative numbers of different magnetic carriers.

A simpler interpretation seems possible. In the first place it is unnecessary to assume that all the nickel atoms are in the same state. Consideration of the properties of alloys suggests that there are groups of atoms in which, by interchange and sharing of electrons, there may be ions of various types. The atomic moment will be a mean for the different ions, but while the saturation magneton value will be an arithmetic mean,  $\bar{p}$ , say, the value deduced from the  $\frac{I}{\chi}$ , T curve will be a root mean square,  $\sqrt{p^2}$ .

$$\text{For Ni } \bar{p} \doteq 3 \quad \sqrt{p^2} \doteq 8.$$

Now the ionic moment measurements show that for ions with the same number of electrons as  $\text{Ni}^{++}$  (26) and  $\text{Ni}^+$  (27) the values of  $\sqrt{p^2}$  are about 16 and 9. If, then, in a group of five atoms there are three with zero moment, one with 1 and one with 2 Bohr magnetons, for the mean atomic moments

$$\begin{aligned} \bar{p} &= \frac{1}{5} (2 \times 5 + 1 \times 5) \quad \sqrt{p^2} \doteq \sqrt{(16^2 + 9^2)/5} \\ &= 3 \qquad \qquad \qquad \doteq 8 \end{aligned}$$

Thus both the high and low temperature moments can be accounted for without the necessity of supposing that there is any essential change in the constitution of the nickel. A similar treatment may be successfully applied to Fe and Co, evidence for the existence of the appropriate atomic groups being adduced from the curves giving the relation between the mean atomic moments and the composition of the binary alloys of Fe, Ni, and Co.

*Single Crystals.*—Among ferromagnetic crystals, pyrrhotite was one of the earliest to be studied by Weiss and his co-workers. Weiss showed that the magnetic properties could be well accounted for if it was supposed to be built up of smaller somewhat idealised elementary crystals each of which behaved in accordance with Weiss's theory. In these there is a plane of easy magnetisation; at right angles magnetic saturation is not attainable, while in the plane there is a direction in which saturation is attained in very weak fields. For this direction the hysteresis curves are narrow (the coercive field is small) and practically rectangular. It was a consideration of these characteristics which led to the foundation of the general theory of ferromagnetics.

Crystals of iron (the structure is cubic, body centred) can now be produced of considerable size, and much attention has recently been directed to the study of their magnetic properties

(Beck, Webster, Gerlach, Honda). The main results for single crystals may be rapidly summarised. The hysteresis loops degenerate practically to straight lines, and it seems as if ideally there would be no hysteresis losses. There is no marked direction of easy magnetisation. In general, the magnetisation does not lie in the same direction as the applied field (in the 100 plane there is coincidence along the X and Y axes and the diagonals); this constitutes one of the most surprising results, for according to Weiss's theory, in a cubic crystal the coefficient  $N$  (Eq. 9) should be the same along the three axes.

The presence of a component of magnetisation transverse to the applied field in the cubic iron crystal seems to involve the existence of a structure field (of the order of some hundred gauss at saturation) different in origin from the Weiss molecular field. (This is still required to account for the ready attainment of saturation.) Mahajani has calculated the effect of the mutual magnetic action of the atomic magnets distributed on the crystal lattice points for the saturation case; with slightly idealised but quite reasonable assumptions, he is able to account quantitatively for Webster's results.

Webster has also studied magnetostriction and the longitudinal magneto-resistance effect in iron crystals. The results are somewhat simpler than in ordinary soft iron. On the application of a field there is an increase in length along the (100) axis, increasing with the magnetisation, but practically no change in the resistance; along the (111) axis there is a continual decrease in length, along the (110) an increase and then a decrease; along both these axes the resistance at first changes little, and then increases to a limiting value. The results suggest that the increase in length is due to an effect of the field on the structure of the atom which does not affect the resistance; and that the change in resistance is primarily due to changes in the orientation of the atoms, which must also be held responsible for the contraction. These suggestions, however, form only the beginnings of a theory.

Gerlach has noted the striking similarity between the hysteresis curves for single crystals and for some specimens of particularly fine grained electrolytic iron. He has suggested that this is an indication that magnetic properties of a specimen depend rather on the way in which the elementary magnetic domains "fit together" than on the size and number of the individual crystals. This leads again to a consideration of the problem of the Weiss molecular field.

*The Weiss Molecular Field.*—Unless there is some fundamental and quite unrecognised error in Langevin's theory of a paramagnetic gas, the existence of a molecular field proportional to the intensity of magnetisation seems to give the only reason-

ably satisfactory explanation of the magnetic behaviour of ferro-magnetics above and below the Curie temperature, of ferromagnetic crystals, and of ordinary paramagnetics; and of the energetic relations peculiar to ferromagnetics. The magneto-caloric effect, and the decrease of the specific heat of ferromagnetics at the Curie point (although this is not so sudden, as shown by the experiments of Sucksmith and Potter, as was at first supposed), seem necessarily to involve the existence of a field energetically equivalent to a magnetic field. But the field is far too large to be accounted for by the mutual action of the elementary magnets. Moreover, since the carriers are generally mononuclear, and even in the case of NO there is no magneto-electric directive effect (Huber), it cannot be supposed that the carriers are also electrostatic doublets, and that the field arises from these. The existence of anomalous paramagnetism independent of, or even increasing with, the temperature for many solid elements perhaps suggests how the field may arise. In a crystal, ions are held together by some kind of shared electron structure. When the ions change their orientation, the shared electron orbits will be distorted, and so the orientation of other ions will be indirectly influenced. The freedom of ions may vary greatly in different crystals, and also the type of interaction. There seems to be a possibility of explaining anomalous paramagnetism along these lines, and also the existence of positive and negative Weiss molecular fields in ordinary paramagnetics.

#### THE SPINNING ELECTRON AND THE NEW QUANTUM MECHANICS —CONCLUDING REMARKS

Although limitations of space preclude any detailed discussion, the fundamental importance of the spinning electron hypothesis and the new quantum dynamics is such that their significance for magnetism must be briefly referred to.

Until recently the doublet character of one valence electron systems was attributed to a splitting of the levels (*e.g.* in sodium, the  $k = 2$  level into  $^2P_1$  and  $^2P_2$ ), due to a magnetic interaction between the core and the series electron. The explanation was unsatisfactory, for the assignment of a magnetic moment to an inert gas-like core was not plausible, and attempted physical interpretations all broke down completely in detail. It was found, moreover, that separations such as  $P^3_1 - ^3P_2$  agreed with the well-known relativity formula for levels of different azimuthal quantum number ( $k = 1$  and  $2$ ), although the  $k$  was actually the same. This essentially constituted Millikan's relativity-magnetic "dilemma." Even for hydrogen, later work has shown that the line spectrum can be interpreted

as completely analogous to that of the alkalis, so that the original Sommerfeld relativity theory of the fine structure is incomplete.

Heisenberg put forward a scheme which enabled the spectral terms to be formally classified by postulating a "duplexity" in the levels which could be regarded as arising either from magnetic or relativistic interaction. It is this duplexity which the Goudsmit-Uhlenbeck hypothesis of the spinning electron attempts to explain. To the spinning electron is assigned an angular momentum of half a Bohr unit. The spin and the orbit rotation may be in the same or opposite directions, and a single orbital energy level will be split into two; for it may readily be shown that the spinning electron will precess as it moves in its orbit as it would if in a magnetic field. The Sommerfeld scheme for spectra may be taken over, but the impulse  $\tau$ , formerly attributed to the core, is now attributed to the non-balancing spinning electrons. The fact that four independent quantum numbers are required to specify a spectral term completely lends additional support to the view that the electron is more complex than a point-charge with only three degrees of freedom. It may be noted that the moment of the spinning electron would not reveal itself in ordinary experiments on free electrons; but nuclear structure presents a baffling problem if the "bricks" consist of electrically charged magnets.

Classical quantum calculations do not lead to satisfactory quantitative results for the term separations, but Heisenberg and Jordan have shown that if account is taken both of the electron spin and the relativity effect, the empirically correct relativity formula is obtained on the basis of the new quantum matrix dynamics.

The magnetic properties of atoms are little elucidated by the spinning electron hypothesis. To account for the Landé splitting factor  $g$ , it is necessary to assign a double magnetic moment to the electron, and a number of artificial features have to be introduced. As an illustration of the artificiality, the unit magnetic moment of alkali atoms is to be regarded (formally) as arising from a zero contribution from the orbital motion of the electron, and unit moment from the spin (that is, double the angular momentum). The gyromagnetic anomaly is still unexplained unless the effective ions in all the ferromagnetics examined are in an S state. (See the discussion on ionic moments.) The hypothesis of the spinning electron has, however, certainly cleared up many difficulties, and undoubtedly marks a great advance.

In the matrix form of the new quantum mechanics which has been so successfully applied to quantum problems, atomic

models are virtually abandoned. Relations are sought between essentially observable magnitudes. With much reason, it is assumed that it is unjustifiable to speak of an electron in an atom being in a particular place at a particular time; and the numbers used in the analysis to specify the analogues of space and momentum co-ordinates, for example, cannot be directly interpreted as physical quantities. In Schrödinger's undulatory mechanics—which leads to the same results—the basis is somewhat more physical. Crudely, a set of vibrations is associated with a mechanical system, and a state is stable when the associated vibrations are stationary and satisfy certain boundary conditions. On Schrödinger's view, too, the position of an electron in an atom at a given time loses significance. Rather, the electron is considered as a localised wave group which has the properties usually associated with the "free electron" only under certain conditions.

From a magnetic point of view, however, the important point remains that the atom may exist in a definite energy state specified by quantum numbers; and with each state will be associated definite magnetic characteristics. It is these magnetic characteristics with which this article has been concerned. It has been necessary, in many places, to slur over points of detail, and many topics, in particular in the field of magneto-optics, have been left on one side. An outline has been given of some of the more recent specific magnetic investigations, and the significance of the results has been discussed. There are innumerable problems for theoretical and experimental research. In studying them, much light may be shed on the wider and ever more baffling problem, now entering on a new phase, of the nature of radiation, of the structure of the atom, and the structure of matter.

# SURFACE TENSION

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## PART II

THE structure of surface films has been worked out in more detail than any other branch of the subject. Two methods have been used for their study, which can be shown to be equivalent. One is the direct measurement, by a bell-crank balance or a system of torsion wires, of the force outwards on a float which separates one end of a film-covered surface of water from a clean surface. In this method, introduced by Langmuir [46], the surface is first swept free from accidental contamination by barriers sliding on roughly flat surfaces, these surfaces forming the tops of the sides of a shallow trough in which the water is contained. The film is then put on in measured quantity, dissolved in a volatile solvent which has been purified so as to leave no trace of a film on evaporating. The barrier at one end of the surface is pushed up, pushing the film before it; the force on the film is transmitted to the other end, and appears as a thrust on the float. The film molecules may be regarded as small floating objects; the action of the barrier and float is naturally regarded as a compression on the film. The area per molecule is calculated from the area of the film and the quantity put on.

If we regard the film molecules and the water in the surface as a solution, then the float is a semipermeable membrane for insoluble and non-volatile substances, which can pass neither under nor over it; while the water molecules can easily pass under and are continually doing so. The surface pressure is therefore a two-dimensional osmotic pressure.

Fig. 3 shows the apparatus of Adam and Jessop [47g], diagrammatically. AA is the float; behind it is a surface of water which can be cleaned by the barriers; in front is another surface on which the film is placed. The float is connected by silk fibres to the mirror F, which reflects a spot of light so as to indicate its position; and by another fibre to a light rigid framework PQS, mounted on a torsion wire MM. By means of the graduated torsion head any desired force

can be applied to the float through the framework PQS; and the value of the divisions of the head in dynes per centimetre can be determined by hanging weights on the hook S. The gap between the ends of the float and the sides of the trough is blocked by thin strips of metallic ribbon B, which exert very little force on the float when slightly displaced, but if properly adjusted, effectively prevent leakage of a surface film even under considerable surface pressure. The apparatus will measure down to 0.01 dynes per cm., and up to about 3 dynes per cm. with a fine torsion wire; with a thicker one it will measure from 0.1 to 60 or more dynes. Two sets of apparatus are therefore desirable for an investigation covering a wide

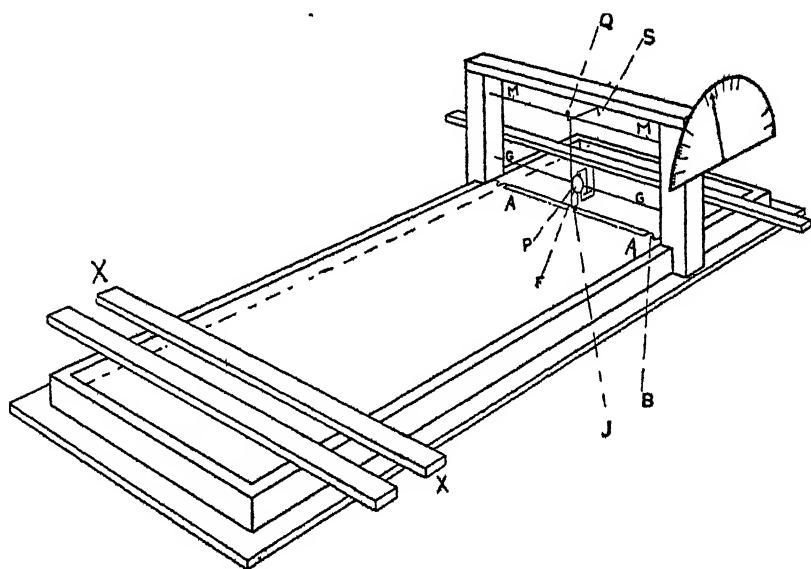


FIG. 3.

range of surface pressures. XX are sliding barriers which touch the surface of water in the trough. They are usually of plate glass, and must be coated with paraffin wax.

Let the free surface energy of the clean water surface be  $\gamma_0$ , that of the film covered surface  $\gamma$ . Let the surface pressure be  $F$ . Then the work done per centimetre length of the float in a slight displacement  $dx$ , is  $Fdx$ . By this process an area  $dx$  of surface of free energy  $\gamma_0$  is replaced by the same area of a surface of  $\gamma$ ; the work is therefore  $(\gamma_0 - \gamma)dx$ . Hence

$$F = \gamma_0 - \gamma \dots \dots (8).$$

Thus  $F$  is the difference between the surface tension of the clean and the film-covered surfaces; and we have the result

that the lowering of surface tension of a surface by a film is the two-dimensional osmotic pressure of the surface solution formed by that film.

This method of investigating surface films is inapplicable if there is appreciable solubility or volatility in the film. But we can always measure the surface tension of the surface, and subtract it from the known tension of the pure liquid; hence the surface pressure  $F$  is easy to find for any solution. Gibbs's adsorption equation, which relates the amount of adsorption in the surface to the rate of variation of surface tension with concentration, provides a means of calculating the amount in the surface per unit area. Let  $A$  be the average area per molecule in the film,  $c$  the concentration of the solution,  $a$  the "activity" (equal to the concentration for ideal solutions)

$$\frac{1}{A} = \frac{dF}{RT d \log_e a} \text{ or approximately } \frac{dF}{RT d \log_e c} \dots \dots (9).$$

The area per molecule in the adsorbed film of any solution is therefore found by taking a series of surface tension measurements at various concentrations, finding the "activity" of the solution, and plotting  $\log a$  against  $F$ . The slope of this curve is proportional to the area. This method ceases to be useful, owing to vanishing solubility of the substance, almost exactly at the point where the first method becomes applicable; there is just sufficient overlapping for it to be possible to compare the results of the two methods.

Fig. 4 shows the variation of surface pressure with area per molecule for the series of straight chain fatty acids, from 4 to 15 carbon atoms in length. The data are plotted with the product  $FA$  as ordinates and  $F$  as abscissæ. The molecules are small floating objects, and on account of their size will partake of the thermal agitation of the water molecules, if not aggregated by cohesion into large islands on the surface.

If free to move independently, the average kinetic energy of translation of each molecule, in each dimension of the surface, will be  $\frac{1}{2}RT$ . It can easily be shown [47*i*], that with this energy, the relation between pressure and area must be the same as that of a perfect gas (neglecting the size of the molecules):

$$FA = RT \dots \dots (10).$$

This equation has been verified for certain insoluble films within about 10 per cent., as a limiting value for very great dilutions of the films [47*i*], the value of  $R$  being the same as the gas constant per molecule,  $1.372 \times 10^{-16}$  ergs per degree. The conditions for agreement with equation (10) are similar to those necessary for the validity of the perfect gas equation  $PV = RT$ , namely, small size of the molecules in relation to

the space they occupy and a negligible tendency to aggregate into complex molecules on the surface. These disturbing factors only become negligible at great dilution of the films, when the area per molecule is several hundred sq. Å.U. ( $10^{-16}$  sq. cm.) or more.  $R$  becomes 1.372 when the unit of area is the sq. Å.U. and  $RT$  is about 400 at room temperature.

Fig. 4 is constructed from the surface tension data on solutions of the fatty acids from  $C_4$  to  $C_{12}$ , obtained by Szyskowski [48] and Frumkin [49], recalculated with activities in place

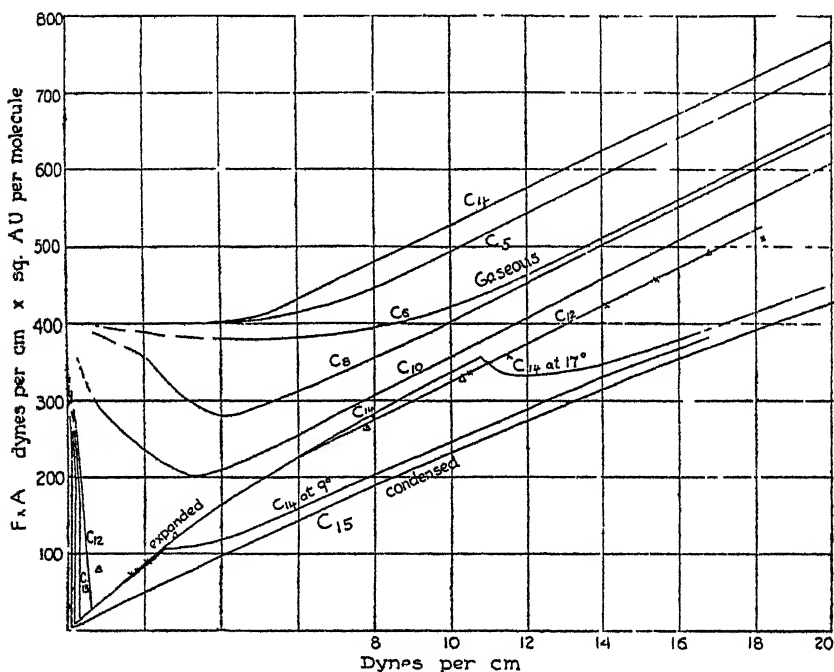


FIG. 4.—GASEOUS FILMS.

of concentrations by Schofield and Rideal [50]; and the measurements of Adam and Jessop [47g], using the apparatus of Fig. 3 on the acids  $C_{12}$  upwards. The shortest chain acids agree very closely with the perfect gas equation up to about 4 dynes per cm. (the form of the curves is not fully mapped here, but is clearly close to  $FA = 400$ ). The longer chain acids show increasing signs of cohesion, there being a minimum in the  $FA - F$  curves, just as with the  $PV - P$  isothermals of gases at moderate pressures. The curves rise and become parallel, nearly straight lines above about 10 dynes per cm. The equation of the parts of the curves above 10 dynes is very closely

$$FA = FB + RTx \dots \dots (11),$$

exactly corresponding to Amagat's equation for gases under high pressures [50].  $\alpha$  decreases with increasing cohesion between the molecules, and is

$C_4$	0.73
$C_5$	0.63
$C_6$	0.43
$C_8$	0.4
$C_{10}$	0.3

The cohesion thus increases with increasing length of chain of the molecule.

The experimentally determined points are marked in the diagram for  $C_{12}$ . The triangles are determined from Gibbs's equation and surface tension measurements; the crosses from the force on a float, treating the film as insoluble. As a matter of fact the film was very slightly soluble, but by working rapidly and noting the rate at which the surface pressure fell off with time, a close estimate could be made of the pressure of a film which had not had time to dissolve. The agreement between the two sets of points is most striking evidence of the accuracy of Gibbs's equation, perhaps the best experimental verification yet given. It is also evidence that there is no structural difference between the films adsorbed at the surface of the solutions of soluble fatty acids and those of insoluble fatty acids put on a clean water surface by evaporation of a solution in petroleum ether.

This correspondence both in the limiting case of extreme dilution and of moderate dilution, between the properties of surface films and of matter in the gaseous state, renders it extremely probable that the surface pressure on the float is due to the collisions of the film molecules; the corrections fall into two classes, those due to the size of the molecules and to their cohesion. In three dimensions, the correction due to the size of the molecules, when an equation of state of the Van der Waals type is used, is four times the volume of the molecules; in two dimensions it is twice the area [47c, 43]. At present, not much information regarding the structure of the films seems to be forthcoming from the various "equations of state" proposed, which is not immediately obvious without putting the data into that form, so we shall not enter into details of these equations.

At great surface dilutions (large areas per molecule) the orientation of the molecules has been found by Langmuir [46], as follows. The work of adsorption, when a molecule passes from the interior to the surface, may be found by treating the adsorbed layer as one region with a definite concentration, and the interior of the solution as another. The distribution of

molecules between the two regions may then be found by the Boltzmann equation

$$\frac{c}{c_1} = e^{\frac{\lambda}{RT}} \dots \dots (12),$$

$c$  and  $c_1$  being the concentrations in the two dilute regions and  $\lambda$  the work of transfer from one to the other. It is found that the work of adsorption per gramme-molecule increases by about 700 calories for each additional  $\text{CH}_2$  group in the chain, from  $\text{C}_3$  to  $\text{C}_9$ . This means that the position occupied by each extra  $\text{CH}_2$  group is the same, in relation to the surface, as those already present in the lower acids; and the only way in which this can be attained is by the molecules lying flat in the surface. Langmuir has also calculated the potential energies of molecules in various positions, from the known dimensions of the molecules and the surface energies of known areas of surfaces of various chemical composition, treating the exterior of the molecule as a surface of known area and adhesion similar to that of a large surface of the same constitution, and finds that the position lying flat is that of least potential energy [51]. There can be little doubt therefore that in the gaseous films the molecules are lying flat at areas of, say, 400 sq. Å.U. and over, and are moving about with the full thermal energy of translation corresponding to the temperature. There is some corroborative evidence that the molecules lie flat in the gaseous films. *A priori*, the molecules of long chain compounds have a much better chance of aggregating together into islands when upright than when flat, and are therefore more likely to form gaseous films when flat. The most perfectly gaseous films yet investigated, among those which can be treated by the float method as insoluble films, are those of the dibasic esters  $\text{C}_2\text{H}_5\text{OOC} \cdot (\text{CH}_2)_n \cdot \text{COOC}_2\text{H}_5$ ,  $n$  being ten or eleven [47]. These have two groups of a water-attracting nature, one at each end of the molecule, and are therefore far less likely to stand upright than the monobasic acids and esters, which have only one such group. Again, in one case it has been possible to transform a film consisting of coherent islands of molecules more or less upright, into one of a gaseous character, by applying a definite chemical attraction to the water at the middle of the hydrocarbon chain. The unsaturated acids, esters, and amides of the oleic and allied series have an ethylenic linkage in the middle of the chain, and on water and dilute acid form either condensed or liquid expanded films (see below), both of which are coherent islands. Acid permanganate in the water, a reagent which tends to oxidise the double bond, attracts it chemically and pulls it

into the water, tending to make it lie flat; all these compounds on acid permanganate form highly gaseous films. It was proved that permanganate had no such action on a number of compounds with no double bond, and practically none on iso-oleic acid with a double bond next to the carboxyl group [47*h*].

Fig. 5 shows the phenomena of condensation in the surface films of fatty acids  $C_{12}$  to  $C_{16}$ . The data are presented with the surface pressure  $F$  as ordinate and area as abscissa, as well as

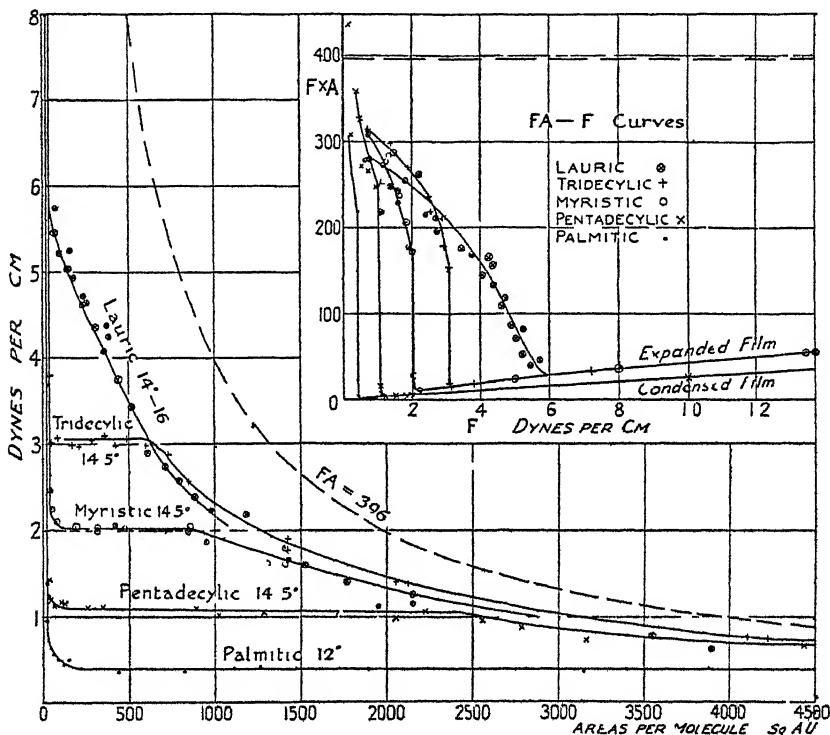


FIG. 5—TWO-DIMENSIONAL EVAPORATION.

in the FA - F curves, thus bringing out the resemblance between surface pressure and gaseous pressure by two forms of isothermals. The F - A curves are a very faithful reproduction of Andrews's well-known P - V curves for carbon dioxide in the neighbourhood of the critical point. For the acids of 13 carbons and up, there is a horizontal region, corresponding to the vapour pressure of the liquid. Here there must be two surface phases in equilibrium, islands of "liquid" (or occasionally solid) film in equilibrium with saturated vapour. The steep lines to the left of the horizontal region are

the comparatively incompressible isothermals of the liquid film; the slightly sloping lines to the right are the isothermals of the vapour gradually approaching the perfect gaseous condition. The isothermal of a perfectly gaseous film is dotted in. These phenomena were predicted by Langmuir in his first paper [46] but were not discovered experimentally until the apparatus of Fig. 3 was available. The acid  $C_{12}$  (lauric) is just above the critical temperature; we conclude therefore that in this surface films there are no islands of "liquid film."

Many other substances have been investigated in this way; they can be divided into two classes, those which form liquid films, and those which do not, being above their "critical temperature." This critical temperature is a function both of the composition of the molecule and of the solution. It is lower, the smaller the tendency to cohere together into aggregates, and this tendency is greater (*a*) when the molecules have plenty of residual affinity; (*b*) when they are of such a constitution that they easily stand upright in the surface, so that the adhesion has a large surface of contact over which to act.

Those films which are above the "critical temperature" vary from the almost perfectly gaseous to those which show a long straight region almost, but not quite, horizontal in the  $F - A$  curves. Instances of the first kind are the dibasic esters [47*i*] and the unsaturated substances on permanganate [47*h*]; of the second, ethyl palmitate [47*g*], at room temperature. The second class are called "vapour expanded" films [47*h*]. In these more or less perfectly gaseous films, the form of the isothermals indicates only one surface phase. It is possible to compress a gaseous film into an area much smaller than that required for the molecules to lie flat. In equation (11) the constant  $B$  is 25 sq. Å.U. for all the acids  $C_4$  to  $C_{10}$ , and the same value is found with the adsorbed films of phenol on water and salt solutions [52]. This constant is the area per molecule at very high pressures; the longest of the molecules for which the equation holds would occupy about 100 sq. Å.U. lying flat and 20.5 if vertical. Therefore high compression on gaseous films tends to force the molecules to stand upright, though complete upright orientation is not attained. Thus the orientation of molecules in gaseous films is that they *lie flat when there is sufficient space; when the area is reduced to near that occupied by the molecules themselves when flat, or below this area, the high surface compression compels many of the molecules to stand upright.*

There is great variety in the films when sufficient cohesion is present in form large aggregates on the surface. It is necessary to distinguish at least four different classes of coherent film, with the long chain compounds.

Condensed films (molecules perpendicular to surface).

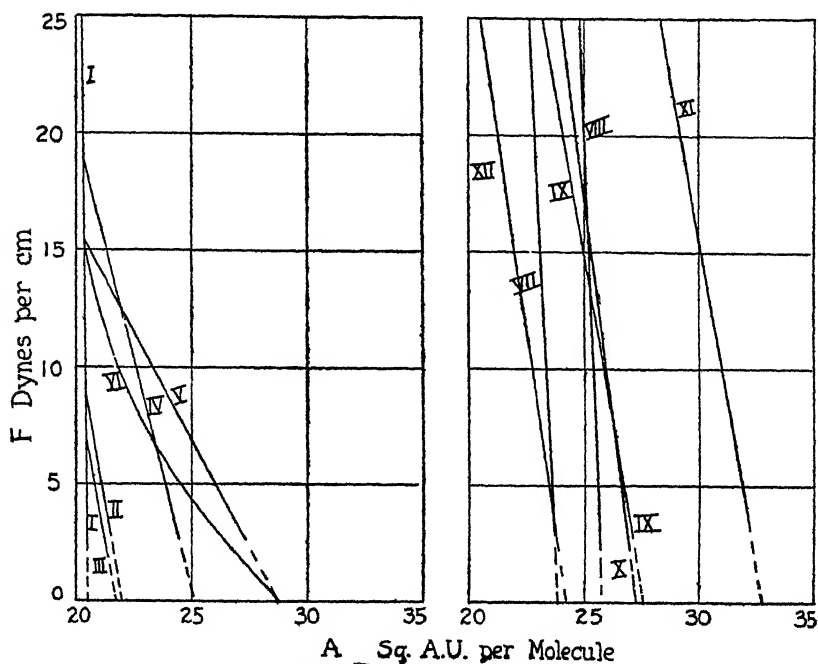
a. Close-packed chains. Close-packed heads.

b. Structure formed by heads resists compression.

c. Structure formed by heads rearranged by compression.

d. Liquid expanded films (orientation probably coiled down chains close packed with vertical axis).

Fig. 6 gives instances of each of the classes  $a$ ,  $b$ , and  $c$ . The curves are of the following compounds.



A Sq. A.U. per Molecule  
FIG. 6.—CONDENSED FILMS.

- I. Acids on distilled water (final curve); amides; triglycerides (areas per chain); ureas above the transition temperature; acetamides below transition temperature.
- II. Ethyl, methyl, and allyl esters of saturated acids.
- III. Alcohols.
- IV. Acids on dilute HCl.
- V. Iso-oleic acid on dilute HCl.
- VI. Ethyl iso-oleate.
- VII. Hexadecyl phenol and allied substances.
- VIII. Ureas below transition temperatures.
- IX. Nitriles.
- X. Final curves of  $C_{18}$  and  $C_{21}$   $\alpha$  bromo-acids, on HCl.
- XI.  $C_{16}$  and  $C_{17}$  bromo-acids on HCl.
- XII. Acetamides above transition temperature.

In most cases there is no alteration of the curves when the chain is lengthened. All but No. VI are sensibly straight lines. Nos. I to VI pass into I under high compression. The non-dependence of area per molecule on length of chain shows that the molecules are oriented perpendicular to the surface, or all at the same angle in every case. That all the compounds which give curve I should be oriented at the same angle is unlikely, since they include both liquid and solid films, many different end groups, and a 50 per cent. variation in length of chain; the proof is therefore practically complete that the chains are oriented perpendicular to the surface.

Curve I, the closest packing, must be close packed chains. It cuts the abscissa at 20.5 sq. A.U. and is exceedingly steep. We can calculate the approximate length of the chain, from the area per molecule and the density, assumed to be the same in the film and in bulk; then assuming that the whole of the compression on the film is borne by the film molecules in contact, we can translate the surface pressures into atmospheres compression on the thin film and compare the compressibility with that of matter in bulk. The compressibility of the film of curve I calculated in this way is of the same order as that of a long chain paraffin in bulk. There has been criticism [50] of this method of comparing surface pressures with bulk pressures, but applied to the condensed films in which the molecules are touching over the whole, or a known part of, their length, it seems perfectly legitimate. A clean water surface does not resist compression, and a film of floating molecules does; the resistance to compression must therefore be due to the floating molecules. Their length may possibly be slightly increased by attached water molecules, but there is not yet definite evidence on this point, and the correspondence between bulk and surface compressibility indicates that the assumed length of the molecule is not far wrong. It seems unlikely that the same comparison can be made with gaseous films, taking the thickness of the films as equal to the thickness of the molecule lying flat; here a complication is introduced by the pressure being a series of collisions. Schofield and Rideal [50] consider that when the correction to the gas laws, measured by the ratio  $\frac{FA}{RT}$ , has the same value for a

surface film and for a gas or a solution (using of course osmotic pressure in place of gaseous pressure), it may be assumed that the film and three-dimensional gas or solution are in corresponding states, so that one can identify the number of dynes per centimetre in the surface pressure of the film with the

number of atmospheres in the bulk pressure. In this way they obtained two estimates of the value of one dyne per centimetre in terms of atmospheres compression on the film, but the results were very discordant. The physical meaning of this comparison does not yet seem clear. It may be invalidated by the somewhat different nature of cohesion in gases and in films; in the films, increased adhesion between the upper parts of the molecule and the water antagonises the tendency to cohere into aggregates, because it tends to make the molecules lie flat. There is no counterpart to such an effect in three dimensions.

All the substances of the curves I to VI give curve I at sufficiently high compressions. At lower compressions most of them occupy larger areas, which are characteristic of the end group. The point where the lower portion of the curves cuts the abscissa is the maximum area of the end group in the films. When it is possible to reach curve I by simple compression it is evident that the end group can either be compressed or tucked away into recesses in the hydrocarbon chains; such recesses must exist on the tetrahedral theory of the carbon valencies. Probably the reduction of area is due to the heads being tucked away; in cases when the heads cannot be tucked away there is only a slight compressibility of the films (*e.g.* curves VII and VIII).

It is evident that these figures give an idea as to the shape of the molecules. Molecules of stearic acid occupying 20.5 sq. Å.U. on the surface must have a thickness of about 4.5 Å.U.: and if their density is the same as in bulk, which is the case at least as regards order of magnitude, their length must be about 26 Å.U. The molecules are, therefore, much elongated. The water attracting ends are usually slight swellings on the ends, as is shown by the figures for their cross section. The benzene ring in hexadecyl phenol and in the corresponding methyl ether and aniline has a cross section of 23.8 sq. Å.U., in good agreement with the section of a benzene ring perpendicular to the ring, as deduced from X-ray measurements, on the crystals. Bulky groups such as bromine in the end of the molecule increase the cross section considerably, and the ethylenic linkage in the  $\alpha\beta$  position to the carboxyl group causes the head to occupy 28.7 sq. Å.U. both in the acid and the ester. This seems probably due to a kink, rather than a local swelling, in the molecule at the double bond.

The principal areas of groups, as packed in the films, are as follows. The end groups are often called the heads of the molecules.

Hydrocarbon chains	.	.	.	.	.	20.5
$\text{CH}_2\text{CH}_2\text{COOH}$	.	.	.	.	.	25.1
$\text{CH}_2\text{OH}$	.	.	.	.	.	21.7
$\text{CH}_2\text{CH}_2\text{COOR}^a$	.	.	.	.	.	22
$\text{CH}=\text{CHCOOH}$	.	.	.	.	.	28.7
$\text{CH}=\text{CHCOOR}$	.	.	.	.	.	28.7
$\text{CONH}_2$	.	.	.	.	less than 21	
$\text{CN}$	.	.	.	.	.	27.5
$\text{C}_6\text{H}_4 - \text{OH}, - \text{OCH}_3, \text{ or } \text{NH}_2$	.	.	.	.	.	23.8
$\text{NHCONH}_2$	.	.	.	.	.	26
$\text{NHCOCH}_3$	.	.	.	.	.	24.2
$\text{C}_6\text{H}_4\text{NHCOCH}_3^b$	.	.	.	.	28.2 or 25.8	
$\text{CHBrCOOH}^c$	.	.	.	.	26 to 32	
$\text{CH}_2\text{CH NOH}$	.	.	.	.	.	25
Hydrocithin	.	.	.	.	.	52
Cholesterol	.	.	.	.	.	39
Triglycerides	.	.	.	.	.	63

*a.* R may be methyl, ethyl, or allyl.

*b.* Two packings according to temperature.

*c.* Several possible packings.

There are changes of state in the condensed films just as in other condensed forms of matter. The change from solid to liquid does not usually appear very sharp, under actual conditions. Films of classes *a* and *b* may be solid, or liquid, but there is no case of a film of class *c* being solid. Sometimes surface viscosity is so great that it is difficult to say whether the film is solid or liquid. Solidity is detected by dusting the surface and blowing or applying a shear. Another test is to put a few drops of a non-spreading oil on the surface and compress the film; solid films compress the drop to an oval shape, the contraction being greatest in the direction of the compression. Liquid films equalise the compression in all directions, and the drop remains circular. The films are always liquid some distance below the melting point of the bulk crystal; this is to be expected, since in the film the water-attracting groups are attached to moving water molecules, but in the crystal these groups are attached usually to other similar groups which are stationary. The acetamides show a definite melting point in the films. Hexadecyl acetamide  $\text{C}_{16}\text{H}_{33}\text{NH CO CH}_3$  melts at  $9^\circ$  and octadecyl acetamide at  $17^\circ$  in the films; the melting points in bulk are  $73^\circ$  and  $80^\circ$ . In these films the solid film is one with close-packed chains; the melting being accompanied with an increase in area, at no compression, of about 18 per cent. The melting in this solid film is simply the sudden development of a new type of motion which breaks down the rigid space lattice in two dimensions; it may be only the development of rotation about a vertical axis, perhaps an unsymmetrical projection on the molecule requiring more space when rotating than when stationary.

The two-dimensional solid structure may be due either to contact along the whole length of the molecules, or to contact over a small length only. The hexadecyl phenol, and other benzene derivatives form strong condensed films, but only the benzene heads seem to be in actual contact. There is probably no rearrangement of their structure on compressing, but the benzene rings take the whole pressure. The compressibility of these films is actually of the order to be expected if this is the structure [47*d*]. The ureas,  $R\ NH\ CO\ NH_2$ , have only their heads in contact below a certain transition temperature and occupy 26 sq. A.U. Here the solidity must be due to a structure formed by the last three atoms in the chain, and the compressibility of the films in this state is of the right order. Temperature has little effect until a definite point is reached, when this crystalline structure of only three atoms thickness is disrupted and slight compression forces the heads out of the way, packing the chains closely [47*f*]. Such changes as this are unquestionably the same as allotropic changes in solids; and their study is in some respects easier, since the molecular structure of the films may be easier to discover than that of the solid.

Class *d* of the films possessing cohesion, and forming a phase distinct from the gaseous, is perhaps the most interesting of all. These are the "liquid expanded" films. Fig. 8 shows the relation between pressure and area with myristic acid (on dilute HCl) from 2.5° to 34.4°; and inset are the curves of variation of area with temperature at constant pressure (isothermals).

The left-hand isothermal is characteristic of the condensed film of fatty acids on dilute HCl (curve IV of Fig. 7). At 2.5° we find the condensed film in equilibrium with the gaseous, having a vapour pressure of the order 0.17 dynes. At 12° there is a totally different film in equilibrium with the gas, the area being about 48. As the temperature rises further, there is only a slight further increase in area at low compression. But the pressure required to cause collapse of the film of larger area into that of smaller increases with the temperature, until at 34.4° it is not experimentally attainable. The film which gives the curve to the right of the figure is called the expanded film. If, as in this case, it is in equilibrium with gaseous film at a definite vapour pressure, the expanded film is a separate surface phase and is called a liquid film [47*h*].

Besides the simple fatty acids, the bromo-acids, amides, nitriles, phenols, ureas, and possibly the alcohols can form liquid expanded films, having in all cases isothermals like the right-hand curve of Fig. 7 [47*h*]. There is astonishingly little difference between the curves for different liquid expanded

films ; the bromo-acid films are, however, somewhat less easily compressed than the others, probably because the molecules are a good deal larger. Out of fifteen substances examined close to the temperature at which expansion takes place, the area was in fourteen cases within two units of 48 sq. Å.U. ; the remaining case was three units smaller, but this also was probably within experimental error, as the film was collapsing slightly during the measurements. All the liquid expanded films have a rather high coefficient of thermal expansion, as is shown by the upper part of the isopiestic in Fig. 7. This

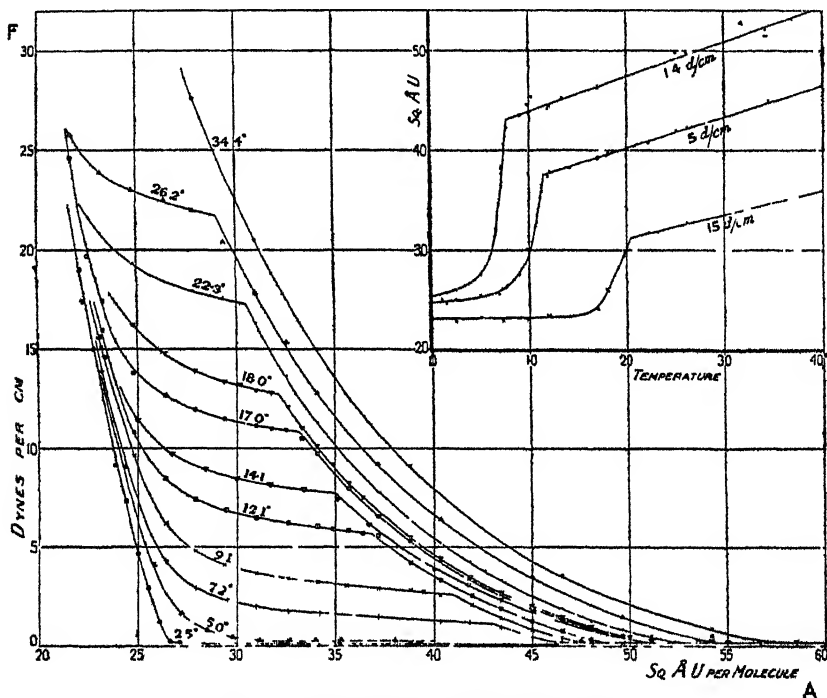


FIG. 7.—EXPANSION OF MYRISTIC ACID.

is of the same order as the expansion of a gas, but some liquids, especially near their critical points, have a similar coefficient of expansion. The substances studied ranged over a variety of heads and chains varying from 14 to 22 carbons in length.

As the molecules are cohering, they are probably in contact. The constancy of the area with different heads indicates that the area is a measure of the cross section of the chains in contact ; and the constancy with different lengths of chain indicates either a general vertical direction, or equal tilt in every instance. As with the condensed films, the variety of substances studied makes it somewhat unlikely that there is

an equal tilt in all cases. The possible configurations of the long hydrocarbon chains include one which could give approximately the area of forty-eight without violating the known facts of stereochemistry. The angle between the valencies of the carbons being  $109^{\circ} 28'$ , a model shows that one can coil the molecule down into a helix with vertical axis, so that when each successive ring is resting on the one below, each will have roughly the dimensions of cyclohexane  $C_6H_{12}$ . Benzene should require about 39 sq. Å.U. if lying flat on the surface and 9 sq. Å.U. does not seem excessive for the increase in cross section caused by six extra hydrogens. Though the configuration of close packed, coiled down helices with vertical axes cannot yet be said to be proved for the liquid expanded films, it seems the most probable. If it is right, then the transitional curves between the expanded curves and the condensed give information as to the stability of the helical formation under compression at various temperatures. It is evident that compression forces the helices to commence straightening out vertically. If the temperature is high enough the compression is met by an increasing resistance as it proceeds. If the temperature is not far above that at which the expansion occurs, the resistance to straightening out increases uniformly until a critical point is reached, when the chain begins to collapse. The curves show that the change at the end of the transitional region nearest the expanded curve is much more sudden than that just before the condensed curve is reached. It seems difficult to explain why, once the transition or collapse of the helix to become a vertical chain has commenced, it should proceed more slowly as the vertical position is reached. We know so little, however, at present about the forces affecting a change of configuration of a long chain molecule that it is difficult to draw conclusions from the shape of the curves. Since the transitional region between expanded and condensed films is never quite abrupt, it is certain that the condensed and expanded films cannot be treated as two separate phases.

Some substances do not form liquid expanded films, but pass straight from the condensed state into a state continuous with the gaseous. These have no vapour pressure region nor do they tend to a definite limiting area at low pressures. In these substances, the temperature of expansion is below the critical temperature of surface evaporation. The ethyl and methyl esters, and the methyl ketones, form these "vapour expanded films," the esters with a  $COOC_2H_5$  group on each end form almost gaseous films on expanding from the condensed state.

In 1922 [47c], when the facts of surface evaporation were

not available, I suggested that the process of expansion was a surface evaporation. The suggestion has been criticised and must clearly be abandoned. There is a slight justification now, for calling the "vapour expanded" films gases; but though in some cases these approach fairly near to the perfectly gaseous state, more frequently between 25 and 120 sq. Å.U. they are so different from gases that a special name seems desirable.

These surface films give a good deal of information as to the force-fields round the molecules. In the condensed films, the molecules stand on end, and it is found experimentally that the films are no longer stable unless the end group is one which tends to dissolve in water and is not blocked by bulky groups. Turning the OH group into  $\text{OCH}_3$  renders a stable film unstable, the molecules leaving the surface and piling up into aggregates. Turning  $\text{COOH}$  into  $\text{CONHC}_6\text{H}_5$  has the same effect, and the latter group can probably not be measured in the films. Cetyl palmitate  $\text{C}_{16}\text{H}_{33}\text{OOC C}_{15}\text{H}_{31}$  also does not form a stable condensed film. Hence an essential for stability of the films is a strong attraction between the end group and water. Again the lateral attractions between the chains are important in stabilising the condensed films. The temperature of expansion is raised by some  $10^\circ$  near  $0^\circ$  and  $7^\circ$  near  $60^\circ$  for each additional carbon atom in the chain; and this law is universal for molecules which have only one chain, although the absolute temperature of expansion depends very much on the nature of the head and often also on the composition of the solution. The state of "liquid expanded" films seems to be stable only if there is a considerable amount of residual affinity in the heads of the molecules. Esterification of the acidic group prevents these films forming. It would seem that adhesion between the heads laterally is necessary to supplement the lateral adhesion of the coiled down chains; if this be insufficient the molecules separate a stage further and the films become gaseous.

We are beginning to see how the surface properties of molecules determine cohesion and surface tension, and that these surface properties are also responsible for solution and are of the same nature as the attractive forces long termed "chemical." Langmuir [53] outlines the calculation of vapour pressures, solubility, etc., from data as to the surface properties of molecules, but as these calculations have not yet appeared in full, it is not time to discuss them. It is probable that the accumulation of data of this kind, and the development of the kinetic theory of molecules closely packed but yet free to move, will unify all these subjects and show that comparatively few independent factors are at work.

Just as it is possible to trace a gradation, from a visible sediment in a beaker of water, through the stage of suspension which settles only slowly, and the colloidal solution, to the true solution with single molecules ; so we can commence with the floating waxed needle whose surface does not attract water strongly, passing next to the particles of ore floated by the oiling of their surfaces, needing only the agitation of air-bubbles to carry them to the surface, thence to the automatically but slowly adsorbed skins of colloidal matter such as albumen, finally reaching the adsorbed molecules of a short chain fatty acid. The influence of gravity becomes less important in each series, the influence of Brownian movement more important, as one passes from the large particle to the small ; yet the surface properties which cause the adsorbed molecule to go to the surface are the same as those which permit the floating of the waxed needle. A surface is equally hydrocarbon in character if the area is so small as to allow room for only one or two  $\text{CH}_2$  groups, or if it is many square centimetres in extent.

Similarly there seems to be no discontinuity between the resistance of a waxed cloth to the passage of moisture and the resistance of a homogeneous membrane in which the water is only slightly soluble. The molecules of water in each case have but little attraction for the walls of the tube through which they have to pass, and though in the first case the thermal excursions of the molecules of the walls are unimportant, and in the second very important, as the molecules fill nearly all the space, the chemical character of the surfaces of the molecules governs the phenomena in both cases. The property of permeability, and the allied one of power to dissolve, are closely connected with the surface properties of molecules. The camper who watches the rain coming through the pores of an imperfectly waxed fabric may try to amuse himself by tracing the connection between the passage of water along a waxed tube and the solution of water in hydrocarbon solvents. What is the property of solutions which corresponds to the friction between liquid and solid, which helps to prevent the rain passing along the tube, and causes the variation of angle of contact between liquids and solids ?

Rise of temperature diminishes surface tension, and we can obtain a clear physical picture of the mechanism by which the free surface energy is reduced by the kinetic motions of the molecules, if we abandon the idea of a contractile surface skin, using the thermal pressure in a horizontal direction of the surface molecules. This is another case where treating the phenomena from the point of view of the pressure due to the molecules gives a reasonable mechanism, and shows the relation between the capillary phenomena and other branches of Physics.

Consider a float which divides a surface at temperature  $T$  from one at  $T + dT$ . This is analogous to the float dividing a clean from a film-covered surface. Let the float be supposed only affected by the thermal pressure of the surface molecules (for the moment leaving open the definition of what constitutes a surface molecule). Let the thermal pressure at  $T$  be  $F$ ; at  $T + dT$ ,  $F + dF$ . The force per centimetre on the float is  $dF$  and the work required to move it through  $dx$  centimetres in the direction of the warmer surface is  $dF \cdot dx$ . This is also  $d\gamma \cdot dx$ , since the movement replaces  $dx$  sq. cm. of warm surface by cooler surface. Hence  $dF = -d\gamma$ .

The approximate, well-known law that the surface tension diminishes linearly with rising temperature, simply means that the magnitude of the horizontal pressure on a boundary, due to the motions of the surface molecules, increases proportionally to the temperature, and this law is the same as Boyle's and Charles's law for gases.

Eötvös [54], and Ramsay and Shields [55], drew attention to the analogy which exists between the linear diminution of surface tension with temperature *decreasing* from the critical, and the linear increase of gaseous pressure with temperature *increasing* from absolute zero; but they regarded surface tension as the tension along a line of the surface, and consequently the analogy was imperfect and the temperature coefficient had the wrong sign.

Eötvös supposed that the surface tension would be connected with the number of molecules per square centimetre of surface and based a method for determination of the degree of association of the molecules in a liquid on the variation of the surface tension with temperature. There has been much work on this question, and it is now generally agreed that the degree of association cannot be calculated by Eötvös' method, and that other disturbing factors, such as orientation of the surface molecules, will affect the value of the thermal coefficient of surface tension.

Let us introduce sweeping simplifications and assume that the horizontal surface pressure is equal to that of a two-dimensional gas with the same number of molecules per sq. cm. Then  $FA = RT$  and  $F = NRT$ , where  $A$  is the area per molecule and  $N$  the number of molecules per square centimetre of surface.

$$\frac{d}{dT} \left( \frac{F}{N} \right) = - \frac{d}{dT} \left( \frac{\gamma}{N} \right) = R \dots \dots (13).$$

Strictly we do not need to assume that the actual magnitude of the thermal pressure is equal to that of a gas, but only that its rate of variation with temperature is the same. From equation (13) we can obtain a value for the "Eötvös" con-

stant,  $\frac{d}{dT} \gamma (MV)^{\frac{1}{3}}$ ,  $MV$  being the molecular volume, making various assumptions as to the orientation and association in the surface. Suppose there is cubical packing in the liquid and no association, then  $N = \frac{6.06 \times 10^{23}}{(MV)^{\frac{1}{3}}}$ , and

$$\frac{d}{dT} \gamma (MV)^{\frac{1}{3}} = 1.372 \times 10^{-16} (6.06 \times 10^{23})^{\frac{1}{3}} \\ = 0.98.$$

The surface is assumed to be one molecule thick. If it is  $n$  molecules thick, the constant is  $0.98n$ .

If the molecules are not cubical, but packed as rectangular parallelepipeds, square section,  $x$  times as long as thick, and are oriented perpendicular to the surface, the number in the surface is

$$\frac{6.06 \times 10^{23}}{(MV)^{\frac{1}{3}}} \times (x)^{\frac{1}{3}}.$$

Hence the constant is increased in the ratio  $(x)^{\frac{1}{3}}$ , and will be four times that for a cubical molecule, if the molecule is eight times as long as thick. Association diminishes the number of molecules in the surface in the ratio of the two-thirds power of the degree of association; if this be  $z$ ,  $N$  is

$$\frac{6.06 \times 10^{23}}{(MV)^{\frac{1}{3}} \times (z)^{\frac{2}{3}}}.$$

On this theory, therefore, there is an underlying theoretical justification for attempting to calculate the degree of association, as Eötvös did, from the value of the constant. But the disturbing factors of orientation and the unknown ones introduced in the assumptions underlying equation (13) may vary from substance to substance, so that it is not surprising that experience has shown the unwisdom of drawing conclusions as to molecular association from the value of Eötvös' constant.

Jaeger's extensive series of measurements [6] gave values for the Eötvös constant from 0.24 up to 6.75, on different substances. Sometimes  $\frac{d}{dT} \gamma (MV)^{\frac{1}{3}}$  was constant over a wide temperature range for a single substance, but very often it varied greatly, sometimes rising, and sometimes falling, with rising temperature. It is evident that the value of 2.1, formerly supposed to be the "normal" value for unassociated liquids, is largely accidental.

Born and Courant [56] have investigated the thermal motions on the molecules, using the quantum theory on the lines of Debye's treatment of the specific heat of solids. They

obtained good agreement with experiment with several liquids for which the constant is about 2.1. Three degrees of freedom were assumed for the possible motions of the molecules, and the value of the constant would be increased in proportion to the two-thirds power of the number of degrees of freedom, if more than three were present. It seems doubtful if their theory as it stands could account for the great variety of values obtained in practice and the crude theory developed above may serve for the present to give an idea of the broad governing rules.

The majority of long chain compounds, with eight carbon atoms or more in the hydrocarbon chains, show much greater regularity in the temperature coefficient of the surface tension, than in the Eötvös constant, which has the molecular weight introduced as a multiplier.  $\lambda - \frac{d\gamma}{dT}$  is generally close to 0.07 dynes per cm. per degree. For the triglycerides  $\lambda - \frac{d\gamma}{dT}$  is only some 25% smaller than for the single chain compounds such as palmitic acid. We may suppose that the molecules of the long chain compounds are oriented perpendicular to the surface, and that the thermal pressure is due rather to the vibratory motions of the upper ends of the chains than to the translatory motions of the molecules as a whole. To a mechanism capable of being affected only by the extreme surface of the liquid, less in depth than the length of one of these long molecules, such vibratory motions could not differ in effect very much from translatory. Hence although the strictly translatory motion of the molecules of the triglycerides as a whole involves the simultaneous movement of three chains instead of independent movement of all, the surface pressure is not much impaired.

Some of the figures are reproduced from my papers in the *Proceedings of the Royal Society*, and *Chemical Reviews*, by kind permission.

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# THE BIOLOGICAL AND ECOLOGICAL ASPECT OF MIGRATION IN APHIDES

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## INTRODUCTORY REMARKS

### PART I

APHIDES, or "plant lice," are well-known insects, not only on account of their economic importance in agriculture and horticulture, but also because of their interesting and complex life-cycles. The biological problems associated with them have attracted the attention of investigators since the time of Leeuwenhoek, who first observed the phenomenon of viviparous parthenogenetic reproduction in these insects.

They belong to the insect order Hemiptera, and present-day systematists regard them as forming a superfamily—the Aphidoidea—composed of two families, the Aphididæ and Phylloxeridæ, which include about 140 known genera and somewhere about 1,000 species, of which some 400 species are found in Britain. They are entirely plant feeders, and live on the leaves, shoots, stems, or roots of plants, from which they abstract the juices by means of specially adapted sucking mouth-parts. In connection with this habit, many species have come to be associated with particular kinds of plants and complicated habits of migration have been evolved, together with the development of definite polymorphic generations. The Aphididæ and Phylloxeridæ are readily separated on biological grounds in that the parthenogenetic females in the latter family, which includes *Phylloxera* of the vine, oak, etc., and the "*Chermes*" group on conifers, are always oviparous and viviparity does not occur, whereas in the former family the parthenogenetic females are always viviparous and oviparity only occurs with the sexual females.<sup>1</sup> Furthermore the life-

<sup>1</sup> According to the rules of nomenclature, the name *Chermes* appears to have no standing as an aphid genus, but strictly belongs to the Psyllidæ. The term has, however, been long used in referring to certain well-known aphides infesting conifers, and as it has not yet been replaced by any generally accepted name, it is used in this sense throughout the present paper.

cycle and habits have reached a more advanced stage of specialisation in the Phylloxeridæ than is the case in the Aphididæ. The latter family contains a large assemblage of forms with diverse habits, which exhibit varying degrees of complexity of the life-cycle.

### I. NON-MIGRATING APHIDES

The various species of aphides whose life-cycles are known may be grouped into two classes, as non-migratory and migratory species. In the former class the life-cycle is completed either on the same kind of plant or at any rate on closely related plants. The species may be polyphagous in a restricted sense, or monophagous, and this free non-migrating habit, as seen for instance in certain members of the tribe Callipterini, probably indicates the more primitive habits of aphides. Non-migrating types have, however, in some instances apparently been secondarily derived from migrating types. Non-migrating species are often associated with trees or woody shrubs, but some are confined to herbaceous plants on which the parthenogenetic generations and sexuales occur and the fertilised eggs are laid in autumn. The destructive wheat aphid in America (*Toxoptera graminum*), for instance, lives entirely on wheat and associated grasses. Some species of the genus *Macrosiphum* in Britain live entirely on herbaceous plants.

The life-cycle of a non-migrating species may be briefly described as follows: The stem-mother, or fundatrix, hatches out in spring from the fertilised egg laid by the oviparous sexual female, being the foundress of subsequent generations of parthenogenetic females. These generations may consist of winged and wingless individuals, although in some species of the tribe Callipterini, apterous forms do not occur. After a varying number of parthenogenetic generations have been passed through, the true sexual females and males develop and the fertilised eggs (winter eggs) are laid, which lie dormant during winter.

### II. MIGRATING APHIDES

The migrating habit has arisen in several groups of the superfamily Aphididoidea and specialisation has developed in different ways. The essential feature of a migrating species is, that there is a definite migration from the food plant on which the fertilised eggs are laid by the sexual females to other food plants, either closely related or more often totally different, on which the parthenogenetic generations are produced. The former food plant, which is usually a tree or woody shrub, is the winter or primary host, and the latter the intermediate or secondary host. These two types of food plants

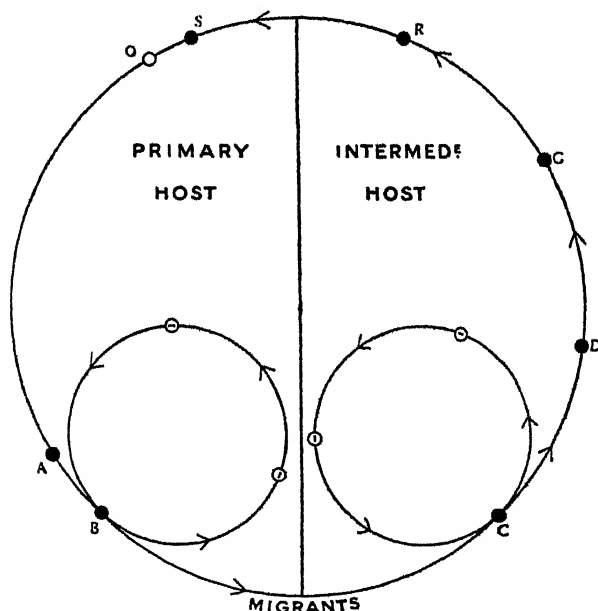
play a definite rôle in the life-cycle of migrating species, more especially in those cases in which the habit has reached an advanced stage of development, as in the "Chermes."

In association with the migrating habit, specialisation of form has occurred in the various generations. For instance, the change of host plant necessitates the development of winged individuals at certain periods of the life-cycle, in order that migration may be accomplished. Moreover, the sexual forms in many cases have become specialised apterous forms. This specialisation of form is well seen in those species which exhibit a highly advanced development of the migratory habit, as in members of the "Chermes" group. In these cases, certain generations may be definitely associated with a particular host plant. Owing to the specially complex nature of the life-cycles of members of the Chermes and Phylloxera groups (fam. Phylloxeridæ), it will be more convenient if they are dealt with later, and for the present we consider only those species generally known as the true aphides or plant lice, which comprise the family Aphididæ.

The general plan of the life-cycle of migrating species of the family Aphididæ, in temperate climates, which is shown in text fig. 1, is briefly as follows. The fertilised eggs are laid by sexual females on the primary host in autumn, from which the parthenogenetic, viviparous fundatrix, or stem-mother, hatches out in spring. Eventually winged migrants are produced, which migrate to the intermediate host, on which a series of parthenogenetic generations occur during summer. In autumn winged re-migrants fly back to the primary host, on which the sexuales occur and the fertilised winter eggs are laid. There are, however, many modifications of this simple cycle, which will be discussed when dealing with the various types of migration. Certain species belonging to the more widely distributed group of aphides, namely, the tribe Aphidini, may be classed as migrating species, but they have evidently not advanced far in the development of the migrating habit, and under favourable conditions the life-cycle can be completed on the primary host. The migration in such species is spoken of as facultative, and in those species in which both host plants are essential for the completion of the life-cycle, it is referred to as obligatory migration.

We see therefore, that in both non-migrating and migrating species, the complete life-cycle consists of a parthenogenetic phase and a sexual phase. In temperate climates, sexual forms normally occur at the end of the vegetative season and the winter eggs lie dormant during winter. The parthenogenetic phase of the life-cycle appears to be an adaptation to seasonal conditions and ensures a wide distribution of the

species over the favourable season of the year. In this respect it is interesting to note that, in semi-tropical countries, where favourable light, temperature, and vegetation conditions occur practically all the year round, many species of aphides are recorded as carrying on parthenogenetic reproduction throughout the year. In fact, in temperate countries, some



TEXT FIG. 1.—A GENERALISED DIAGRAM SHOWING THE MAIN FEATURES IN THE LIFE-CYCLE OF MIGRATING APHIDES.

On the left half of the circle the generations which occur on the primary or winter host are represented and on the right half those on the intermediate or summer hosts. The continuous large circle shows the normal, parthenogenetic and bisexual cycle and the two smaller circles show the cycle of those non-migrating races or species which reproduce continuously either on the primary or the intermediate hosts as described in the text. A = Fundatrix generation; B = individuals of first Fundatrigeniæ generation; C = individuals of the first alienicolæ generation, which are produced by migrants from the primary host; D-G, further alienicolæ generations which develop during the summer; R = sexuparæ, or return migrants which produce the sexuales; S = sexual forms; O = fertilised eggs; (⊖) = parthenogenetic individuals.

species are known to carry on parthenogenetic reproduction under sheltered conditions throughout winter, in addition to the production of sexuales in the autumn.

In a few species (*Aphis saliceti* on *Salix* and *Mindarus abietinus* on *Abies*) the sexual forms are produced in early summer and the fertilised eggs remain dormant until the following spring. With the "Chermes" also, the sexuales are produced in early summer, and the larvæ which hatch out

from the fertilised eggs remain immature until the following spring.

### III. THE POLYMORPHIC FORMS IN APHIDES

In the life-cycle of a migrating aphid, certain polymorphic forms occur. These forms have received various names from time to time, and as the use of definite terms will greatly facilitate reference later on, the terms which are now generally used for members of the Aphididæ will be explained. The special features found in Phylloxerons and "Chermes" will be dealt with later.

(a) *Fundatrix*.—This is the parthenogenetic female which hatches out on the primary host from the fertilised egg. It is apterous, being viviparous in members of the Aphididæ and oviparous in the Phylloxeridæ. (It should be noted that in some non-migrating forms of the Callipterini the fundatrix may be winged.)

(b) *Fundatrigeniæ*.—This term refers to the generations initiated by the fundatrix on the primary host. There may be one or more generations in which winged or apterous and winged individuals are produced. The alate forms include the migrating individuals or *fundatrigeniæ migrans*, but may also consist of individuals which do not migrate but reproduce on the primary host. These latter are the *fundatrigeniæ non-migrans*. The apterous individuals of these generations are the *fundatrigeniæ apteræ*.

In the "Chermes" group these generations are called "*gallicolæ*" on account of their gall-forming habits on the primary host, *Picea* (spruce).

(c) *Alienicolæ*.—The winged migrants (*fundatrigeniæ migrans*) which fly to the intermediate food plants, start a series of parthenogenetic generations on these plants which are called the *alienicolæ* generations. (In the "Chermes" group they are called the *exsules* or *colonici* generations.) They may consist of apterous individuals only (*Alienicolæ apteræ*) or apterous and winged individuals (*Alienicolæ alatæ*). The latter ensure distribution of the species from one food plant to another, and certain of them which are destined to produce sexual forms are known as the *sexuparæ*, being re-migrants which fly back to the primary host.

(d) *Sexuparæ* or *Re-migrants*.—These terms are applied to certain of the alate alienicolæ which, born on the intermediate food plants, migrate to the primary host and there produce the sexual forms. They are normally produced towards the end of the vegetative season. In the case of the migrating members of the Aphidini, they usually produce only sexual females, winged males being produced about the same time,

by apterous alienicolæ on the intermediate food plants, which then fly over to the primary host where mating occurs. In those species which have more specialised migrating habits, as in the tribes Eriosomatini and Pemphigini, both sexual forms are apterous and are produced by the sexuparæ on the primary host.

(e) *Sexuales*.—The true sexes are associated with the primary host, and with the production of fertilised eggs by the sexual females, the normal cycle is brought to an end. As will be seen later, however, in some instances the parthenogenetic alienicolæ generations may be continued independently throughout the year, on the intermediate food plants. The sexes exhibit varied specialisation of form in the different groups of aphides and in general the ancestral winged condition of the sexual female has been replaced by apterous forms, although in a few non-migrating species, e.g. *Neophyllaphis podocarpi*, alate sexual females have been recorded. In those species which exhibit advanced habits of migration, however, both sexual forms are apterous, and in many cases have developed into highly specialised forms.

#### IV. THE VARIOUS TYPES OF MIGRATION

It would be impossible in this short account of the subject to refer adequately to the many distinguished investigators who have advanced our knowledge of the migrating habits of aphides. The names of Balbiani, Blockmann, Börner, Cholodkovsky, Lichtenstein, Marchal, Mordwilko, Nüsslin, and Patch, and their noteworthy contributions on this aspect of the biology of these insects, are well known to entomologists. More recently Mordwilko has discussed certain theoretical considerations regarding the origin and development of the migratory habit which will be discussed in Part II of this paper.

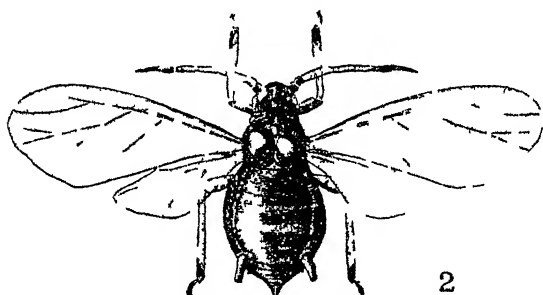
In so far as the life-history of migrating species have been worked out, the lines along which the habits of migration appear to have developed may be conveniently grouped into four classes. There are, however, many species of aphides whose migrating cycles have not yet been definitely traced.

#### CLASS A. MIGRATION FROM TREES OR SHRUBS TO HERBACEOUS PLANTS

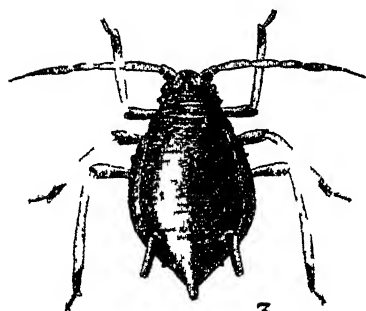
This type of migration occurs more particularly with members of the tribe Aphidini, in many of which the migrating habit appears to be in an early stage of development.<sup>1</sup>

<sup>1</sup> C. Börner (in Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, 1926, Lieferung 204, p. 223) lists 56 species, with their primary and intermediate host plants, which on the data given could be placed in this class of migration.

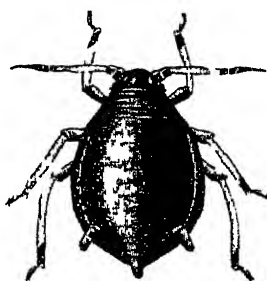
The intermediate host does not play such an important and definite rôle as is the case in those species which have more



2



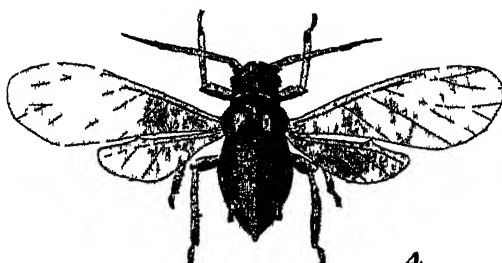
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1



5



4

THE VARIOUS FORMS WHICH OCCUR IN THE LIFE-CYCLE OF A MIGRATING APHID  
(*APHIS RUMICIS* L.)

(1) Fundatrix, or stem mother (2) Winged viviparous female migrants which produce parthenogenetic females in spring and summer (3) Apterous, viviparous, parthenogenetic females (4) Males (5) Sexual oviparous females produced in autumn by winged re-migrants (sexuparae) which resemble (2) in appearance

specialised migrating habits. Further, many species in this class may live on a wide range of intermediate host plants,

which may vary in different countries. The important consideration is, that herbaceous plants afford favourable food conditions during the summer months for the parthenogenetic generations, in comparison with the somewhat unsatisfactory food conditions on the primary host during that period. Some well-known species whose life-cycles have been traced belong to this class, such as the currant-lettuce aphid (*Amphorophora cosmopolitana*), the willow-parsnip aphid (*Cavariella capreae*), the mealy plum-reed aphid (*Hyalopterus arundinis*), the hop-damson aphid (*Phorodon humuli*), and the Euonymus-bean aphid (*Aphis rumicis*).

An account of the life-history in Britain of the black, bean aphid (*Aphis rumicis*), which has been investigated by the writer, will serve as a representative of this class of migration.

The fertilised eggs are laid about October by the apterous sexual females on the spindle tree (*Euonymus europæus*), from which the fundatrices hatch out the following March or April. These initiate two or more generations of fundatrigeniæ which consist of apterous and alate individuals. The latter migrate to the intermediate food plants such as beans, poppies, etc., on which a series of alienicolæ generations are produced, the length of the developmental period of the generations being greatly influenced by the prevailing temperature. Winged forms develop at irregular intervals in the alienicolæ generations, thus resulting in a wide distribution of the species. Winged males appear about the end of September on the intermediate food plants and at the same time alate sexuparæ, which both return to the primary host, the latter producing the apterous sexual females. Mating occurs and the fertilised winter eggs are laid and the sexual forms die. The alienicolæ generations may, however, continue reproducing parthenogenetically throughout winter, under favourable sheltered conditions, on suitable food plants; and in the warm days of spring when food plants again become plentiful, a rapid increase of these colonies takes place.

Several species of aphides belonging to this class are known to carry on parthenogenetic reproduction throughout winter under suitable conditions. In fact, field observations seem to show that, in certain latitudes, the same species may reproduce principally by continuous parthenogenetic reproduction and the sexual forms may be rare or may not occur at all.

#### CLASS B. MIGRATION FROM TREES OR SHRUBS TO ROOTS OF HERBACEOUS PLANTS

There are only a few species whose migrating cycles have been definitely worked out which can be included in this

class, and with the exception of one species—the dogwood aphis (*Anæcia corni*)—they all belong to the *Pemphigus* group. Börner (1926, *loc. cit.*) gives ten species which on their listed food plants would fall into this class. It is interesting to note that a few migrating members of the tribe Aphidini exhibit semi-subterranean habits on the intermediate food plants, feeding on the superficial roots and root stalks as is the case with the coltsfoot aphis (*Anuraphis farfaræ*).

The general features of this class of migration may be represented by the poplar-lettuce root aphis (*Pemphigus bursarius*) and the dogwood aphis (*Anæcia corni*).

The sequence of the life-cycle is in general similar to those species included in the previous class, but in these cases the host plant plays a more definite rôle. With the dogwood aphis the fertilised eggs are laid on the dogwood, *Cornus sanguinea* (primary host), in autumn and the fundatrices hatch out about end of April or early in May. The fundatrigeniæ generations are composed almost entirely of winged migrants which migrate to certain grasses, e.g. *Holcus lanatus*, on the roots of which the alienicolæ generations develop. These generations usually consist only of apterous individuals, although winged forms may occasionally occur. The alate sexuparæ, or return migrants, return to the dogwood tree about October, on which they produce the small, apterous, sexual forms and fertilised eggs are laid by the sexual females.

With *Pemphigus bursarius* we have a species which has developed the gall-forming habit on the primary host. The fertilised eggs are laid on certain species of *Populus* and the fundatrices, which hatch out in spring, form galls at the base of the leaves. The fundatrigeniæ are produced inside the galls and develop into winged migrants which migrate to various herbaceous plants, *Lactuca*, *Cichorium*, etc., on the roots of which the alienicolæ generations are produced during the summer. These generations usually consist of apterous individuals, but about September winged sexuparæ develop and fly back to the poplar trees on which the small, apterous sexuales are produced. There is evidence that parthenogenetic females of the alienicolæ generations may continue reproduction on the roots of the intermediate food plants throughout winter, as colonies are frequently found on these plants in early spring. It is of interest in this respect to note that there are some species of aphides, which, living entirely on the roots of plants, usually in close association with ants, reproduce only parthenogenetically throughout their life-cycle. The need for a primary host appears to have been lost in these cases, together with the normal bisexual life-cycle, and it may

be that these species represent off-shoots from species having originally migrating habits.

#### CLASS C. MIGRATION FROM TREES OR SHRUBS TO ROOTS OF OTHER TREES OR SHRUBS

In many respects the migrating habits of species which belong to this class resemble those of Series B. It may be, however, that this habit has developed earlier, since herbaceous plants appeared later in the evolution of the world's flora. The life-history of five species belonging to this class have been worked out, of which that of the elm-currant root aphid (*Eriosoma ulmi*) is best known. Another species is the honey-suckle—*Picea* root aphid (*Prociphilus xylostei*). Two other species of *Prociphilus* migrate from the ash (*Fraxinus*) to the roots of certain conifers. The vine *Phylloxera* and woolly apple aphid (*Eriosoma lanigerum*) also have a root-feeding phase of the life-cycle, but these species more strictly belong to the migrating forms included in Class D.

The sequence of the life-cycle in members of Class C does not differ markedly from the general plan. The winged migrants (*fundatrigeniæ migrans*) leave the primary host in spring and fly to the intermediate host, where they produce off-spring near the base of the host plant, which initiate the alienicolæ generations on the roots. After several alienicolæ generations have been passed through, winged sexuparæ develop in late summer and leave the roots of the intermediate host, returning to the primary host, where the apterous sexuales are produced and fertilised eggs laid. There is evidence here, as in some members of Class B, that, with some species, e.g. the elm-currant root aphid, a series of parthenogenetic generations may be continued on the roots of the intermediate host plant throughout winter.

#### CLASS D. MIGRATION FROM TREES OR SHRUBS TO OTHER TREES OR SHRUBS

Excluding the "Chermes" group, the life-cycles of about a dozen species of aphides, which may be included in this class, have been traced, showing that the habit has developed in several groups of the Aphidoidea. *Myzus illinoisensis* migrates from *Viburnum prunifolium* to *Vitis* in N. America; *Prociphilus tessellatus* from *Acer dasycarpus* to *Alnus* in N. America; *Hamamelis spinosus* and *Hormaphis hamamelidis* from *Hamamelis virginica* to *Betula nigra* in N. America. The cosmopolitan woolly apple aphid (*Eriosoma lanigerum*) primarily has a migrating cycle from *Ulmus americana* to the apple in N. America. Two species of *Phylloxera*, namely *P. quercus*

and *P. florentina*, migrate from *Quercus coccifera* and *Q. ilex* in France and Italy respectively to *Q. lanuginosum*. Finally, there is the well-known vine *Phylloxera* which lives on the aerial portion and roots of certain vines. In addition there are several species in the "Chermes" group which all have spruce (*Picea*) as the primary host and migrate to various conifers, *Larix*, *Abies*, etc., as intermediate hosts.

In this class of migration the habit appears to have become more specialised with some species than with others. The host plants play a more definite rôle in the life-cycle of the insect than is the case, for instance, in Class A. In *Hormaphis* and *Hamamelistes* the gall-forming habit has been developed and the sexual forms are small, apterous individuals but have retained functional mouth parts. In the case of the woolly apple aphid, a further development of the migrating habit has taken place, which is complicated by the behaviour of certain of the alienicolæ generations on the intermediate host plant. The sexual forms are small dwarf-like individuals, incapable of feeding, and moreover the functioning ovaries of the females are reduced so that only one fertilised egg is laid. This species has been extensively studied on account of its importance as a pest in apple orchards. Its original home is considered to be the temperate regions of North America, where, according to Baker (1915), it probably lived on *Ulmus* and *Crataegus*. With the introduction of the cultivated apple into those regions, it adopted this host as an intermediate food plant. It is generally thought that the insect was introduced, probably with nursery stock, into Europe, and it is now found as a pest on apple trees in practically every country where they are grown.

Patch in 1912 and 1915 showed that in America there is a definite migration of this species from *Ulmus americana* to apple trees. The fertilised eggs, found on *U. americana*, hatch out in spring and the fundatrix causes a rosette-like curling of the leaves, in which eventually winged migrants are produced, which fly to the apple and there initiate the alienicolæ generations. Eventually in late summer, sexuparæ are produced, which fly back to the elm and produce the sexuales, and fertilised eggs are laid. In France, on the other hand, Marchal (1919 and 1924) has been unable to trace the migration from elm to apple, and concludes that the cycle is purely a parthenogenetic one on the apple in that country. Marchal found that alate sexuparæ may appear and even produce sexuales, but the latter are inoperative, in so far that the fundatrices never developed even when the eggs were laid on *Ulmus americana*. Marchal observed a species on elm in France (*Eriosoma ulmosedens* n. sp.) which closely resembles

in its habits and morphology the elm phase of *E. lanigerum* in America. It is, however, morphologically distinct, and moreover performs the whole of its life-cycle on the elm, being a non-migrating species. Schneider-Orelli and Hans Leuzinger (1926), working in Switzerland, have recently established that winged forms may occur at irregular intervals throughout the summer in the alienicolæ generations on the apple. Certain of these produce apterous, parthenogenetic, viviparous females which carry on the generations on the apple tree. This feature, as we have seen in previous migrating types, affords a means of distribution from one intermediate food plant to another. Certain of the alate forms which were produced were found by these observers to be sexuparæ, in that they produced sexuales, particularly during August and September. The sexuales were, however, inoperative and died out, which observations confirm those of Marchal. We have, therefore, a modification of the normal bisexual life-cycle and a purely parthenogenetic cycle established, in which the alienicolæ generations continue indefinitely on the intermediate host. It would appear, in fact, that in the absence of *Ulmus americana* in Europe, the sexual phase has gradually become inoperative, and although sexuparæ and sexuales may be produced, they have lost the habit of completing the bisexual cycle even in the presence of the original primary host.

On the other hand the work of Theobald (1921), in England, indicates that the elm may be the primary host of *E. lanigerum* in this country. Further investigation appears necessary in order to establish whether there are different races or strains in different countries.

In addition to the parthenogenetic colonies on the aerial portions of the apple tree, a series of root-feeding parthenogenetic generations develop on the roots, and there appears to be an irregular wandering of individuals from the roots to the upper parts of the tree in early summer and a return of certain individuals to the roots in the autumn.

#### THE PHYLLOXERONS

As referred to previously, the Phylloxerons and "Chermes," which form the family Phylloxeridæ, differ fundamentally from members of the family Aphididæ, in that the parthenogenetic females are oviparous and viviparity does not occur. The Phylloxerons differ somewhat in their migrating habits from the species hitherto considered, in that they are associated throughout the life-cycle with the same species of host plant, or at any rate plants which are closely related. Phylloxera of the vine, on account of its economic importance in viticulture, has been investigated by many authorities and its

history is well known. It was in 1855 that Fitch in N. America first discovered an aphid species forming galls on the leaves of wild vines, which he called *Pemphigus vitifolii*. In 1867 Planchon recorded a species on the roots of the vine in Europe which he called *Rhizaphis vastatrix*. It has long been held that the latter species is probably the root-feeding phase of the species found by Fitch, being referred to in the literature as *Phylloxera vastatrix* or *Peritymbia vitifolii*. In any case it seems fairly well established that the original home of the vine *Phylloxera* was N. America, and Davidson (1921) gives its life-history under natural conditions in that country as follows :

The fertilised eggs are laid on the aerial portion of certain vines, and, hatching out in spring, the fundatrix forms a gall on the upper side of the leaf, in which parthenogenetic eggs are laid. Apterous, parthenogenetic, oviparous females develop from these eggs and several broods may be produced (*fundatrigeniæ apteræ*). Certain individuals of the fundatrigeniæ generations go down to the roots of the vine and there initiate a series of generations of apterous, parthenogenetic females which are called the *radicicoles*. From those colonies which remain on the aerial portion of the vine, winged females (*fundatrigeniæ migrans*) are eventually produced, which migrate to other vines. Certain of the radicolle forms may hibernate as larvæ during winter, and completing their development in spring, continue reproduction by means of parthenogenetic eggs. During the spring, some of the radicolle forms may ascend to the aerial parts of the vine. Some may develop into winged sexuparæ and, migrating to other vines, ova are laid from which the sexuales develop.

It was found that on wild forms of *Vitis*, e.g. *V. rupestris* and *V. berlandieri*, the bisexual cycle is the usual one and the hibernating larvæ on the roots (radicicoles) are rare. On *V. labrusca*, both the hibernating radicicoles and the winter-egg stage may occur, while on *V. vinifera* it is the radicolle generations which more commonly occur. In European and Californian vineyards, the radicicoles may carry on parthenogenetic generations (larvæ hibernating during winter) indefinitely, and it is only in the presence of certain American vines that the complete bisexual cycle becomes possible. If sexuparæ are produced on European vines, they are inoperative in the absence of a suitable variety of vine on which the fundatrices, emerging from the fertilised eggs, can develop. The aim of the viticulturist therefore, as a control measure, has been to use vine stocks resistant to the root-feeding forms (radicicoles), such as *Vitis riparia*, *V. rupestris*, *V. berlandieri* and their hybrids, on which to graft suitable commercial varieties of grapes.

Investigations during the past fifteen years in Europe, notably by Börner, show that there are two races of the vine *Phylloxera* in Europe. Börner found that in Lorraine, where *Phylloxera* has been known to exist for about fifty years, the type differs in its habits from that found in Southern France. The Lorraine type does not produce galls on the leaves of the American vines, excepting *Vitis labrusca*, but it does produce galls on the leaves of the European vines (*V. vinifera*). In Southern France, on the other hand, galls are usually produced on the leaves of the American vines *V. riparia*, etc. Furthermore, the Lorraine type of *Phylloxera* differs from the type found in Southern France in its behaviour on the roots of European vines and certain American vines such as *Vitis berlandieri*, *rupestris*, etc. Börner therefore gave the Lorraine race a new name, *pervastatrix*, to distinguish it from the other type, *vastatrix*. It would appear therefore that, either there were originally two races of the vine *Phylloxera* introduced into Europe from America, or that two races have evolved in Europe, owing to climatic and other influences, from the original American strain. Börner favours the former view, and—although it is not certain which form Fitch originally had before him—considers the race from Southern France (*vastatrix*) is probably *P. vitifolii* Fitch, its original food plant in the Missouri region of N. America being *Vitis riparia*. The Lorraine race (*pervastatrix*), on the other hand, is probably Planchon's *R. vastatrix*, its original home being the region of the Alleghany mountains, where it lived on *Vitis labrusca*, a species of vine very near the European type.

It is clear that the question of biological races is of great importance from the practical point of view as well as being a subject of great biological interest.

### THE "CHERMES"

In members of the "Chermes" group, which includes several species belonging to different genera, the migrating habit has reached a more advanced stage of development than in any other members of the Aphidoidea. The normal bisexual cycle occupies two years, but there is the same general plan underlying the sequence of the five, main, polymorphic types as obtains in those migrating aphides we have already discussed. There is, however, a more complex specialisation of form in the parthenogenetic generations on the intermediate host (*alienicolæ* or *colonicæ*). Several European species have been studied by Blochmann, Dreyfus, Cholodkovsky, Nüsslin, Börner, Marchal, and more recently in England by Steven, Speyer, and Chrystal, and the life-histories of about six species

are fairly well established. The primary host is always *Picea* (spruce) and the intermediate hosts are other conifers, such as *Pinus*, *Abies*, *Larix*, and *Pseudotsuga*. The latter are definite and distinct for the various species and appear to play an important and definite rôle in the life-cycle.

Although the sequence of the life-cycle resembles in general plan that of members of the family Aphididæ, certain special features should be noted.

(a) *Fundatrix*.—After hatching out from the fertilised egg in early summer, the fundatrix hibernates on the primary host in an immature stage throughout the following winter, and completing its development the following spring, initiates the *fundatrigeniæ* or *gallicolæ* generations.

(b) *Fundatrigeniæ* (*Gallicolæ* or *Cellaris* generations).—These forms develop in galls on the primary host during spring and early summer. Winged forms develop (*gallicolæ migrans*) and migrate to the intermediate host plants, on which they initiate the parthenogenetic *alienicolæ* or *colonici* generations.

(c) *Alienicolæ* (*Colonici* or *Exsules* generations).—These forms are apterous and hibernate on the intermediate host plant, during the winter, in an immature stage. Development is completed the following spring, and parthenogenetic eggs are laid from which hatch individuals of the first *colonici* generation. In succeeding generations winged forms develop (late spring) which migrate back to the primary host, being the *sexuparæ*.

(d) *Sexuparæ*.—These winged forms, which return to the primary host in late spring or early summer, produce the sexuales on spruce.

(e) *Sexuales*.—These are small, apterous individuals, which are produced on the spruce (primary host) in early summer. The sexual female lays only one egg, and the fundatrices which hatch from these eggs remain immature until the following spring.

This simplified account of the sequence of the various generations does not, however, give a fair idea of the detailed specialisation of form and habit which occurs. The position is complicated by the fact that in some species, certain forms carry on a continuous parthenogenetic cycle on the primary host, fundatrix and *gallicolæ* generations only being represented. Winged forms may occur, but they produce parthenogenetic eggs on the primary host and do not migrate, being called *gallicolæ non-migrans*.

Similarly, the progress of the *alienicolæ* or *colonici* generations on the intermediate host becomes complicated in some species by the development of two different types of individuals, which have a special significance in relation to the biology

of the species. In some cases a continuous parthenogenetic cycle may be continued indefinitely on the intermediate host, as in *Dreyfusia piceæ* Ratz. on *Abies nordmanniana*.

We have seen that somewhat similar relationships occur in migrating members of the family Aphididæ, and it appears probable that these so-called parthenogenetic races have evolved from species having originally a bisexual migrating life-history.

The special features in the migrating life-cycle of members of the "Chermes" group will be better understood by reference to certain species whose life-histories are known.

### 1. THE SPRUCE-PINE "CHERMES" (*Pinus pini* Linn.).

Marchal in France and Chrystal in England have investigated this species, and their observations show that there are two races, one race having a normal, two-year, bisexual cycle, migrating from certain kinds of spruce (*Picea excelsa* and *P. orientalis*) to pine (*Pinus sylvestris*), and the other, carrying on parthenogenetic reproduction indefinitely on the intermediate host (*Pinus*). Sexuparæ may develop in the latter race, but only sexual females develop from their parthenogenetic eggs, and in the absence of males the sexual phase is therefore inoperative.

### 2. THE SPRUCE-LARCH CNAPHALODES (*Cn. strobilobius* Kalt.)

This species has a normal two-year bisexual cycle, migrating from spruce (*Picea excelsa*) to larch (*Larix europæus*). Cholodkovsky, however, found a species on spruce, which does not migrate, but consists of a succession of fundatrix and gallicolæ generations without the development of a sexual phase. This species closely resembles the spruce phase of *Cnaphalodes strobilobius*, and may be a phase of that species or a distinct non-migrating parthenogenetic race on the primary host.

### 3. THE SPRUCE-LARCH CHERMES.

It is evident from the researches of Cholodkovsky and Börner, that we have to deal in this case also with a complication of at least two closely allied races or species. Ratzeburg described a species (*Chermes viridis*) having a normal two-year bisexual cycle, and migrating from spruce to larch. Kaltenbach records a species from spruce, on which host it is known to have a purely parthenogenetic cycle, consisting only of fundatrix and gallicolæ non-migrans generations. This species is known as *Ch. abietis* Kalt. Börner considers that Kaltenbach's species is the spruce phase of Ratzeburg's migrating species "*viridis*," and both should be considered as *Ch.*

*abietis* Linn. From Cholodkovsky's observations, however, it appears that we have evidence here of the development of parthenogenetic races from migrating species, resulting in complication of the life-history by the introduction of secondary cycles. This author showed that "*abietis*" of Kaltenbach in Switzerland differed somewhat from "*abietis*" Kalt. in Russia, and moreover that there are actually three species of this type in Europe: (a) *Ch. viridis* Ratz., which has the normal bisexual, migratory cycle; (b) "*abietis*" of Kaltenbach, which occurs over Northern and Eastern Europe, only on spruce; (c) "*occidentalis*" of Cholodkovsky, which occurs in Western Europe, having a migrating bisexual cycle, like "*viridis*" of Ratzeburg, from spruce to larch, as well as a non-migrating phase on spruce.

#### 4. THE SPRUCE-DOUGLAS FIR "CHERMES" (*Gillettea cooleyi* Gill.).

This species, first described by Gillette in 1907, has been investigated by Chrystal in N. America, and the evidence shows that in that continent it has a two-year life-cycle, migrating from spruce, on which galls are produced, to the Douglas fir (*Pseudotsuga douglasii*). Chrystal found that the life-cycle in Britain consists only of the colonici generations on the Douglas fir (*Pseudotsuga*). Sexuparae may be produced, but the sexual forms appear to be inoperative and the gall-forming fundatrix and gallicolae generations on spruce, so far as we know, do not occur in Britain.

#### 5. THE SPRUCE-ABIES "CHERMES" (*Dreyfusia nüsslini* Börner).

This species has a normal two-year bisexual cycle migrating from *Picea orientalis* to *Abies pectinata*. Certain individuals of the colonici generations may, however, continue parthenogenetically indefinitely on the intermediate host (*Abies*). It has also been observed that certain of the winged forms, produced in the gallicolae generations on the spruce, do not migrate (*gallicolae non-migrans*) and die without producing eggs.

There is another species *Dreyfusia piceae* (Ratz.), found only on *Abies*, particularly *A. nordmanniana*, which is entirely parthenogenetic. It was long considered as the *Abies* phase of *D. nüsslini*, but the observations of Cholodkovsky, Nüsslin, and Marchal showed the two species to be distinct, and they were definitely separated by Börner in 1908. The parthenogenetic generations in *D. piceae* are usually apterous, but the observations of Marchal in France show that non-migrating winged forms (*colonici non-migrans*) may occur. The sexual cycle, however, would appear to have been entirely suppressed.

Nüsslin in 1910 produced evidence that sexuparæ may occur, but that the sexuales produced by them fail to develop. He considered that this species is probably derived from a normal bisexual one owing to modification of its habits, since it lives on the trunks and larger branches of *Abies*, whereas normal winged sexuparæ are only produced by the species feeding on the needles.

We see therefore that, in the "Chermes" group, some species have either lost the sexual phase of the life-cycle or it has become inoperative and reproduction is entirely parthenogenetic. In some cases the parthenogenetic generations occur only on the primary host, consisting of fundatrix and gallicolæ (fundatrigeniæ) generations. In others they occur on the intermediate host (*Abies*, *Larix*, *Pinus*) and consist only of colonici (alienicolæ) generations, succeeding one another indefinitely, as in *Dreyfusia piceæ* on *Abies*. The parthenogenetic generations do not always consist exclusively of apterous individuals; winged forms may occur which, however, appear to have lost the migrating tendency (*gallicolæ non-migrans* and *colonici non-migrans*), and in fact winged sexuparæ may occur and even migrate, but the sexuales produced by them die out and are inoperative.

The problems associated with the occurrence of parthenogenetic species and biological races are of great biological interest. Are we to consider that, while some species may continue parthenogenetic reproduction indefinitely under certain environmental conditions, the sexual phase is only latent and will appear under the influence of proper conditions of climate, food, etc., or have the parthenogenetic species or races evolved at the expense of species originally bisexual, the sexual phase having been permanently eliminated?

In Part II of this paper some general and theoretical considerations regarding the migration of aphides will be discussed.

## THE NEWEST VITAMIN

By WALTER P. KENNEDY, PH.D., B.Sc.

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SOME twenty years ago Prof. F. G. Hopkins made the first clear statement of the existence of what he called accessory food substances, which he later backed up with conclusive evidence. He found that these substances were necessary adjuncts to a diet, and that their absence resulted in various pathological conditions. Casimir Funk, a pioneer in this field, which has become one of the most extensive in modern biological research, introduced the term *vitamine* to designate these accessory substances, but at the suggestion of J. C. Drummond, the final "e" was dropped to avoid the implication that they were necessarily nitrogenous bases. The vitamins, which have justly aroused considerable interest both in the scientific and lay public, even to the extent of figuring largely in advertisements, are four in number; namely, fat-soluble A, the growth vitamin; water-soluble B, which gives protection against beri-beri and certain other pathological conditions; water-soluble C, the anti-scorbutic substance; and fat-soluble D, the antirachitic.

Several workers have shown that a deficiency of either A or B results in total or partial sterility in both male and female animals, but as yet no evidence has appeared regarding the effect of vitamin C on reproduction. The antirachitic substance D has very similar properties and natural distribution to fat-soluble A, and so far no distinction has been drawn with regard to its effects in this respect. As however it has been shown that a dietary deficiency of calcium diminishes fertility even to the point of producing complete sterility, it is very probable that a deficiency of D has a like effect, owing to its influence on calcium metabolism.

In 1922 H. M. Evans and K. S. Bishop, of the University of California, published the results of their researches on reproduction in rats which were maintained on purified rations. The rations appeared to be adequate in every way, and consisted of casein (protein), corn starch (carbohydrate), lard (fat), milk fat (supplying vitamin A), dried yeast (supplying vitamin B), and inorganic salts. The rat does not suffer from an

absence of vitamin C. This basic diet, which contained all the known essentials for normal metabolism, maintained the animals in perfect health and activity, and resulted in growth which compared very favourably with that which is described as normal for the species on a natural dietary. Nevertheless reproduction was not normal, for the animals were usually sterile, although a few second generation females were obtained; these were invariably sterile. On searching for a reason for the sterility, Evans and Bishop found that with some animals a slight interference had occurred in the incidence of sexual maturity and in the regularity of the sexual cycle. This was not sufficient to explain the results, and further investigation showed that a peculiar disease attacked the developing embryo and the placenta (which forms the link between the embryo and the mother) resulting in their death and absorption, for abortion did not occur. The onus of responsibility was removed from all the known vitamins and other components of the diet, since additional quantities of these did not effect a cure. Rats of the same stock, however, were exhibiting normal fertility on other diets, so the addition of other food substances was tried, and with some of these a cure was obtained. The efficacious foods were fresh or dried lettuce leaves, wheat embryo, oats, alfalfa, egg yolk, ox cheek, ox liver, and milk fat, and it was assumed that these substances contained a vitamin complex which was a necessary factor for the maintenance of placental integrity, and hence the life of the developing embryo. It may be noted that the content of this substance in milk fat is small, and that the milk fat fed in the basic diet was insufficient to remedy the disease. This will be referred to again further on.

The vitamin was named X by the discoverers, but more recently it has been designated E to bring it into line with those already known. They described it as being resistant to ordinary cooking temperatures, and this has been confirmed, but probably the presence of oxygen would diminish the resistance to heat. It is insoluble in water, but soluble in alcohol, ether, benzene, and acetone (Barnett Sure). Hence it is a fat-soluble vitamin like A and D, *i.e.* it exists in the foods mentioned in close association to their fatty constituents. Recently it has been shown by Sure to be present in cotton seed, palm, corn, hemp, peach kernel, pea nut, soy bean, and olive oils.

The effect of this vitamin deficiency on the pregnant female has been confirmed by numerous workers, some of whom have also tested its action on the male. Rather surprisingly it has been found by H. A. Mattill and M. M. Clayton, and by K. E. Mason, that vitamin E deficiency results in a progressive

degeneration of the testicle, depending on the duration of the vitamin deprivation. This result, which has been confirmed by H. M. Evans, and by W. P. Kennedy, is a little unexpected, for the action here is on the gonadal, or primary germ tissue, while in the female the gonadal tissue—the ovary, which is the homologue of the testicle, is almost unaffected, the site of the damage being the placenta. Another important sex difference is that while female animals deprived of vitamin E can recover their fertility if suitable foods are administered, this is not the case with the male unless the period of deprivation has been so short that the degeneration has not affected the whole testis. In the early stages some of the tubules which make up the gland are attacked, while others are unaffected: in such cases fertility may be restored; later however, all the tubules exhibit pathological states, and then the animal is permanently sterile. In the case of the female the function of the ovary may be in some measure impaired, as evidenced by the irregularity of the œstrus or sexual cycle, but the structure of the organ is not altered.

It is well known that the œstrus cycle manifests itself in two ways, namely, by changes in the ovary, which essentially consist of the growth and shedding of the egg, and by changes in the uterus, which are periodic alterations in the lining layer which prepare the organ for the reception of the egg, should it chance to be fertilised. The influence of the ovary upon the uterus is easily demonstrated, and is universally accepted, but the reciprocal action of the uterus upon the ovary is not so well established. A certain number of workers have asserted that there is such an action, but it has also been denied by as many, and the question may be said to be undecided. The influence of vitamin E is on the placenta, which is in part developed from the maternal tissues, and in part from the embryonic, and vitamin starvation only becomes markedly apparent when the placenta is present, *i.e.* in pregnancy, although in the non-pregnant animal œstrus or heat is irregular under such a regime. The fact that eggs are produced which are capable of fertilisation and development, however, may be taken as an indication of the relative normality of the ovary and the egg.

H. M. Evans and G. O. Burr have stated that only the foetal parts of the placenta suffer structurally. This may be due to a specific effect on these tissues, or to an effect on the maternal portion of the organ which interferes with the normal nutrition of the foetal part, and with the foetus itself. In similar foetal absorption described by C. H. N. Long and A. S. Parkes, the destructive processes begin in the liver of the embryo and spread, affecting the placenta last. The

latter structure is in the best position for obtaining nutriment, but whether or no this is the deciding factor is still an open question. Vitamin E may then be directly necessary for normal uterine functioning, and may not affect the ovary, unless indirectly through the hypothetical mechanism of the effect of the uterus upon it.

A second hypothesis is possible, however, namely that the accessory substance is a requisite for complete ovarian function, which also depends on the activity of the corpus luteum, or yellow body. This corpus luteum is a peculiar gland tissue which forms in the pocket or follicle, left by the egg when it is shed, and which has begun to atrophy before the next cycle culminates. After a long and extensive controversy it is now accepted that this structure is an endocrine organ, or gland of internal secretion, that is, in the same class as the thyroid and adrenal glands; and that it elaborates a hormone, one of the functions of which is to "sensitise" the uterus, and allow of the implantation of the fertilised egg. If this body is removed or injured in the early stages of pregnancy, the embryo dies and is absorbed, owing to a failure of the placenta to maintain the normal supply of nutriment. It is within the bounds of possibility to suggest that vitamin E is necessary for the complete and normal production of the hormone of the corpus luteum, and that in its absence the implantation process fails just as in the case of the operative removal of the gland.

These hypotheses do not, of course, exhaust the possibilities, but they serve as signposts for further research, and may lead to far-reaching conclusions regarding the physiology of reproduction, and to knowledge which may have considerable practical bearing on the important problem of the sterility which costs the farmer and the country so many thousands of pounds every year. In passing, it may be remarked that this loss is much more serious than is usually supposed, for the maintenance of a breeding animal which is not earning its keep means the waste of food, labour, and housing space; but, in addition, the incidence of sterility diminishes the natural increase of real capital with all its concomitant advantages.

In many experiments on the relation of diet to reproduction it has been found frequently that a ration has supported growth, and the normal production of young, but only a very few of the progeny have been reared owing to high mortality. This failure to rear appears in most cases to be due to a deficiency of lactation, either quantitative or qualitative. It must be remembered that lactation places a far greater strain on the mother than does pregnancy, for not only has she to supply

more food to meet the demands made by the greater growth of her young, but she has also to supply food to meet their energy requirements, which, of course, are minimal in the uterus, if present at all. Hence if a ration is on the borderline for the production of normal growth, health, and young, it is easily seen how it may fail to be adequate for rearing the progeny to the stage of weaning. The deficiency of such borderline rations may be due to almost any of the constituents of the diet, such as protein, which must not only be present in sufficient amount, but must also be "good" protein, that is, it must contain the necessary eighteen or twenty amino acids in its composition, which are essential for normal metabolism in the animal. A vitamin deficiency in the diet of the mother may affect her lactation capabilities, and may, in addition, produce definite deficiency disease symptoms in the young, for the vitamins cannot be synthesised in the body of the animal, nor stored for long periods. Such a case is seen in suckling in infants of mothers suffering from beri-beri, for they also develop the disease.

The case of vitamin E appears to be even more complicated. Recent work of Barnett Sure has shown that the unsaponifiable fraction of wheat oil contains E in high concentration, and apparently in association with another active principle. The vitamin complex contains a lactation promoting factor as well as an anti-sterility one, the former being relatively easily destroyed by heat while the latter is relatively resistant. Their ability to be stored in the body also appears to be different. This work is in a preliminary stage at present, and we must await further investigations to discover if the two factors of the complex are separable, or if they can be found apart in natural sources. Other workers have found that minimal doses of E which allowed of the completion of pregnancy, did not support the subsequent rearing of the young, and as the vitamin was obtained from different sources, it still remains to be settled whether this is due to the anti-sterility factor or to yet another, a lactation promoting factor. We have here, perhaps, the foreshadowing of a vitamin F.

As was to be expected, certain opposition has been raised to the necessity for postulating a new vitamin requisite for reproduction, and attempts have been made to explain the phenomena in known terms. This very proper attitude is valuable, if not indeed essential for the stabilisation of a hypothesis, for the more criticism falls short of its objective, the stronger does the position of a new theory become. Some of the critics of the new vitamin used butter fat and yeast in their diets, which were supposed to be E free. Butter fat contains the vitamin, however, though in varying amounts,

depending on the food of the cows from which it was obtained. Milk is a very variable food, and differs not only according to the fodder and the season, but also according to the breed of the cattle. While the amount of vitamin E in butter fat is not great, the results of several investigators show that it must be adequate for the rat under certain conditions. These, in part at least, may be determined by other constituents of the ration, for several complicated relationships have been shown to exist between the various dietary components; for example, the proportion of carbohydrate determines the amount of vitamin B required. It may also be determined by the disintegrative processes of digestion and bacterial destruction in the intestine, which may influence the amount of E absorbed. These questions still await solution. In some rations butter fat is used as a source of vitamin A, and apparently a quantity which supplies an adequate amount of A may not contain enough E, which would explain some of the discrepancies between different reports. Certain investigations also suggest that yeast may contain something in the nature of vitamin E.

While, as has been indicated, much is yet obscure, we can say with some confidence that a vitamin complex exists which is necessary for normal reproduction in rodents. So far it has not been demonstrated that other genera are so affected by a deficiency of the vitamin, and while they may not be, there are certain suggestive facts that point to the possibility that other animal forms will come into line with the rodents. The problems at least are worthy of complete investigation, and perhaps it is not too vain to hope that some of Nature's valued secrets may be disclosed on the quest.

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## POPULAR SCIENCE

### OUR TOTAL SOLAR ECLIPSE

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FROM the astronomical point of view the year 1927 is of exceptional importance to the general public, for on June 29 there will be an opportunity of seeing from this country a total eclipse of the sun—the first for more than two hundred years.

In its monthly journey round the earth, the moon comes into the same direction as the sun at the time of new moon, into the opposite direction at the time of full moon a fortnight later, and into intermediate directions at other times.

If the earth-moon plane coincided with that of the ecliptic (the name we give to the earth-sun plane), the moon would come between the earth and the sun each time it were new and an eclipse of the sun would be observed, for the moon would then be projected against the brilliant solar background and would appear to cut out a piece of it. Under the same conditions, each time full moon came round, our satellite would plunge into the shadow of the earth and a lunar eclipse would be observed.

Actually, however, the earth-moon plane is inclined to that of the ecliptic at an angle of about five degrees, and the consequence is that, when the moon is in the direction of the sun, it usually passes above or below the brighter body and nothing unusual is noticed. Twice a year, however, new moon occurs when our satellite is in, or very close to, the earth-sun plane, at or near the points where it is cut by the lunar path—the so-called “nodes”<sup>1</sup>—and on these occasions a solar eclipse is seen. It follows from this that the sun must necessarily be eclipsed twice at least in every year.

When the moon is nearly, but not quite, in the same line as the sun, it does not obscure it all, and only a partial eclipse takes place. Even when it is exactly in line it may not hide the whole of the solar disc, for the distances of the two bodies

<sup>1</sup> For a fuller discussion of the meaning of “nodes,” though planetary instead of lunar, see SCIENCE PROGRESS, No. 63 (January 1922), pages 427 *et seq.*

from the earth may happen to be such that the moon appears to be slightly smaller than the sun and thus is projected only against the central parts of the disc, the outer parts of which form a ring of light all around it ; in this case the eclipse is called "annular." But when the moon is somewhat nearer the earth it is large enough to cover the sun completely and then the eclipse is total.

That both total and annular eclipses are possible is one of the most curious facts of nature ; it is due to the fortunate

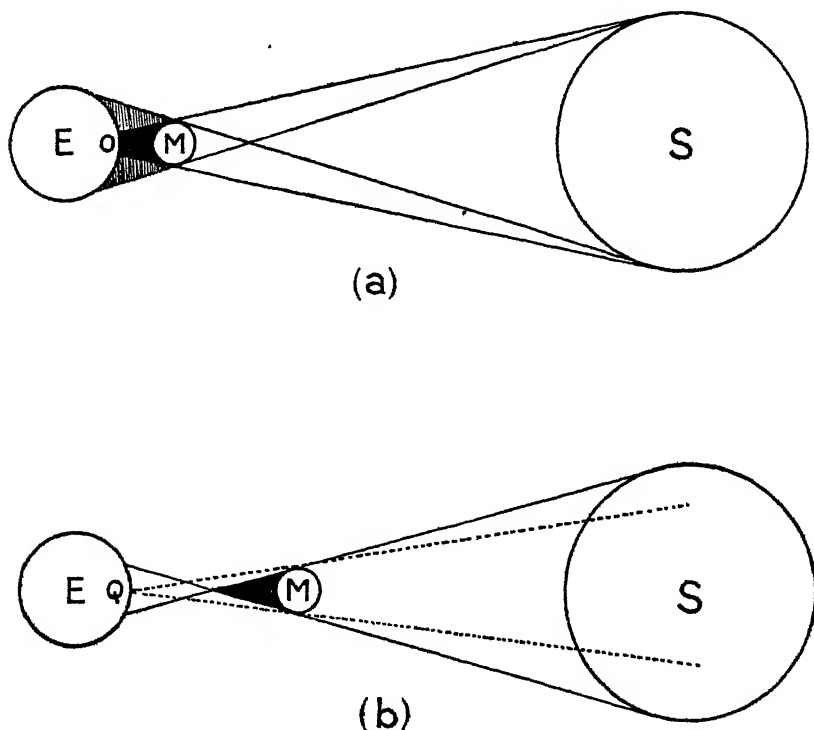


FIG. 1.—Conditions for (a) a Total, (b) an Annular Eclipse of the Sun.

coincidence that the apparent sizes of the sun and moon are practically the same. The sun is almost four hundred times further away from us than is our satellite, but its diameter is also nearly four hundred times greater, so that one body appears to us to be as large as the other. To these circumstances we owe our ability to see both types of eclipse, for, as the sun and moon run through the small changes in their respective distances from the earth, their apparent sizes vary a little, and either may appear to be larger than the other at any particular time, though the preponderance will be but slight.

The geometrical conditions existing at the time of an eclipse of the sun are shown in Fig. 1, which, needless to say, is not drawn to scale. The sun, S, causes the moon, M, to throw its shadow on the earth, E, the dark portion being the "umbra" and the lighter the "penumbra." To an observer within the former the sun would appear to be totally eclipsed, but to an observer within the latter it would appear only partially so. The slight variation necessary for an annular eclipse is shown in Fig. 1 (b), in which the dotted lines have been drawn to indicate why the ring would be seen, for the part of the sun included in the cone bounded by these lines would not be visible to an observer at Q, but the part beyond would be.

During an eclipse, as at all other times, the moon and the earth are moving relatively to each other and to the sun, and the earth is, of course, rotating all the while; the result of these movements is that the small circular patch, marked O in Fig. 1 (a), formed by the shadow on the earth, travels along the surface of the latter and appears to trace out a path called the "shadow-track" or the "track of totality." These tracks are long, but comparatively narrow, they often run about one-third of the way round the earth, as is shown by those illustrated in Fig. 4. From a study of that diagram it will be evident that the chance that such a narrow path will fall on any given place and, therefore, that a total eclipse will be seen from that place, is rather remote, and no surprise can be felt that eclipses occur so infrequently in given localities.

Taking the British Isles as a whole, a vast area if compared with a single town, ten tracks only have crossed any part of them since the time of William the Conqueror, an average of just over one per century, though in the two hundred years before the Conquest three others came. These eclipses have been investigated by Maguire in vol. xlv of the *Monthly Notices of the Royal Astronomical Society*, in which he gives the widths of the shadow in miles and the durations of totality as follow:

Date.	Width. Miles.	Duration. Mins. Secs.
878 October 29	168	2 13
885 June 16 .	196	4 55
1023 January 24	130	2 24
1133 August 2	154	4 34
1140 March 20.	132	3 26
1185 May 1	154	4 33
1330 July 16	42	0 56
1424 June 26	168	4 14
1433 June 17	194	4 27
1598 March 7	84	1 34
1652 April 8	122	3 0
1715 May 3	184	4 0
1724 May 22	136	48

The actual occurrence of the events has not at all conformed to the average stated above, for the first two given in the table are separated only by seven years, as also are the fourth and fifth, with the sixth not far off—the twelfth century was abnormally rich—whilst those of both the fifteenth and eighteenth centuries are separated by nine years. It is, however, scarcely fair to analyse the list in this way, for, from the non-technical point of view adopted in a general discussion like the present, it is largely a matter of "chance," depending on the exact co-ordination of the many circumstances of the moment, whether a particular track of totality happens just to enter or just to miss a given area. But of the paths of these four "pairs" of eclipses which actually did enter our Islands, the constituents of the first pair intersected in north-west Ireland, those of the second pair in the North Sea, just reaching Scarborough, those of the third in the extreme north of Scotland, and those of the fourth in south-west England, the central lines cutting at the village of Bishops' Lydeard, near Taunton, so that the inhabitants of these four parts of the country might have seen, twice in a short time, a sight which draws astronomers to the remotest parts of the globe, a sight which has never yet been revealed to any untravelled Britisher now alive! Those parts of the 1715 and 1724 tracks which crossed our own islands are shown in Fig. 2.

An examination of the thirteen eclipses in the above table shows that two of the shadow paths, those of 878 and 1715, passed over London, two over Dublin, and five over Edinburgh; and every spot in the British Isles, with the exception of a small area at Dingle on the west coast of Ireland, has been covered at least once by the moon's shadow.

The track of totality in the coming eclipse of next June begins in the Atlantic Ocean at a point off the south-west coast of Ireland, passes across the northern parts of Wales and England to Norway, skirts the northern coast of Asia, and finally crosses the extreme north-east of Siberia into the northern Pacific Ocean. This path is traced roughly among the others in Fig. 4. Its course through Britain is illustrated in Fig. 3, which is drawn on a larger scale than that used in the previous diagram (Fig. 2), for the path is considerably narrower than those of the eighteenth-century eclipses, being only about twenty-six miles wide. This map, which is based on the excellent chart in the Handbook of the British Astronomical Association, was recently published in *The Times* to illustrate an article by Professor H. H. Turner, and is reproduced in these pages by permission of all concerned.

The map almost explains itself. The total eclipse will be seen at all places within the darker lines, weather permitting,

and will be visible longest at places on the central line. The white numbers in the black spaces show how long totality will last, 22.7 seconds at Southport, for instance; and the hours, minutes, and seconds above the top continuous line indicate the Greenwich Mean Time at the middle of the eclipse. Adding an hour for summer time, which will then be in use, we find

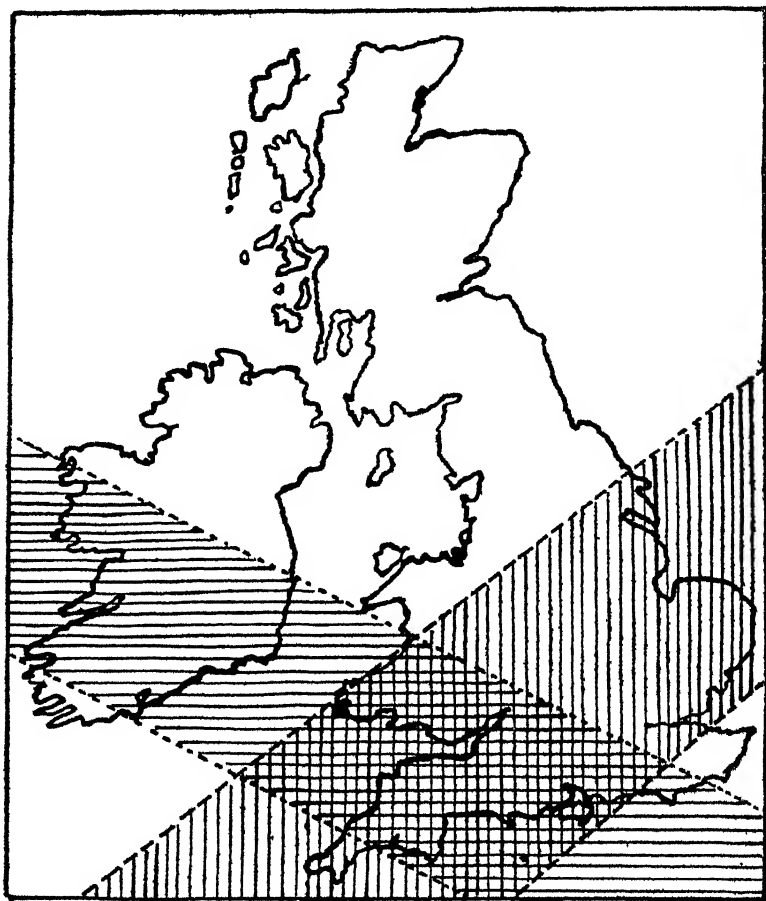


FIG. 2.—Tracks of Totality, 1715 (vertical shading) and 1724 (horizontal shading).

that the time for Darlington, for example, will be just before twenty-five minutes past six in the morning; totality will continue there for a little more than twenty-four seconds, a lamentably short time.

Eclipses fall into groups or series, often known as "families." The constituent members of a family are separated from each



track will not fall on the same part of the earth as did the first, but will be about 120 degrees further to the west. The third will be another 120 degrees beyond the second, and, therefore, two-thirds of the way round the earth as compared with the first. The fourth will have moved three-thirds, or all, of the way round, thus returning to the same part of the earth as the first, and the same thing applies to the seventh, tenth, and

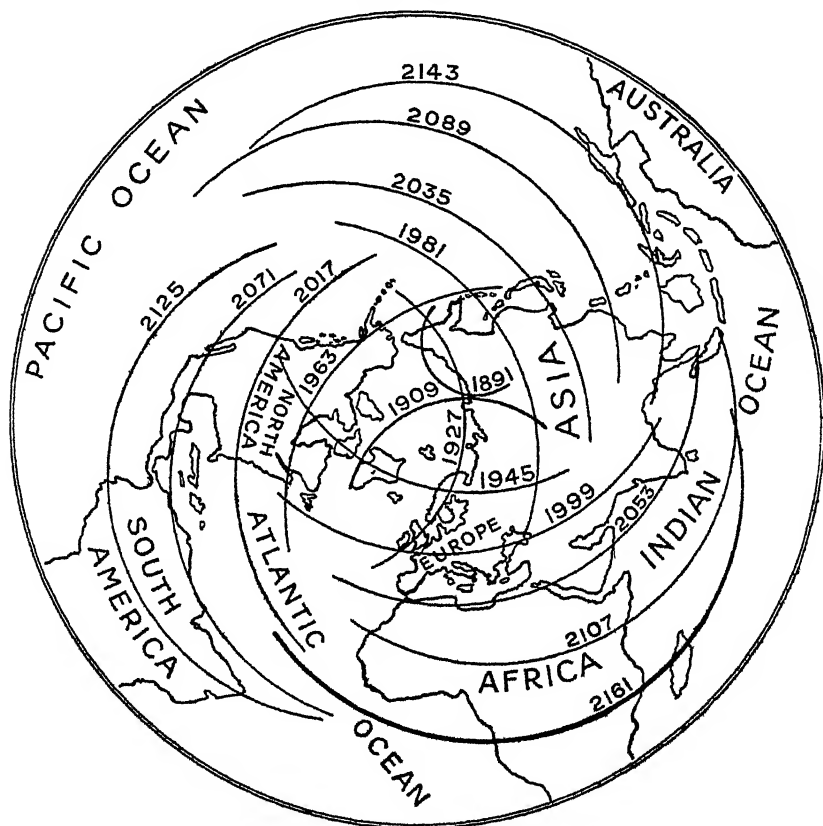


FIG. 4.—A Family of Eclipse Tracks.

subsequent third eclipses. But the tracks do not overlap, for half the families are moving north and half are moving south, and the individual members are displaced accordingly. Fig. 4 shows this clearly for a south-moving family—that to which our own eclipse of 1927 belongs—though it shows only part of its life-history. In designing this diagram the writer has had recourse to the data in Oppolzer's *Canon der Finsternisse*, and has used the same projection, the defects of which were illustrated in a recent number of SCIENCE PROGRESS.<sup>1</sup>

<sup>1</sup> SCIENCE PROGRESS, No. 82 (October, 1926), p. 300.

The life-history of a family extends over a period of nearly thirteen hundred years, the first member being "born" when its penumbra just grazes the earth near one of the Poles. Then come a dozen or more partial eclipses, each larger than the one which came before it, and these are followed by forty or fifty total or annular ones, the number of annular being 50 per cent. more than the number of total. By this time the family has reached the other Pole and the central line leaves the earth. A dozen or more partial eclipses follow, getting smaller and smaller, and with the passing of the last the family becomes extinct. Eighteen years later a new family is born and the total number kept constant.

The family to which our own eclipse of 1927 belongs was born in high northern latitudes on January 4, 1639, back in the days of Charles I. It was then a partial eclipse and was followed by thirteen other partial eclipses, the dates (including the first) being—

1639 January 4	1765 March 21
1657 " 14	1783 April 1
1675 " 25	1801 " 13
1693 February 5	1819 " 24
1711 " 17	1837 May 4
1729 " 27	1855 " 16
1747 March 11	1873 " 26

The next eclipse of the series was total in some places and annular in others; it took place on June 6, 1891, and is the first shown in Fig. 4, where it stands out clearly as the short track near the centre of the diagram. The diagram then traces the history of the family for the next 250 years, and if the dates be examined in the subjoined table the Saros period stands out very clearly, as it also does in the previous list. When the Saros contains five leap years, the 6,585 days represent eighteen years and ten days, but the latter become eleven days when there are only four leap years, and twelve when only three. The dates of the eclipses of which the tracks are traced in Fig. 4 are:

1891 June 6	2035 September 2
1909 " 17	2053 " 12
1927 " 29	2071 " 23
1945 July 9	2089 October 4
1963 " 20	2107 " 16
1981 " 31	2125 " 26
1999 August 11	2143 November 7
2017 " 21	2161 " 17

The last eclipse in this list is also the last given by Oppolzer in his monumental work ; its track is traced slightly more heavily than those of the others in Fig. 4.

At any one time twelve eclipse families are in the total stage, six of them moving north and six moving south. Since these are spread over a period of eighteen years, after which they merely repeat themselves, it follows that a total eclipse cannot occur every year. The way in which they do actually occur can be illustrated from the following list, which includes the next twelve eclipses, *i.e.* one member of each family, and it will be seen that they are spread fairly evenly over the period covered by the table :

Date.	Where visible.
1927 June 29 .	Britain, Norway, etc.
1929 May 9	Indian Ocean.
1930 October 21	Pacific Ocean.
1932 August 31	Polar Regions and Canada.
1934 February 14	Pacific Ocean.
1936 June 19 .	Greece, Siberia.
1937 June 8 .	Pacific Ocean, South America.
1938 May 29 .	Southern Hemisphere.
1940 October 1	South America, South Africa.
1941 September 21	Asia, Pacific Ocean.
1943 February 4	Japan, North-west Canada.
1944 January 25	South America, West Africa.

In order to get the approximate dates of the next set we have to add the Saros. Adding 18 years 10 days (for we have five leap years) to the first of these eclipses, that due at the end of June, we have 1945, July 9, on which day an eclipse track will run across Canada, Greenland, Scandinavia, and Russia, as shown in Fig. 4. The next eclipse visible in Britain, that of 1999, happens to be shown on the same diagram ; it will graze the extreme south-west of Cornwall.

The family which includes 1937 is remarkable for the long duration of the total phase of the eclipse. This particular one lasts more than seven minutes, and its successor in 1955, June 20, will be only about twenty seconds short of the absolute maximum of seven minutes thirty seconds. The 1934 eclipse is not particularly long, lasting less than three minutes, but its family increases to a longer duration than even the 1937 series, and reaches in 2150, June 25, a duration of seven minutes sixteen seconds, and at its return in 2168, July 5, a length of seven minutes twenty-eight seconds, the longest known in historical times.

Both these eclipses take place near midsummer, as also do those of 1937 and 1955. Other things being equal, summer eclipses are more prolonged than winter ones, for in our northern summer the earth is at its farthest distance from the sun, so that the latter appears as small as possible and hence can be concealed by the moon for a greater time.

The Saros is not the only important period of eclipse recurrence, though it is the fundamental one and governs the grouping into families. Newcomb found a period of about 10,572 days, which is 20·3 days short of twenty-nine years. The eclipses of this period are taken from different families and so differ greatly in characteristics. Total eclipses may be followed by annular and annular by total, though every third, forming an 87-year period, less 61 days, is of the same kind. The following eclipses illustrate these points :

1915 February 14	Annular
1944 January 25	Total
1973 January 4	Annular
2001 December 14	Annular
2030 November 25	Total
2059 November 5	Annular

If we consider eighteen of these 29-year periods, or, of course, six of the 87-year periods, the 20·3 days short of the year accumulate to 18 times 20·3 = 365·4 days, almost exactly a year, so that after eighteen of the 29-year periods, *i.e.* after 521 years, eclipses occur on the same day of the month.

To illustrate this point, let us start from the historical eclipse of Nineveh, which took place on June 15 in the year 763 B.C. and about which something further will be said later, and let us add 521 years a number of times. We find that eclipses occurred on

B.C. 763 June 15	A.D. 801 June 15
242 „ 15	1322 „ 15
A.D. 280 „ 15	1843 „ 27 (New Style),

the last date being equivalent to June 15, if converted into Old Style.

At first sight it might seem that a mistake had been made in adding 521 to the second date in the above table to get the third, but it must be remembered that the year 763 B.C. is called - 762 by astronomers and 242 B.C. is - 241, so that the addition or subtraction of numbers may be made by the ordinary rules. The reason for this apparent muddle is that the nomenclature of historians is mathematically incorrect, for they have no year 0 ; they pass directly from 1 B.C. to A.D. 1. If we tabulate the critical years the point becomes obvious, thus :

Historical Year	B.C. 3	corresponds to	Scientific Year	- 2
„	2	„	„	- 1
„	1	„	„	0
„	A.D. 1	„	„	1
„	2	„	„	2

Computers have also to allow for the change of date from the Old Syle to the New, as has already been implied, a change made by Pope Gregory XIII in 1582, but not adopted in England until 1752, when the calendar was eleven days slow. September 3 of that year was called September 14. Hence the popular election cries :

Give us back our eleven days !

and—

In seventeen hundred fifty-three

The year was changed to popery !

the latter referring to the alteration of New Year's Day from March 25 to January 1, 1753.

The eclipse of Thales, 585 B.C., May 28, is the most celebrated in history. Herodotus (i, 74) says : " There was war between the Lydians and the Medes for five years ; each won many victories from the other, and once they fought a battle by night. They were still warring with equal success, when it chanced, at an encounter which happened in the sixth year, that during the battle the day was turned into night. Thales of Miletus had foretold this loss of daylight to the Ionians, fixing it within the year in which the change did indeed happen. So when the Lydians and Medes saw the day turned to night they ceased from fighting and both were the more zealous to make peace."

This eclipse is also referred to by Pliny and by Cicero. It may possibly have been witnessed by Ezekiel, and may account for verses 7 and 8 of his thirty-second chapter : " And when I shall put thee out, I will cover the heaven, and make the stars thereof dark ; I will cover the sun with a cloud, and the moon shall not give her light. All the bright lights of heaven will I make dark over thee, and set darkness upon thy land, saith the Lord God."

The first eclipse verified with certainty is the eclipse of Nineveh, which took place, as has already been stated, on June 15, 763 B.C. Nineveh was probably just outside the path of totality, but a very great obscuration occurred there at 9.47 a.m. This eclipse is recorded on Assyrian tablets, and was interpreted in 1867 by Sir H. C. Rawlinson as " In the Eponymy of Burgasole, Governor of Gozan, a revolt in Assur took place in the month Sivar, and the sun was eclipsed."

It will be appropriate to close this article by giving some description of one of the most curious phenomena visible during an eclipse, that known as " Baily's Beads." If a bright body and a dark one be placed side by side, the former seems to

intrude upon the latter. In connection with eclipses, this gives rise to a curious effect by which the edges of the sun and moon appear to cling together and an arc of luminous points is seen. This was observed by Francis Baily at the annular eclipse of 1836, May 15, and described by him in volume x of the *Memoirs of the Royal Astronomical Society* so vividly that the phenomenon has since been known by his name. He writes most graphically of what he saw: "When the cusps of the sun were about  $40^\circ$  asunder, a row of lucid points, like a string of bright beads, irregular in size and distance from each other, *suddenly* formed round that part of the circumference of the moon that was about to enter on the sun's disc. Its formation, indeed, was so rapid that it presented the appearance of having been caused by the ignition of a fine train of gunpowder. Finally, as the moon pursued her course, the dark intervening spaces (which, at their origin, had the appearance of lunar mountains in high relief and which still continued attached to the sun's border) were stretched out into long, black, thick, parallel lines, joining the limbs of the sun and moon; when all at once they *suddenly* gave way, and left the circumference of the sun and moon in those points as in the rest, comparatively smooth and circular, and the moon perceptibly advanced on the face of the sun."

This observation of the "beads" stimulated interest in eclipses, and several astronomers repaired to Central and Southern Europe six years later for the total eclipse of July 8, 1842. Not only was their trouble well repaid, but a new and unexpected marvel was revealed, the corona and prominences being seen, just as many people may expect to see them on June 29 next.

Baily had set up his apparatus in one of the upper rooms in the University of Pavia, and was intent on looking at his "beads" again, when (to quote from the *Memoirs*, volume xv), he was "astounded by a tremendous burst of applause from the streets below, and at the same moment was electrified at the sight of one of the most brilliant and splendid phenomena that can well be imagined. For at that instant the dark body of the moon was suddenly surrounded with a corona, or kind of bright glory similar in shape and relative magnitude to that which painters draw round the heads of saints. . . . Pavia contains many thousand inhabitants, the majority of whom were, at this early hour, walking about the streets and squares or looking out of windows, in order to witness the long-talked-of phenomenon, and when the total obscuration took place, which was *instantaneous*, there was a universal shout from every observer, which 'made the welkin ring,' and, for the moment, withdrew my attention from the

object with which I was immediately occupied. I had, indeed, anticipated the appearance of a luminous circle round the moon during the time of total obscurity, but I did not expect, from any of the accounts of preceding eclipses that I had read, to witness so magnificent an exhibition as that which took place. . . . But the most remarkable circumstance attending the phenomenon was the appearance of *three large protuberances* apparently emanating from the circumference of the moon, but evidently forming a portion of the corona. They had the appearance of mountains of a prodigious elevation; their colour was red, tinged with lilac or purple; perhaps the colour of the peach blossom could more nearly represent it. . . . The whole of these three protuberances were visible even to the last moment of total obscuration; at least, I never lost sight of them when looking in that direction; and when the first ray of light was admitted from the sun, they vanished, with the corona, altogether, and daylight was instantaneously restored."

Having regard to the vagaries of British weather, it is unfortunate that a thin veil of cloud is sufficient to hide all trace of the corona. But, even with an overcast sky, another spectacle is left, which never fails to produce a profound impression on all who see it. This spectacle is the terrific onrush of the moon's shadow through the air at a velocity, on this occasion, of nearly a hundred miles an hour. At such a time, as Major Hepburn recently remarked in *The Times*, "we find ourselves, as it were, brought into actual personal contact with the planetary motions," an opportunity which occurs only when an eclipse is total.

## ESSAYS

### FOSSIL BONES (J. Reid Moir)

As with the colour changes in the surfaces of broken flints, very little is known regarding the natural processes that give rise to the remarkable condition of certain ancient bones—known as fossilisation. In a former article in *SCIENCE PROGRESS* (No. 78, October 1925, pp. 249–55) I showed that, with flint implements, those exhibiting a deeply ochreous or mahogany-brown colour are generally of a very high antiquity, while others showing “basket-work” patination, or a blue or white coloration, are, in the great majority of cases, of less age. It was shown also that the most primitive and ancient implements exhibited the deepest ochreous shade, and those of less antiquity the other colours mentioned. While, with the least ancient of all no colour change has taken place in their surfaces. Finally it was suggested that the processes responsible for the imposition of these various colours appear to have acted with ever-decreasing intensity from the Pliocene period to the end of the Stone Age.

When we turn to the study of fossil bones, a somewhat similar picture presents itself. If, for example, an examination is made of a series of such specimens from (*a*) the Suffolk Bone Bed; (*b*) the Cromer Forest Bed of Norfolk; (*c*) the Hoxne deposits, and (*d*) the lowest of two occupation-levels in a small valley to the north of Ipswich, it is seen that, speaking generally, the oldest deposit, *a*, contains the greater proportion of the most ancient types of mammals, while *b*, which is of a less antiquity, contains a smaller proportion of such types—and so on. Thus the geological and the palæontological evidence is, in the examples quoted, in agreement, and shows that according to the age of the deposit so the proportion of ancient types of mammals in it varies. The condition of the bones from horizons *a*, *b*, *c*, and *d* also supports these conclusions; for a study of the specimens will show that the most ancient are more fossilised than those of later date. In order to establish this claim I give below the result of an analysis carried out by Herapath (*Survey Memoirs*, “The Geology of the Country around Ipswich, Harwich, and Felixstowe,” p. 103) of certain bones from deposit *a*—and of others from deposit *d*, which were examined in recent years by Messrs. E. Packard & Co., Ltd., of Bramford, near Ipswich. In the

former case the bones were found to contain 4·9287 per cent. of organic matter, while the latter contained 20·85 per cent. of this material. Several interesting points arise from the above analyses. It seems, in the first place, that in most cases the age of any given specimen has a very definite relationship to its state of fossilisation, and that any bones embedded in a deposit of the type of the Suffolk Bone Bed will, whatever their history may have been prior to such entombment, in the course of sufficient time assume a uniform fossilised condition. For the Suffolk Bone Bed is made up of constituents of widely different ages, and its contained bones must have had very varied histories before reaching their present resting-place. It is not reasonable to suppose that these bones—during their pre-Bone Bed existence—had all been lying in the same kind of deposit, nor exhibited a similar fossil state upon their arrival in the Bone Bed; yet their condition now shows very little variation and is one of advanced fossilisation. Thus we must conclude that during the period of time—possibly 500,000 years—since they were washed into the Suffolk Bone Bed, the difference in their states of fossilisation has been overcome, so that they now all contain about 5 per cent. of organic matter. It would be of much interest to ascertain—if such a thing were possible—whether the process of fossilisation is at the present time proceeding in the Suffolk Bone Bed, and to be able to form some opinion as to the possibility that after a further great lapse of time the bones would have lost the whole of their organic matter. It may be, however, that the 5 per cent. of this material which the specimens have retained represents the minimum capable of being reached under the conditions to which the Bone Bed relics are subjected. If we imagine, as seems highly probable, that certain of the specimens entered this deposit when in a fresh condition, we must conclude that a period of possibly 500,000 years has been necessary to remove most of their organic matter. On the other hand, it was found (*Journ. Roy. Anthr. Inst.*, vol. xlvii, July to December 1917, p. 385) that fresh bones contained 30·78 per cent. of organic matter, while the specimens from deposit *d*, which was probably laid down about fifteen to twenty thousand years ago, contained 20·85 per cent. of this material. Thus, in the period of time mentioned these bones have lost about 10 per cent. of organic matter, and if it may be regarded as normal for fresh bones to contain 30 per cent. or thereabouts, then it is clear that the process of fossilisation must have proceeded with much greater rapidity in bed *d* than in bed *a*. It is possible, however, that this process of fossilisation acts upon a fresh bone at first with rapidity, and afterwards with decreasing speed, until finally in some cases,

and for some unknown reason, it ceases without having eliminated the whole of the organic matter from the specimen. This, I admit, is largely speculation, nevertheless the facts quoted above seem to point to some such explanation. But the manner in which the bones—in different conditions of fossilisation upon their arrival in the Suffolk Bone Bed—have, under the circumstances obtaining in that deposit, been all brought to a comparable condition is very difficult to understand and presents a highly complex problem for solution.

The suggestion that the process of fossilisation acts upon a *fresh* bone with initial rapidity is supported by the work of A. F. Rogers (*Bull. Geol. Soc. America*, vol. 35, pp. 535-56, 1924), who thinks it probable that bones become fossilised in a comparatively short time, and afterwards no further fossiliferous changes of importance take place. It has been widely held that fossil bones are generally silicified, but this has been disproved by Rogers, who has made a close examination of 300 different examples, ranging in age from the Ordovician to recent times, and collected from various countries in every continent. This examination has shown that fossil bone consists almost entirely of the amorphous mineral collophane, which is also the principal constituent of phosphate rock. Rogers's work represents a distinct advance in our knowledge of the fossilisation of bone, but there are many problems yet remaining to be solved. For instance, the manner in which, and by what means, the organic matter is removed from the pores of the bone, and its place taken by collophane, thus preserving the structure, is not, so far as I am aware, ascertained. Another unexplained phenomenon is the remarkable change in condition undergone, with comparative rapidity, by bones removed from certain deposits of brick-earth. In many cases these specimens, when first found, are literally as soft as bread, and need the most careful handling—but, after exposure to the air for some time, they become quite hard and can be handled with impunity. This process of exposure allows the bones to dry, but it would not appear that the mere evaporation of the contained moisture is alone sufficient to account for the marked change in the condition of the specimens. Those who have had experience in discovering ancient bones will know that sometimes a process the very reverse of that just described is noticeable, in that the specimens when found are more or less hard, but on exposure to the air become soft and often crumble at the slightest touch. This latter condition is, I think, more frequently met with than the former, but both are of considerable interest, and the exact reasons for them should be investigated. It would seem probable that the material in which the bones have lain may

have a good deal to do with their behaviour on exposure to the air, as the nature of their matrix has certainly determined the state of preservation of many specimens. In the case of the bones from deposit *d*, it was found that where this was a calcareous clay, the specimens were well preserved, while where the bed was composed of sand they were in a most fragile state. Water was present in quantity, both in the clay and in the sand, so it is evident that moisture itself had nothing to do with the condition of the bones. Some years ago I carried out a series of experiments in the fracture and the shaping of bone (*Proc. Prehis. Soc. E. Anglia*, vol. ii, pt. 1, pp. 116-31), and found that, in the case of fossil examples from the Suffolk Bone Bed, it was next to impossible to produce any desired form by blows with a hammer-stone. The specimens fractured, usually along their natural grain and in a splintery manner. The bones were very highly fossilised, and in one or two examples I actually produced a pseudo cone of percussion, such as is formed by a blow upon flint, and other similar substances, when breaking them. It is interesting to note that, as with flaked flints, the bones from bed *a* exhibit a very deep mahogany-brown colour, while those of later date are not stained so deeply. This coloration is certainly due to the material in which the specimens have lain, and depends chiefly upon the amount of salts of iron contained in this material. The bones from bed *a* also often show a very high polish upon their surfaces—but the origin of this is obscure. In this short note I have merely drawn attention to some of the problems with which those who make a study of fossil bones are confronted. The solution of these problems must be undertaken by chemists and others possessed of the wish, and the necessary knowledge, to explore a little-known region of science.

#### **HISTORICAL NOTES ON THE MECHANICAL THEORY OF HARDENING** (John Innes, Royal Corps of Naval Constructors)

In his *History of Civilisation in England* Mr. Buckle [1] observes that "The progress of every science is affected more by the scheme according to which it is cultivated than by the actual ability of the cultivators themselves. If they who travel in an unknown country spend their force in running on the wrong road, they will miss the point at which they aim, and perchance may faint and fall by the way."

Each of the physical sciences may be divided into two parts; treating respectively of the manner in which the things concerned comport themselves, and of the causes of their observed behaviour. The science, or whatever it may be called, which deals with the mechanical properties of solids, and in

particular with the mechanics of metals, may be thus divided. The scheme according to which the former part has been cultivated was discussed last year [2] in *SCIENCE PROGRESS*; the scheme of the latter, its consequences, and some alternative suggestions, are the subject of the present article.

The members of a group of related phenomena cannot be understood singly, as a knowledge of any one of them necessarily involves a knowledge of them all. The particular topic to be selected for consideration is thus of little importance, but "hardening" has been chosen because the susceptibility of hardness to variation was recognised and made use of in remote ages, and has been the subject of continuous discussion during the last hundred years. The result of practical familiarity and prolonged deliberation may be seen by comparing the views of the seventeenth and twentieth centuries. Steel is the most important of the metals, and on the hardening of steel the Hon. Robert Boyle [3] writes :

"It is sufficiently known that the chymists ascribe the firmness and hardness of bodies to salt, and teach, that the saline ingredient of them is the principle of coagulation in them, and the cause of their compactness and solidity. But though this opinion of the chymists be embraced by so many modern philosophers and physicians, that some may think it superfluous to make enquiry after other causes ; yet others (to whom the explications of chymists seem not always so much as intelligible) will, upon the very account of the receivedness of the proposed opinion, think it rather worthy to be examined than to be acquiesced in. . . . And as for the hardness and brittleness they ascribe to the same principle, how much they may be increased or diminished in a body, without the accession or decrement of the saline principle or ingredient, may appear by that experiment mentioned by us to several purposes, of tempering a slender piece of steel : for when it has been sufficiently heated by plunging it red-hot into fair water, which is more likely to dissolve than increase its salt, you may make it not very hard alone, but very brittle ; whereas by only suffering it to cool leisurely in the air it will be both much less hard and more tough. And if after having quenched it in cold water you again heat it till it have attained a deep blue, it will become (comparatively) soft and very flexible, and that not from any wasting of the saline ingredient by the fire ; for if this softened steel be again heated red-hot, and suddenly refrigerated, whether in water or otherwise, as before, it will re-acquire both hardness and brittleness."

In 1926 Dr. Sauveur [4] addressed a series of questions on this subject to some twenty metallurgists, whose opinions may be taken to represent those prevailing in the leading manu-

facturing countries of the world. He comments on their replies as follows: "After this brief survey of the views held by those who have devoted much time to the study of the phenomenon of the hardening of steel, I believe that it is not possible to tell what is the prevailing view of metallurgists. Differences of opinion are met with at every stage. . . . It is evident that those who believe they have solved the problem of the hardening of steel have done so only to their own satisfaction, and to that of a few followers."

Natural science can afford but few parallel instances of a stagnation so complete and prolonged, and relating to a problem so conspicuous and important. A distinguished philosopher [5] has observed that the abuse of language serves not only as a cover for ignorance, but as a hindrance to true knowledge. The fatal defect in the scheme of metallurgical-mechanics is the looseness of its conceptions. That the indefinite mechanical terms employed in this science operate as an insuperable barrier to the commerce of thought may be seen in the circumstance that, while metallurgy disagrees on the hardening of steel, physics, with a more precise vocabulary, has long but vainly offered a theory which explains, not the hardening of steel alone, but the hardening of every metal, and which also affords illuminating suggestions on other technically important matters whose causes now seem to be regarded as obscure.

The theory in question—the colloid theory—was originated [6] by Dr. Arnold, Professor Benedicks, and others, some forty years ago, but its most vigorous exponent [7] is Dr. Wolfgang Ostwald. Recently published works, however, both on general metallurgy and on special metals, make no reference to the colloid theory. Some few writers, of course, have been more liberal [6], but an examination of the recent history of hardening [8] will show that the colloid theory has been very systematically ignored.

A theory is an induction of particulars, and it is proposed, in what follows, firstly to fill a conspicuous gap in the technical records by collating the principal observations which a theory of hardening must explain; and secondly, to suggest that, while other theories explain few or none of them, the colloid theory, when a little elaborated, appears to explain them all.

Metals, of course, may be "hardened" in two ways, by thermal treatment and by distortion, but it is merely a whim of the technical language that the same word should be used for two such distinct things. The subject of hardening is in sufficient disorder without this unnecessary complication, so only one kind of hardening—that referred to by Boyle—will be considered. Upon this understanding, namely, that "distortion hardness," or "cold work" as it is sometimes called,

be absent, the phenomena of hardening, as we have defined it, are as follows :

(1) Specially refined metals [9] (*e.g.* iron and aluminium) are much softer than the corresponding "commercially pure" varieties. Metals only commercially pure are but little affected by chilling ; the addition of a small percentage of almost any miscible body both hardens them and makes them more susceptible to heat treatment.

(2) The harder chilled form of an alloy, as compared with the softer, is less ductile and more brittle. It has a smaller coefficient of thermal expansion and a lower density.

(3) Hardening treatments take what appears to be an immediate effect on strong alloys. In the weaker alloys hardening, and its related phenomena, often take some considerable time to develop. The velocity of reaction depends in the usual way upon the temperature.

(4) When the metal contains suitable impurities in sufficient quantity, and when hardening treatment is sufficiently drastic, an alloy may fall to powder, either when the treatment is applied, or after a delay corresponding to the time of reaction.

In view of misapprehensions which have gained some currency, it may be well first to clear the ground by establishing a preliminary proposition. A body composed of pellets of a soft substance—like dough—would itself be soft even if occasional hard pellets were included in the mass. The body would become hard if the pellets were separated by a reticular structure composed of a firm and strong material, and the crystals of a metal, indeed, are, or are supposed to be, separated by a structure of this kind. A little computation, however, suffices to show that in the useful metals the inter-crystalline films are far too tenuous to act as a honeycomb and directly to lend any appreciable rigidity to the mass ; their function must be merely to transmit force—any kind of force—from one crystal to the next. In ordinary circumstances, therefore, the hardening of a metal must be ascribed to the hardening of its component crystals, or to the hardening of a majority of them, and not, as is sometimes suggested, to "refinement of grain size" and a consequently improved "interlocking" of the crystals. It is not clear, indeed, that grain size and "interlocking" are in any way related, and "interlocking," moreover, is rather suggestive of the conveniently hooked molecules which Boyle derided in the earliest days of rational mechanics.

A theory of hardening is therefore required to show how, at an assigned temperature and pressure, a single crystal of a metal or compound of metals can exhibit hardness, or resistance to distortion, in a degree which is dependent on the

cycles of temperature to which it has been exposed. We refer, of course, not to æolotropy in the crystal, but to a variation in any assigned one of its properties. The theory is further required to explain the already recited list of observations which are normally found to characterise hardness and its changes.

A branch of research recently initiated [10] by Dr. Carpenter confirms and illuminates what was originally discovered by Sir James Ewing and Dr. Rosenhain, namely, that single crystals of a metal, when exposed to sufficient stress, are prone to "landslides" along particular atom-planes. Combinations of such landslides are the only mode by which a crystal can permanently change its shape, and it is evident that, if the atom-planes, instead of being flat, are puckered, and fit into one another, the crystal will become "not very hard alone, but very brittle"; it will take a greater force to originate a landslide, and when sliding has once started it will be more likely to result in a total rupture of continuity.

Puckering of the kind postulated, in a degree dependent on the circumstances, would seem naturally to arise when differential thermal contraction follows the formation of an impure metallic crystal. When a liquid mixture cools, the first conspicuous phenomenon is the precipitation of a cloud of the least miscible material. The cloud floats, sinks, coagulates, or remains as a fine suspended fog, according to the laws, known or unknown, of colloid reaction. Meanwhile the remaining constituents become solid. The growing crystals may, in part, push the impurities aside, but to some extent they will merely enclose them without any disturbance of the crystal lattice.

According to a very general rule of nature, substances of higher melting point have the smaller coefficient of thermal expansion. When a crystal shrinks on cooling, and contains particles of a foreign matter which shrinks less, the space lattice in the neighbourhood of the inclusions must be distorted. A general idea of the action and of the influence of its various factors may be obtained from the mathematical theory of isotropic elasticity. The upshot of the analysis is that the continuous phase is subject to distortional stress, which is greatest at the surface of the inclusions, and decreases in the radial direction. Approximate numerical estimates show that the stress may be expected to exceed the elastic strength of the crystal. Inelastic deformation, however, causes a body to swell [11], so material in the neighbourhood of an inclusion finds itself in a quandary; it feels that it should yield to distortion, but to do so would involve expansion, an increase of pressure, and more widespread distortion. The rate of cooling, and the details of any subsequent heat treatment, will clearly affect the final result. The conditions of

equilibrium seem complicated, and in this way they correspond with the observed phenomena. Differential contraction of neighbouring crystals in a conglomerate mass will occasion additional stresses and strains, but it is suggested that the effect within the crystals is the more important one. Centres of stress of the kind postulated may be seen in any transparent crystal which contains suitable inclusions, and which has cooled appreciably since the date of its formation.

Really pure metals and stable metallic compounds can give no foreign precipitate; and are thus unaffected by chilling, and at their maximum softness. In less pure metals the zone of strain which surrounds the inclusions will give rise to inelastic distortion and a reduction of density. When a chilled and hardened metal is warmed the pressure of the disperse phase will be lessened, and the normal expansion which accompanies heating will be modified by an abnormal contraction; with the result that the coefficient of expansion will be less than that of the same metal when in the soft state.

The weaker of two metals (aluminium, for example, as compared with iron) has the greater coefficient of expansion, and, in a given temperature range, will be more severely strained. It is well known that distortional stress does not produce its full effect instantaneously, and in weak metals the full effect of chilling (the full growth of the spheres of distortion) may not occur until after the lapse of many days. The reaction will naturally proceed faster at the higher temperature. If the inclusions be sufficiently large, sufficiently close to one another, or composed of sufficiently non-contraction material, the metal will break up, either immediately upon chilling, or later when the effects of chilling are developed.

The colloid theory, if it proves acceptable, would seem to involve the revision of many current ideas.

Professor Willard Gibbs's phase-rule, for example, has been requisitioned as the theoretical support for a large part of modern metallurgical doctrine, although Dr. Ostwald [7] has emphasised that the rule takes no account of surface energies, and is therefore inapplicable to the technically important alloys. According to the foregoing elaboration of the colloid theory, there are other grounds upon which the phase-rule is invalid. Its equations are based on the supposition that the stress is a uniform pressure, whereas, in an impure metallic crystal, the stress has been seen to be a pressure which not only varies from point to point, but is variable according to the direction in which it is estimated. It were needless, however, to consider this and other objections in detail. The phase-rule deals with the components, phases, and degrees of freedom of a system, and establishes a relation

between the numbers obtained by taking a census of these quantities. Distortion is a matter of degree; the parts of a body which have been much and little distorted have different intrinsic energies, and, in the accepted meaning of the word, are different "phases." The objection to the phase-rule as a metallurgical instrument rests, not so much on its invalidity, as on the suggestion that a continuously variable quantity can be enumerated. The proposed census is an absurdity.

In experimental, as distinct from theoretical, metallurgy, great importance has naturally been attached to "thermal analysis" or the study of cooling curves. Arrests in the cooling of a solid are ascribed to the transformation of phases, whereas they must often be due to the development of internal hardening strains.

When their theoretical basis is unsound, and their experimental basis misinterpreted, it would seem that the "equilibrium diagrams" of metallurgy, and the conclusions drawn from them, are things which "although embraced by so many modern philosophers and physicians" are "worthy rather to be examined than to be acquiesced in." Dr. Desch has observed that "metallographic researches have resulted as yet in comparatively few far-reaching generalisations." These may prove to be erroneous, and if so metallography will lose the characteristic mark which distinguishes a science from a collection of memoranda. Such a collection, of course, may be useful, but can make no claim to the utility of a science.

Hardening, finally, is a mechanical phenomenon whose manifestations the colloid theory seems to explain quite satisfactorily. The other theories available are expressed in non-mechanical terms whose relevance, at best, is remote. In the opinion of at least one eminent metallurgist they resemble the theories current in Cromwell's time, and are "not always so much as intelligible."

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## NOTES

### **Pest Destruction by Aeroplane (A. E. Blake)**

Mr. Amery's forecast to the Imperial Conference of the day when the aeroplane would be applied to the eradication of the tsetse fly from Africa is a reminder that there is no more hopeful augury of eventual victory in the conflict between men and insects for the possession of the world's fruits than the success which has attended recent applications of the aeroplane to the sterilisation of insect-infested crops.

There are few cultivated plants which provide the raw material for industrial processes or human food that are not equally esteemed as an article of diet in the insect menu. Agriculturists of a past age watched their crops consumed by insects or ruined by disease with a fatalism dictated by their impotence to defend their lands from the visitation, but increased technical knowledge has made the farmer less patient under adversity, and the forced development of the war-years has added several very effective weapons to his armoury.

It is, of course, plantation agriculture that will benefit most from the application of such instruments of wholesale pest destruction as the aeroplane, but since instances exist of the successful use of aircraft in spraying fruit orchards and timberland, and since the principles involved in plantation-crop dusting are the same as for the spraying of many fruit crops grown on a large scale, an account of the development of the idea and a description of the apparatus and methods employed may not be without interest to the fruit grower.

American entomologists were the first to realise the possibilities of the aeroplane for crop dusting. In 1921 a grove of catalpa trees near Dayton, Ohio, being infested with the catalpa sphinx, was dusted from an aeroplane with lead arsenate. The machine's method was to fly some 50 feet from the trees on the windward side of the grove, while a strong wind was blowing. The poison was fed from a hopper with a capacity of 200 lb. attached to the side and rather below and to the side of the occupants, to avoid the danger of inhalation. The dust was distributed from the hopper by a small reel with spoon-like arms, which was operated by a crank handle. Despite the rather primitive nature of the apparatus, the

dusting was found to be effective for a depth of 60 feet from the edge of the grove, and a mortality of 99 per cent. of the pest was revealed by subsequent examination. So successful was the experiment that Dr. B. R. Coad, who was in charge of the Delta Laboratory for Cotton Boll-weevil Investigation at Tallulah, Louisiana, conceived the plan of adapting the principle to his own problem of showing the cotton planters of the southern States how they might save the 225,000,000 dollars' worth of cotton that was the annual tribute levied by the weevil.

After experimenting for some time, Dr. Coad realised the necessity for an apparatus specially adapted for use in conjunction with aircraft, and invited the collaboration of the Huff-Daland Airplanes, Inc. The technical experts of this firm succeeded in perfecting such a device and, in 1924, a company known as the Huff-Daland Dusters, Inc., commenced operations on a commercial scale. The results exceeded expectation. In speed of operation the aeroplane was at least a hundred times as fast as the best mule-drawn machine, which occupied a day in dusting 30 acres. The new method was much more economical in its consumption of chemical, diverted none of the workers from picking or cultivation, and could be performed without delay immediately the state of the crops showed treatment to be desirable. The possibility of damaging the crops caused by the passage of the machines was obviated, and muddy land was powerless to hamper the work of the aeroplane as it does the ground machine.

Even more important an advantage was the fact that the work was actually more efficiently performed. This was in part due to the employment in the distributing mechanism of certain characteristics of the new method. Essentially, the apparatus consisted of special hoppers which deposited a thin stream of finely pulverised calcium arsenate into the current of air generated by the propeller and the drift of the aircraft, the velocity of which current had been raised to some 200 miles per hour by a funicular scoop. The down-draught of the air-screw then forced the powder down into the plants.

Dusting by any form of ground mechanism had, until the advent of the aeroplane, to be effected when the dew provided a surface to which the powder could adhere, thus necessitating nightwork, which was particularly disliked by the workers and made supervision very difficult. It was found, however, that the poison deposited by aircraft adhered to the leaves and flowers when their surfaces were normally dry, and the operation could therefore take place by daylight. This was due partly to the force with which the powder was blown upon the crops, partly to the minuteness of the powder particles, but chiefly to the fact that the dust-particles had, in the process of

delivery, become positively charged with electricity and had come into contact with plants carrying a negative charge. On this hypothesis, a ground machine utilising an electrical apparatus has been invented, and it is believed that this will prove vastly superior to any known type, and will have the further advantage of being operable in daytime.

This machine, however, is designed for use by planters who desire to perform their own dusting, and is not likely to affect the growing popularity of dusting by aircraft. There are two main types of aeroplane used for cotton dusting. About 80 per cent. of the area under cotton in America consists of relatively small and separate fields, the dusting of which demands a small and readily manœuvrable machine carrying 300 lb. of insecticide, and with a fuel capacity sufficient for four hours' continuous operation. The remainder of the cotton acreage consists of large estates over which an aeroplane can proceed for long distances without change of direction, and for this work a larger machine carrying 1,000 lb. of calcium arsenate and able to remain in the air for the same period is employed.

During operation, the aeroplane flies at a height of from 5 to 25 feet above the crops, its speed of 80 to 90 miles per hour enabling it to dust at the rate of 75 acres a minute, in slightly-overlapping strips some 200 feet wide.

In addition to cotton, spruce trees, peccans, tobacco, peaches, potatoes, citrus, and sugar cane have been subjected to aerial dusting with every success.

Germany provides another instance of a satisfactory experiment in eradicating insect pests. In 1925 there was a severe plague of nun moth in a forest of mixed timber extending over some 600 acres. An aeroplane was commissioned to dust the area thoroughly, a task it performed by flying between 12 and 60 feet above the tree-tops and depositing poison at the rate of 26 lb. to the acre, in strips of from 80 to 120 feet wide. Nun moth is reputed to be strongly resistant to insecticides of ordinary intensity, but the work was done so effectively that, on examination about a week later, it was found that all the moth larvæ were dead. It is stated that no harmful effect on birds or game was noticeable.

Quite recently an aeroplane was employed to spray the forests in the neighbourhood of Hagenau, France, which were infested with caterpillars. Britain has been backward in availing itself of the help of the aeroplane in combating its insect pests, but this is probably due rather to the rarity of the plantation system of agriculture. There are many crops now grown in England, however, as well as orchards and forests of some extent, that could economically be dusted from the air. Even in 1924. 50 acres of orchard land near

Sevenoaks were dusted against caterpillar infestation, while a potato grower in the Spalding district has this year employed an aeroplane to spray his crops as a preventive against the epidemic that has been rife.

Only the fringe of a vast field of activity has as yet been touched by the aeroplane. Of the 15,000,000 acres under cotton in America, only 100,000 were dusted in 1926, employing some score of aircraft. Every hot country is plagued by the locust, South African Agriculturists suffering losses to the extent of over a million pounds sterling in 1906. Malaria, too, might still further be reduced if the breeding-places of the mosquito were systematically sprayed from the air, as has been done in the flooded rice-fields of America.

It is not extravagant to say that, in the war men have waged with the insects from immemorial times, the odds have been in the favour of their tiny antagonists. Our methods of wholesale destruction have destroyed friends as well as foes, and our entomologists now tell us that, in order to alter the balance of Nature favourably to ourselves, we must work with cunning as well as slaughter. To enlist the most recent and wonderful of the products of science into service against the insects is therefore in consonance with modern tendencies, and in the eternal problem of making two blades grow on land that has hitherto yielded only one, the aeroplane may be as serviceable as the plough.

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### The Optical Convention, 1926

The permanent record of the British Optical Convention, held at the Imperial College of Science last April, takes the form of a comprehensive catalogue of British optical products and

two bulky volumes containing the text of the presidential address and the papers, numbering nearly one hundred, read at the Convention. The catalogue contains descriptions of all classes of optical goods from the raw glass to the most complicated instrument, and will be found most useful as a reference work in the laboratory. It was, no doubt, impracticable to quote prices, although their inclusion would have added very greatly to the immediate value of the book. The instruments which are described are, presumably, all British ; but on turning over the leaves we noticed one illustration showing a design identical with that of a well-known German manufacturer !

The papers published in the *Proceedings* cover so much ground that it is hardly possible here to do more than indicate their scope. An attempt at systematic arrangement has been made, and the first volume contains the papers dealing with the instruments and methods of the workshop, colorimetry, photometry, and the eye. Vol. ii opens with a series of most interesting papers on historical apparatus, and contains papers of interest to the ophthalmologist, papers on astronomical and surveying instruments, photographic lenses, projection apparatus and spectroscopic appliances. It is rather astonishing that there should be only two papers dealing directly with the microscope and none at all on polarised light, the nearest approach being a paper entitled "Light Sources for use in Polarimetry."

Perhaps the most important paper in the whole of the two volumes is that written by Mr. J. Guild, of the National Physical Laboratory, and entitled "A Survey of Modern Developments in Colorimetry." We have here gathered together for the first time a reasoned and complete account of a subject concerning which very little is known by the average physicist. It is, however, a subject of great commercial importance and much scientific interest, and it is probable that vol. i will find a place on many bookshelves for the sake of this paper alone. Another paper by Dr. L. C. Martin on "Some Visual Factors affecting the Use of Optical Instruments" is second only to Mr. Guild's in importance and has perhaps an even wider application. "The Theatre Mutochrome," by Mr. C. F. Smith, describes an instrument which may afford entirely new possibilities in theatrical effects, while Mr. E. F. Fincham gives a very complete account of the mechanism of the accommodation of the eye in a paper bearing that title. Vol. ii contains an interesting account of the methods of stellar photometry entitled "The Next Task of Astronomy," by Prof. R. A. Sampson ; an account of his methods of manufacturing optical parts by Mr. William Taylor ; and a whole series of papers on lenses introduced by "A Review of Lens Theory," by Mr. T. Smith.

These two volumes should be added to every science library and they are absolutely essential to all those engaged in optical work.

### Notes and News

The New Year Honours List did not include many rewards for scientific achievement. Knighthoods were conferred on Dr. Henry Head and Mr. A. E. Kitson, Director of the Geological Survey, Gold Coast Colony, while Mr. H. T. Tizard, Principal Assistant Secretary of the Department of Scientific and Industrial Research, was made C.B. (Civil Division).

Prof. Julian S. Huxley has been appointed Fullerian Professor of Physiology at the Royal Institution.

The Wollaston Medal of the Geological Society of London has been awarded to Prof. W. W. Watts ; the Murchison Medal to Mr. G. T. Prior of the British Museum ; and the Lyell Medal to Sir A. E. Kitson.

Prof. J. S. Haldane, the Director of the Mining Research Laboratory, has been awarded the Gold Medal of the Royal Society of Medicine.

Among the men of science whose death was made known during the past quarter were Prof. C. J. Eberth, one of the discoverers of the typhoid bacillus ; Dr. F. Exner, Professor of Physics in the University of Vienna ; J. D. F. Gilchrist, Professor of Zoology in the University of Cape Town ; Mr. J. Goold, of Goold's bars fame ; Sir George Greenhill ; W. J. Hussey, Professor of Astronomy in the University of Michigan ; Prof. E. Molinari, of Milan, chemical technologist ; Dr. F. E. Nipher, Emeritus Professor of Physics in the Washington University, St. Louis ; Dr. H. Campbell Ross, Director of the McFadden Research Foundation, at the Lister Institute ; Mr. T. S. P. Strangeways, pathologist ; Sir William Tilden, chemist ; Sir Charles Warren, soldier and archaeologist ; Mr. J. Webster, Senior Scientific Analyst to the Home Office.

The British Association will meet at Leeds during the period August 31—September 7. Sir Arthur Keith has been elected President of the Association and the sectional presidents will be : A (Mathematics and Physics), Prof. E. T. Whittaker ; B (Chemistry), Dr. N. V. Sidgwick ; C (Geology), Dr. H. H. Thomas ; D (Zoology), Dr. G. P. Bidder ; E (Geography), Dr. R. N. Rudmose Brown ; F (Economics), Prof. D. H. Macgregor ; G (Engineering), Sir J. B. Henderson ; H (Anthropology), Prof. F. G. Parsons ; I (Physiology), Dr. C. G. Douglas ; K (Botany), Prof. F. E. Fritsch ; L (Education), the Duchess of Atholl, M.P. ; M (Agriculture), Mr. C. G. T. Morison.

Mr. Roy J. Kennedy has repeated the Michelson-Morley experiment at the Norman Bridge Laboratory, Pasadena, and at Mount Wilson. The apparatus used was only one-sixteenth the size of that used by Prof. Dayton Miller at Mount Wilson two years ago, but its sensitiveness was such as to permit of the detection of fringe shifts equal to one-fourth of those obtained by Miller. The reduction in the size of the apparatus made it possible to obtain an effective control of the temperature and pressure of the gas lying in the paths of the light waves. The whole apparatus was, in fact, enclosed in an air-tight case filled with helium at atmospheric pressure, helium being chosen because it reduces the effect of density changes as compared with air. The experiments failed to show any trace of the effects obtained by Prof. Miller, and Mr. Kennedy concludes that there is no evidence of any motion of the aether relative to that of the earth on its journey through space.

The Annual Report of the Director of the Bureau of Standards, U.S.A., for the year 1925-6 is a business-like document, which in 36 pages summarises the work of the Bureau in a manner designed to make clear to the American citizen the benefits he receives as a result of the two millions of dollars expended annually in his Government Laboratory. Most of the activities of the Bureau find a parallel in the work of the National Physical Laboratory and the Trade Research Associations in this country. One most unfortunate difference lies in the care taken by the Bureau to give publicity to its labours. A monthly *Technical News Bulletin* is issued containing brief references to the progress of current work, and "There are also released to the press numerous short accounts of interesting achievements, together with many photographs illustrating the Bureau's work. These have been well received all over the country." No such efforts are made to acquaint the public over here with the work of the N.P.L. and the most enthusiastic supporter of the D.S.I.R. would scarcely venture to describe the typewritten reports which it occasionally issues to the press as interesting. Here, for example, is a notice recently issued by the Safety in Mines Research Board :

A detailed account of an apparatus designated "a wave-speed camera" is given in a paper by W. Payman and W. C. F. Shepherd, entitled "The Pressure Wave sent out by an Explosive, Part II" (Safety in Mines Research Board Paper No. 29: H.M. Stationery Office, Adastral House, Kingsway, London, price 1s. net). This apparatus was devised to obtain records of (a) the rate of propagation of the shock- and pressure-waves sent out by an explosive, (b) the rate of projection of the flame and products of detonation, and (c) their relative positions at each instant after the shot has been fired. Experiments are described illustrating the operation of the apparatus, using detonators as the explosive charge.

The experiments have shown that the flame of a No. 6 detonator never comes into contact with the external atmosphere, being always surrounded by a "blanket" of non-combustible gases, the products of detonation. This fact explains the inability of the flame of a detonator to ignite a mixture of firedamp and air.

A very modest effort of imagination would have sufficed to show those responsible for this note that it was not well suited for general circulation. What is the underlying practical problem which is being attacked? What is the difference between the shock- and pressure-waves? And, above all, what precisely is the significance of the use of a No. 6 detonator? It would have been quite possible within the space of a single foolscap sheet of typescript to give an intelligible and interesting account of the work referred to.

Reverting once more to the Bureau of Standards, one particular branch of its work is worthy of special notice: the standardisation of equipment. This refers to the efforts which are being made to reduce the varieties of articles in common use made by manufacturers in the States—of course with the cordial co-operation of the said manufacturers. The varieties of chinaware used in hotels, hospitals, and railway cars have been reduced to 160 from 700, the different sizes of blankets from 78 to 12, of cut tacks and nails from 428 to 121, and of shovels, spades, and scoops from 5,136 to 1,178. These are merely four items chosen at random from a very long list, showing, in most cases, a reduction of at least 50 per cent. Estimates running into many millions of dollars have been made of the economies effected in this way. The latest example is given in Technologic Paper No. 324 published since the Annual Report was written. It is entitled *Standard Hosiery Lengths* and describes an investigation of the variation of the lengths of men's, women's, and children's hose carried out at the request of the National Association of Hosiery and Underwear Manufacturers. To take the simplest case, it appears that the length of ladies' hose (measured in a manner specified in the paper) varies in a random manner from 25 to 29.5 inches, and the recommendation is that the length in future shall be 27.5 inches, with a tolerance of 1 inch either way irrespective of size. In the case of men's and children's hose the lengths recommended vary with the size of foot. Such work undoubtedly forms a somewhat extreme example of applied physics, and the Bureau is engaged on many researches of a purely scientific character. One interesting item is a re-determination of the gravitational constant; the experiments are still in progress, but the value  $6.653 \times 10^{-8} \text{ cm.}^3/\text{gm. sec.}^2$  is quoted as a tentative result.

The Report of the Fuel Research Board for the year 1925

(H.M. Stationery Office, price 1s. 3d. net) is not a very cheerful document, for it shows that progress on the main problem confronting the Board, the low-temperature carbonisation of coal, has been very slow. Retorts of several kinds have been tried; but for one reason or another (generally distortion of the metal of the retort) they have shown themselves incapable of commercial use. No immediate solution is to be expected, for, as the chairman states, "a few weeks or months may be sufficient to prove that a plant is not commercial, but some years are necessary to give a proof that it is really satisfactory." Dr. C. H. Landier, the Director of Fuel Research, prepared a memorandum on the Utilisation of Coal for the Royal Commission on the Coal Industry (1925) which is printed as Appendix IV in the Report. In it he gives a clear and concise account of the advantages and disadvantages of low-temperature carbonisation, and concludes that not for a long time to come is the low-temperature process likely to displace the present methods of the gas industry. The whole of this appendix will be found most interesting by those interested in coal and its uses. Two other matters referred to in the Report deserve mention. Firstly, the increasing importance of methods of sampling and analysing coal owing to the demand of foreign buyers for coal of a definite specification and the compliance of foreign producers with this demand. Secondly, the progress which is being made in the physical and chemical survey of the whole of our national coal resources. This survey is essential for the economic use of the coal, and has an important bearing on the question of the specification of coal just referred to.

We have received a circular dealing with the work and aspirations of the Eugenics Society which illustrates once again the pressing need for the application of the outstanding principles of eugenics to the crowded population of this country. The number of notified insane in England and Wales on January 1 last was nearly 134,000, and in addition, some 55,000 defectives were known. The public expenditure on these people in 1924 was some £7,000,000. Such expenditure is now inevitable; but both mercy and sense demand that all reasonable means should be adopted to prevent its increase. Dr. Tredgold estimates that more than 80 per cent. of this mental deficiency is due to hereditary causes, and yet children from the special schools for mental deficients are allowed to go out into the world and become parents of further tainted generations. It is the prime purpose of the society to insist that the palpably unfit and degenerate shall be prevented by segregation or sterilisation from multiplying their kind. The address of the Society is 20 Grosvenor Gardens, S.W.1.

## ESSAY-REVIEWS

**EXPLANATION IN BIOLOGY.** By J. H. WOODGER. Being a Review of *Experimental Embryology*, by G. R. DE BEER, M.A., B.Sc. [Pp. 148 with 51 illustrations.] (Oxford. At the Clarendon Press, 1926. Price 7s. 6d.)

THIS attractive little book contrives, within the small compass of its 148 pages, to cover the whole field of modern experimental inquiry into developmental processes, including regeneration and tissue culture, in a way well calculated to set the reader strenuously to think and to whet his appetite for more. The latter contingency is provided for by a tabular summary of the experiments at the end of the book, in which full references to the original sources are given.

The birth of *Entwicklungsmechanik* was a momentous event in the history of the biological sciences. How lustily the infant has grown, and what shrewd knocks it is already giving to some of the cherished dogmas of its sedate predecessors! Before the advent of the experimental method, morphology occupied itself with speculations about events which were supposed to have happened at a period of time so remote as to be entirely free from all danger of verification or refutation. The application of experiment has shifted the focus of attention from speculations about what organisms were *once* to questions about what they are and do *now*—problems to which we can reasonably hope to find a solution, and which are of vital interest for biological theory.

Each short chapter in this book bristles with interesting controversial questions. The new data revealed call for the formation of new concepts to deal with them, and this has been done in regard to certain general features of the developmental process. These new notions give the general impression of being crude tools for such intricate work, and when the attempt is made to think them out more fully and give them greater precision, many contradictions present themselves. Particularly do there seem to be cleavages between the concepts of genetics, cytology, and experimental embryology. But this is inevitable at the present stage of the problem, in which any suggestion which offers guidance for research will, so far at least, justify a trial.

We may turn from questions of detail and consider certain fundamental matters of principle of a more general nature. It is interesting to note that the generation of biologists who are devoting themselves to this work is discovering that its first duty is the establishment of *biological* concepts; in other words, that the problems of biology are not to be solved by a wholesale importation of the explanatory equipment of another science—neither by forcing the concrete facts presented by the living organism into the abstract moulds of physics, nor by having recourse to the *deus ex machina* offered by the various sects of vitalism. In illustration of this change and of the seeming reluctance with which it is made, the following passage from Mr. de Beer's book may be quoted (p. 133): "Just as the properties and behaviour of chemical substances had to be determined before any great advance could be made in that science, so in biology the experimental method is revealing phenomena which one must call 'biological properties' of living matter; such as the organisation of the axial structures by the dorsal lip of the blastopore or the formation of a lens by the activity of the eye-cup." There is a faint air of apology about the wording of this sentence which suggests that the author has not yet entirely freed his mind from the trammels of the mechanical philosophy. Chemical properties are quite correct, but biological properties require inverted commas! Again, on p. 134 we read: "There is no hiding the fact that most of the complex components of development are as yet unintelligible, but by the experimental method the analysis is gradually splitting them up." Why should there be any question of "hiding" facts, except in the interests of a made-up mind which does not wish to be disturbed from its dogmatic slumbers? The unconscious influence of the mechanical philosophy of the last three centuries is revealed in a curious way in the following passage from p. 134: "With regard to the term 'explanation' it must be remembered that ultimately nothing can be really explained. It is idle and absurd to imagine that experimental embryology will explain the development of organisms. What it can do is to show that the processes involved therein, however complicated and dissimilar they may be, obey certain definite laws." When experimental embryology succeeds in establishing these laws, surely it will thereby have offered an explanation of development. What other meaning can the word have in science? Mr. de Beer himself, in the second passage quoted, speaks of certain "components of development" as being "not yet intelligible"—implying that the progress of investigation may some day render them so. A convenient word for rendering intelligible is explanation and that is its

usual meaning, without any reference to ultimate explanations which lie beyond the scope of science. Consider the following example of a genuine biological explanation. Suppose we have the following propositions: "All mammals secrete milk" and "All mammals bear hair." From this we might conclude that milk secretion and hair production always occur together in the same animal class. This suggests the further question: Is this connection merely "accidental" or are the two facts causally connected? Further investigation shows that the first proposition can be put into the form: All mammals have milk-secreting glands in the skin; and the second into the form: All mammals have a skin in which hairs develop. This narrows the common term down to the skin. Now it has been shown that the mammary glands are a specialisation of glands which always develop in connection with hairs. The connection between mammary glands and hair is thus explained, *i.e.* made clear, and it is seen to be no mere chance association, by a gradual narrowing of the terms common to them both down to their developmental *Anlagen*. If now we go back to the original propositions, we see that a great deal has been accomplished. An intelligible connection has been established between two things which at first seemed without any link save in the fact that they occurred together in the same animal group. It would be in the highest degree pedantic to deny the title of explanation to this process merely because still further questions present themselves. And yet we frequently find, even in biological literature, this denial of all claim to explanation. Whence comes this modesty of science?

It is to be traced to a remark in the introduction to a book on mechanics by Gustav Kirchhoff in which he declared the task of that science to be "die in der Natur vor sich gehenden Bewegungen *vollständig* und auf die *einfachste Weise* zu beschreiben." This view was furnished with a philosophical basis and elaborated by Ernst Mach, into phenomenalism—a doctrine which has found much favour among those men of science who have considered such matters. It has been made familiar to English readers by Prof. Karl Pearson's *Grammar of Science*. The doctrine has been read by biologists, and faithfully copied by them in accordance with the belief that what is good for mechanics is good also for biology. There are grave objections to phenomenalism, and especially to the arguments by which it is reached, which are worth considering from the standpoint of biology. We can first inquire how it came to recommend itself to physicists, leaving aside the wider issues and concentrating on the question of explanation.

During the development of science two processes have been going on. On the one hand there has been thinking in terms

of real perceptible things, and on the other the working out of the discovery of Galileo that most of the qualities of bodies could be neglected for the purposes of physics if attention was confined to those which admitted of measurement, and hence of mathematical treatment. Now, when the corpuscular hypothesis of matter was employed for the investigation of terrestrial phenomena these two processes of thought continued. Physical objects were thought of as composed of ultimate particles far below the level of perception. But it was possible to *imagine* these particles on the basis of perceptual experience, and the mental picture rendered many processes intelligible, *e.g.* the expansion of bodies under the influence of heat, etc. But at the same time the calculating process was also applicable. It was possible to treat the particles just as perceptible objects had been treated, in accordance with the Newtonian laws of motion, and, as everybody knows, with astonishing success. Later it began to be realised that the mental pictures of these things must be to a large extent illusory. A modern textbook of physics still tells us to regard molecules as "hard, perfectly elastic spheres," but how can they be spheres when they usually contain at least two atoms which are themselves particulate? What meaning can be attached to such terms as hard or solid on the infra-perceptual level? Finally, if we never meet perfectly elastic bodies in the perceptual world, upon what grounds can we assert their existence in the molecular realm? Thus the tendency is to discard the mental picture as a convention or fiction which plays no lasting part in the explanatory process, the real work being done by the mathematics. In other words, progress is seen to lie in increasing precision and scope of *calculation*, but this brings no increase of intelligibility, and so explanation tends to be abandoned. This is still more clearly recognised to-day when the very latest concepts of physics can no longer be represented in the imagination at all because they are not directly borrowed from the perceptual world, and hence the doctrine of relativity can only be expounded in mathematical terms. These considerations enable us to see why Kirchoff wished to renounce explanation. If we add to these facts the banishment of force from physical thought as a mere anthropomorphism, and the fact that the criticisms of Hume have had their due effect, we realise why physics has tended to abandon all the real categories and come to rest finally on the formal ones which are all that is required by mathematics. This, of course, means the complete abandonment of explanation, and the question arises how far these facts concern biology.

Anyone who takes the trouble to inquire will find that the changes now in progress in the fundamental principles of

scientific thought are nothing short of revolutionary, and are likely to have important consequences far beyond the confines of science. The difficulties of the mechanical philosophy, which has dominated that thought for more than three centuries, are no longer able to conceal themselves. Thinkers like Prof. A. N. Whitehead are endeavouring to repair the damage wrought by that "murderous stroke" (as the late Prof. James Ward called it) by which Descartes cut the world into two halves: the physical on the one hand, and the mental on the other. Later physical inquirers, who did not share the Cartesian theological predilections, which provided a link between the two, found no difficulty in seizing one half and abandoning the other. Thus natural philosophy exploited the mathematical treatment of the "real" primary qualities, and banished the secondaries to the "illusory" world of mind. Moral philosophy, on the other hand, gave its attention to the latter half and to the problems of the relation between the two. Phenomenalism offered a way out of these perplexities by abandoning the real world and by contenting itself with renouncing all claims to make assertions about it. It tells us that in science we can do no more than devise conceptual schemes for subsuming the phenomena which, so it asserts, are all that is given to us in perception, because there is no real world apart from it. Mach says: "Das monströse, unerkennbare 'Ding an sich,' welches hinter den Erscheinungen steht, ist der unverkennbare Zwilling Bruder des vulgären Dinges, welcher den Rest seiner Bedeutung verloren hat!"<sup>1</sup> You may have as many alternative conceptual schemes as you please, preference only being given to the simplest. This doctrine claims to give its adherents freedom from the difficulties which beset others, but one wonders how many of its supporters really believe it, and how far it is possible to avoid difficulties by not looking at them.<sup>2</sup> From this point of view we should look with suspicion on the fact that the last great English idealist—F. H. Bradley—gave this doctrine his blessing *as a doctrine for science*, for this suggests that such thinkers saw in it a convenient means of drawing the teeth of physical science—of rendering it harmless because meaningless. Under the influence of the New Realist Movement in epistemology phenomenalism must tend to lose favour, and modern thought

<sup>1</sup> E. Mach: *Erkenntnis und Irrtum*, dritte Auflage, Leipzig, 1917, p. 10.

<sup>2</sup> For example, how does the Doctrine of Evolution stand in the light of Phenomenalism? Is it only a conceptual scheme for describing certain morphological appearances, or does it claim to apply to a real historical process? Phenomenalism would seem to be committed to the former alternative, in which case it is difficult to see how it can use the doctrine in the way Mach does to interpret human psychology.

is grappling with the difficulties which phenomenalism was content to set aside. This movement, in some directions at least, involves an abandonment of the all-embracing character of the mechanical philosophy with its over-generalising of abstractions, and a reinstatement of secondary qualities, meanings, activity, purpose, and values, and other non-spatial or non-measurable things into the real world. Because mechanics quite rightly banishes "force" and "activity" from its conceptual equipment as mere anthropomorphism is it correct to turn this deanthropomorphising process upon *ἄνθρωπος* himself? Because such things are now recognised as projections of ourselves upon the inorganic world, does it follow that they can also be dismissed from the very sphere from which they have been projected? Yet it has been considered "necessary" on such grounds as these to consider spontaneity, activity, etc., as myths even in the organic realm. After more than three centuries under the dominion of these notions, thought is trying to reach a *Weltanschauung*, which will have an equal place for psychology as well as physics, and for art and ethics too. This means that biologists can take heart again and forge their own weapons for the attack on their own proper problems, without feeling the need to apologise for their boldness. In experimental embryology concepts borrowed from the physical sciences do not admit of calculations being made, and until they do that they are not really playing the same rôle as they do in the sciences from which they have been borrowed and for which they were devised. Consequently they may be playing a spurious part tending to mislead rather than to guide. Explanation and calculation are quite different means of interpretation, as we see in those sciences in which calculation plays little or no part.

It has been the custom to define natural science in such a way as to exclude all possibility of the introduction of psychological concepts, but, when this is extended to biology, it involves (at least in the case of man) the very process of cutting the organism into two which has been the source of all our troubles. It means regarding the body exclusively as a machine, and the mental function as either a meaningless collateral process of some kind, or as an utterly disparate thing with the flimsiest connection with the body. This complete separation of the biological from the psychological point of view is now recognised as having had unfortunate consequences for psychology, and there can be no doubt that the progress of biology has also been thereby retarded. We have a great deal to learn about organisms from psychology, as we have already learnt much from the physical sciences, but in neither case does this necessitate the crude and uncritical use of explanatory

tools taken directly from these related sciences. The notion of a hierarchy of sciences with mathematical physics at the bottom and biology at the top is totally misleading. Knowledge may be more correctly likened to a net. Each science is independently concerned with the construction of its own portion of the whole, and although two portions will approach one another and involve mutual aid for their completion, this never absolves each one from devising ways and means for the fulfilment of its own task.

**HUBER.** By HERBERT and EVELYN MACE. Being a Review of: **New Observations upon Bees**, by FRANCIS HUBER. A new translation from the French by C. P. DADANT, Editor of the *American Bee Journal*. (Hamilton, Illinois: *American Bee Journal*, 1926.)

THE more satisfactory the foundations, the more completely they are hidden. Modern apiarists, carrying out with ease and certainty the manipulations necessary to scientific queen-raising, have little conception of the profound mystery which enveloped the whole subject, before the untiring efforts of the great Swiss provided something sound and solid to build upon.

He was, indeed, floundering in a morass. "I had acquired," he tells us, "the knowledge of new facts; but these facts proved so contradictory that they rendered the solution of the problem still more difficult. . . . Embarrassed by so many difficulties, I was on the point of abandoning the subject of these researches."

Needless to say, this was only a moment of temporary depression. The true scientific mind is never crushed by seeming insolubility or masses of contradictions. It is never content with anything but truth; and whether or not all truth can be encompassed by human reason, it is apparent that it can never be composed of contradictions.

Amidst all his bewilderment, Huber knew by sure instinct what was the key to the whole problem. "La fécondation de la reine-abeille." The phrase haunts him. It is an obsessing nightmare. We can see him and the faithful Burnens feverishly following up a clue, examining every single bee to find out whether perchance there had been a queen present in a hive from which they had already been carefully excluded. "Burnens spent eleven days in this operation, and during that time scarcely allowed himself any other relaxation than that required for the relief of his eyes. He took in his hands every one of the bees that composed those two hives, examined carefully their proboscis, their posterior legs, and their sting."

Only two solid facts in connection with the subject had so

far been ascertained. Swammerdam had demonstrated by dissection what Butler in *The Feminine Monarchy* had asserted more than a hundred years before, that the queen was not a mere ruler of the hive, but the female parent of all its members ; and Schirach had shown that the queen is raised, at the will of the community, from the same kind of larva as a worker, merely by special diet. On these two facts a host of fantastic theories, some more plausible than others, had been built up. Huber examined them all with an openness of mind which is beyond praise, and demolished them so that not a vestige remained.

Swammerdam's own theory, that the drones gave off an *aura seminalis* which fertilised the queen, he disposed of in a single experiment. "It was necessary," says he, "to confine all the drones of a hive in a box perforated with very minute holes which would allow the passage of the odour, without permitting the passage of the organs of generation ; place this box in a well-peopled hive, but entirely deprived of males. We made this experiment, and the queen remained barren."

De Braw, an English naturalist whose name scarcely survives—it is hardly to be wondered at, seeing how superficial were his observations—had made a very plausible statement. He had seen, at the bottom of certain cells containing eggs, a whitish liquid different in appearance from the jelly given to young larvæ. He declared that this was the seminal fluid, and that he had actually seen the drones sprinkle the eggs with it. Since something of the same method is pursued by batrachians and fish, this idea gained a good many adherents.

"I resolved," says Huber, "to repeat the experiments of Mr. de Braw, and to take greater precautions than he did himself. . . . We found, Burnens and I, several cells containing an appearance of liquid, and I must acknowledge that, during the first days after making this observation, we had no doubt of the reality of this discovery : but we recognised this illusion afterwards, caused by the reflection of the rays of light ; for we could see traces of this liquid only when the sun was sending its rays to the bottom of the cells. . . . Although this observation inspired us with a sort of distrust of Mr. de Braw's discovery, we repeated his other experiments with the greatest care. . . . We examined all the bees, and made sure there was not a single drone in the hive. We examined also all the combs, and we made sure there were neither nymph nor larvæ of males in them. We adapted a glass tube to the entrance . . . and we watched this tube attentively during the four or five days that the experiment lasted. We can affirm that not a drone was seen. However, the queen laid . . . and all the worms hatched. Since the eggs weré fertile that the queen laid in a hive con-

taining no drones, it is very certain that they do not need to be sprinkled with the sperm of the drones in order to hatch."

By this time, the "sort of distrust" which Mr. de Braw had inspired had become a stronger feeling. Huber had seen how shallow the observations and experiments of this gentleman had been, and his just mind revolted against what was little better than charlatanism. "I thought that Mr. de Braw's partisans would say that the bees deprived of their drones might seek those living in other hives, remove the fertilising liquid from them, and bring it to their own home to deposit it upon the eggs. . . . It was only necessary to repeat the previous experiment, confining the bees in their hive so closely that not one of them could escape. I made sure that there was not a single drone among them; they were kept prisoners for four days, and at the end of that time I found forty worms newly hatched. I went to the length of immersing this hive again to make sure that no males had escaped my search; we examined all the bees separately, and we can assure that there was not a single one which did not show us her sting. . . . I need only indicate that which led him into error. He used queen bees with which he was not acquainted from their birth. Had he used a virgin queen he would have had a very different result."

One theory after another was sifted in this painstaking manner until there was only one possible explanation left. "The females cannot be fecundated in the interior of the hives; they must go out in order to receive the approaches of the males. It was easy to ascertain this by direct experiment."

Comparatively speaking, it was easy. Solid ground had been reached, and from that time one fact after another was built upon it. The reconnaissance flights of short duration, the half-hour's absence when the actual mating takes place, the astonishing return of the queen with the supposed coagulated seminal fluid, and the discovery of the real nature of this substance, follow naturally and sequentially.

Having planted these solid foundations, Huber might well have been pardoned for thinking his work complete, but another problem in connection with *la fécondation* was to arise, and this baffled him completely and finally. He discovered that some of his queens, after mating, laid nothing but drone eggs. Furthermore, he ascertained that there was a definite reason for this. After a long series of experiments on the problem, he concludes: "It is therefore true that when the mating of queen bees is delayed beyond the twentieth day, there is but a semi-fecundation, if one may thus call it: for instead of laying both worker and male eggs, those queens

lay male eggs only. I do not aspire to the honour of explaining this strange fact ; in all the physiology of animals I do not know of any observation bearing the least similarity to it. In natural circumstances, the queen lays worker eggs forty-six hours after mating, and continues, for the space of eleven months, to lay almost only these. It is usually only at the end of eleven months that she begins a considerable and uninterrupted laying of eggs of drones. On the contrary, when fecundation is retarded beyond the twentieth day, the queen begins to lay drone eggs from the forty-sixth hour in considerable numbers, and never lays any others during her entire life. It is clear that the worker eggs and the drone eggs are not indiscriminately mixed in her oviducts ; doubtless they occupy a situation corresponding to the laws which regulate her laying : those of workers are first, those of drones behind them. It appears that the queen cannot lay drone eggs until she has discharged all the worker eggs in her oviducts. Why is this order inverted by retarded fecundation ? How does it happen that all the worker eggs which the queen should have laid, if fecundated in time, are withered and disappear, without obstructing the passage of the drone eggs, which occupy only the second place in her ovaries ? . . . It is an abyss wherein I am lost."

Here Huber was obliged to leave the problem, which was finally solved by Dzierzon's discovery of the *spermatheca* and the control exercised over it by the queen.

*La fécondation* was, without question, the most intriguing problem. It lay right at the heart of bee life, and its true solution was essential to sound progress in bee culture ; but any other of Huber's discoveries might well entitle him to the premier position among apiarists. The confirmation of Schirach's theory and the corrections made in it, the final settlement of the vexed question of the nature of wax, the illuminating details he ascertained on the subject of the rivalry and jealousy of queens, the discovery of the means by which the overcrowded and all but hermetically sealed hive is ventilated, and the proof that pollen is required primarily for feeding the young, were all of the first magnitude.

Actually, the whole of these discoveries were made possible, or at least enormously facilitated, by an invention which, more than any other, marks the dividing-line between primitive and modern apiculture. It was his " Leaf hive " which made Huber famous, and was the germ, not only of every type of movable-comb hive, without which modern beekeeping methods would be impossible, but also of the single-combed observation hives, which enable not only the scientific, but the vulgarly curious, to see everything the bees do in perfect comfort and

without the least danger. Huber himself disclaims the invention, which he ascribes to M. Bonnet in his first letter to that gentleman. "You advised for naturalists the use of much narrower hives, the panes of which should be so close together that there would be room between them for only one row of combs." In practice, he found that in such hives the bees would not construct a single lengthwise comb, but several small transverse combs, and although he could and did arrange a comb, in such a hive, he thought this too great interference with natural instinct, therefore "I procured several small frames of pine, a foot square and an inch and a quarter thick; I caused them to be joined together with hinges, so that they might open and close at will, like the leaves of a book, and I had the two outside frames covered with panes of glass, representing the covers of the book."

Such was the work, and from the work we may judge the man, who at the almost tender age of fifteen had ruined his health and sight by reading at night, even, in default of a lamp, by the light of the moon. A sojourn in rural surroundings improved his health, and at the same time directed his attention to the beauties of nature; but the oculist had pronounced the state of his eyes beyond cure, and foretold, too truly, complete blindness. At this stage the singular habit was formed which stood him in good stead in after-years—that of seeing things through the eyes of others, and speaking as though he had actually seen them himself. If he did not, from time to time, deliberately mention his helper, Burnens, it would be impossible to discover from *Nouvelles Observations* that Huber himself saw nothing whatever.

In every sense the formation of this habit was romantic. At the age of seventeen he met Maria Lullin, and they fell violently in love. The prospect before Huber naturally caused Maria's parents to throw every obstacle in the way of the couple, but the more certain the prospect of François's blindness, the more determined was Maria to marry him. Perceiving that the calamity which threatened him was the sole cause of the opposition, Huber began to pretend that he could see more than he could. Things which were told him he told to others as though he had seen them himself, and although this did not effect his purpose and the two were compelled to wait till Maria was of full age, the seven years during which the habit was practised, rendered it permanent.

This trick of conversation, reproduced in his writings, successfully obscures from us the true facts that his reading and writing were done by his wife and the experiments carried out by the servant Burnens. Nevertheless, it is quite apparent

that his was the master-mind to seize on the essential points, to suggest lines of experiment, and to draw the correct and logical deductions from their results.

Nothing could be more fitting than that a name already associated with another great beekeeping classic should appear on the title-page of a new translation of *Nouvelles observations sur l'abeille*; for not only has Langstroth been acclaimed by his compatriots as the "Huber of America," but the chief work for which he is justly eminent was a direct continuation of Huber's practical invention of the Leaf hive.

Up to a certain point Mr. Dadant has done his work well. From his brief preface, it would seem that he is not quite conscious of the fact that what he aimed at was the only thing there was to do. He seems only to think that he is improving on former translations, but this is not quite an exact statement of the case, for the original translations of 1808 and its followers (which were, as Mr. Dadant says, to all intents and purposes the same) were made with quite a different object. Although Huber obtained more than the usual amount of fame which falls to a genius in his lifetime, he was not viewed with quite the same reverence as beekeepers must show him to-day. The first translator aimed to give the English people the benefit of Huber's discoveries, not to create a memorial to Huber. The world now knows all that Huber knew and more, and we are more interested to know about Huber, to catch something of his spirit, and to learn, if we can, the secret of his genius. Therein lies the merit of Mr. Dadant's work, for, taken as a whole, it is an exquisitely faithful reproduction of the style and manner of the great Swiss. This detracts nothing from the original translation when the purpose of each is duly weighed.

Mr. Dadant points out that the old translations were considerably abridged, which is true. It is no less true that there is scarcely one omission of any importance so far as the subject-matter is concerned. One instance we may mention is in Letter 3, where the original translator says: "I was much surprised to see the bees close up the cells at the moment when they should have been hatched, and demonstrate by anticipation that the included worms would change into drones." He has missed the important point, *i.e.* that the bees close up the cells with convex cappings (*couvercles bombés*); but in the same sentence Mr. Dadant himself is not free from blame, for he has, no doubt inadvertently, omitted the phrase "et m'apprendre d'avance que tous ces vers devoient se transformer en faux-bourçons," and also the sentence "de ne pas se laisser induire en erreur par cette circonstance." Both of these sentences are in the old translation. Mr. Dadant's omissions,

it is true, do not contain important matter, but they affect his work as much as an important one would that of the old translator when we consider their different objects.

Here and there it is evident that Mr. Dadant has been influenced by the old translator, which is scarcely surprising, since he must have studied him pretty closely. This comes out in one or two curious words and turns of phrase. Generally speaking, Dadant gives us the original word "fecundation." "Impregnation" is always used by the first translator. True, the word is sometimes employed in this sense, but not generally. The "impregnation" of a queen-bee with paraffin would hardly produce *fécondation*. In one case that I have noticed, Mr. Dadant has slipped back to the word "impregnation."

Huber's style flows easily, and as a rule lends itself to literal translation, but here and there Mr. Dadant has been led astray in his commendable anxiety to reproduce Huber's own language: *prétendue* is translated "pretended" (p. 18), when it should be "alleged" or "supposed"; *méconnue* means "unrecognised" or "misunderstood," rather than "unknown" (p. 28); and *altéré* never means simply "altered," but always conveys the idea of deterioration, hence "impaired," "impoverished," or even the original translator's "affected," according to the context, would be better.

Mr. Dadant makes few mistakes in the translation of idiom, but we think it a pity that "Les Schirach, les Riem . . ." (a quite usual idiom for "men like Schirach and Riem") should have been translated simply "Schirach and Riem" (p. 109). Perhaps we shall be thought too exacting if we mention also the sentence on p. 15: "We examined her two days later and found her fertile": "la" in the original evidently refers, not to "cette reine," but to "son habitation," and so the literal translation "we examined it . . . and found the queen fertile" is more correct.

It must be admitted that Mr. Dadant's work is, on the whole, remarkably free from this kind of thing, and it appears more so when we compare it with that of Mr. Graham Burt, who has no doubt been entrusted with the translation of chap. viii, vol. ii, because of his special knowledge of its subject-matter. Mr. Burt has somewhat missed the point. He has followed in the original translator's footsteps, by concentrating on what had already been done—giving Huber's discoveries to the English world. He omits phrases and frequently wanders from the original style. This more than once leads him into actual error. For example, at the end of the second experiment in Part II, "la consommation presque totale" he gives as "total absence." Why not "almost entire consumption"? In the tenth experiment in the same part there

is a grievous error, doubtless due to carelessness, for the "temperature of the surrounding air" is given as 140° R. instead of 14° R.!

In the fourteenth experiment considerable liberty has been taken. Mr. Burtt gives "the breathing of bees *in all their stages*" instead of "in their early stages" (*en bas âge*), and in the following paragraph is therefore obliged to translate "*étoient conservés*" as "were also to be found." It is a pity that Mr. Burtt did not keep strictly to the letter of the original, because the translator of 1808 pruned this chapter even more than the rest of the book—the first part of it, which occupies six pages of Huber, he compressed into two short paragraphs.

Perhaps the most peculiar error in the whole book is Mr. Burtt's translation of the word "altération" (p. 185). Elsewhere, like Mr. Dadant, he wrongly translates it "altered," "changed," etc., but it is quite obviously not used in the same sense here. "*Paraïssoient éprouver une forte altération*," even without the context, could hardly be anything but "appeared to suffer great thirst"; but in view of the sequence, "they licked up the moisture from the sides of the flask," it seems almost inconceivable that anyone could have put "undergo a great change."

Another grave error is on p. 192, where Mr. Burtt translates "*tendent à relever et à réhausser l'abeille par derrière*" as "enable the insect to raise and lower itself"; but all beekeepers know that the effect of the attitude is that, as the fanning becomes stronger, "the posterior of the insect is raised more and more"—which is precisely what Huber says.

There are rather too many minor blemishes in the book. *Beau* is generally translated "beautiful," when "remarkable," "striking," or "great" are obvious. "Series" for "course," "appliances" for "machines," "perforated box" for "grated box," are emendations we should like to make. Split infinitives and misprints abound: "to artificially fecundate" is almost appalling, while "to such a extent" is hardly euphonious. "Eudiometric" is made "endiometric" in every instance.

Altogether, Mr. Dadant's book is a fine piece of work which would be much improved by the vigorous use of literary sandpaper.

## REVIEWS

### MATHEMATICS

**Matrices and Determinoids.** Vol. III, Part I. By C. E. CULLIS. University of Calcutta Readership Lectures. [Pp. xviii + 681.] Cambridge: at the University Press, 1925. Price £3 3s. net.)

It is difficult to say much about this massive volume, which is only the first part of the third volume of the complete work, of which the first two volumes appeared some years ago. It is, however, complete in itself. The first three chapters are purely algebraic and deal, in great detail and with much generality, with polynomials in any number of variables, highest common factors, resultants, eliminants, discriminants. Subsequent chapters deal with matrices whose elements are polynomials, with polynomials in a square matrix, and the equation, discovered by Cayley, which is satisfied by a square matrix, and with Weierstrass's reduction of a square matrix with constant elements to a canonical form. Next comes the theory of "Commutants," i.e. of matrices  $X$  such that  $AX = XB$ , and in particular it is shown how to construct a matrix which is a commutant of every commutant of a matrix  $A$ .

Nowadays one cannot dispense with matrices, whether in analysis, geometry, or physics, but, while we think that it would do a student of quanta a great deal of good to read this monumental work, yet it would be a pity if the theory which he requires for his applications could not be got into a smaller compass.

F. P. W.

**Leçons sur la composition et les fonctions permutables.** Par VITO VOLTERRA et JOSEPH PÉRÈS. [Pp. viii + 184.] (Paris: Gauthier-Villars et Cie, 1924. Price 20 fr.)

**Leçons sur les propriétés extrémales et la meilleure approximation des fonctions analytiques d'une variable réelle.** Par SERGE BERNSTEIN. [Pp. x + 207.] (Paris: Gauthier-Villars et Cie, 1926. Price 50 fr.)

**Les fonctions quasi analytiques.** Par T. CARLEMAN. [Pp. 116.] (Paris: Gauthier-Villars et Cie, 1926. Price 30 fr.)

(Collection de monographies sur la théorie des fonctions.)

THESE three books form the latest additions to the well-known series of Borel tracts on the theory of functions. They well maintain the high standard which has been set by their predecessors, giving in lucid French the results of investigations in branches of analysis which for the most part are of recent creation and for which the respective authors are largely responsible. It only remains to give some short account of what they are about.

1. Given two square matrices  $m$ ,  $n$  of  $g$  rows and columns, their product  $\mu = mn$  is defined as the matrix of which the element in the  $i$ th row and  $k$ th column is the sum of the elements in the  $i$ th row of  $m$  multiplied respectively by the elements in the  $k$ th row of  $n$ ; in symbols

$$\mu_{ik} = \sum_{l=1}^g m_{il} n_{lk}.$$

Products so defined do not in general satisfy the commutative law; if the equation  $mn = nm$  does hold, the matrices  $m$  and  $n$  are said to be per-

mutable. The theory of matrices based upon these remarks is, of course, perfectly well known; the step which was taken by Volterra in his work on integral equations was to pass from the discontinuous to the continuous, replacing the indices by continuous variables and summations relative to these indices by integrals. Thus corresponding to the matrices  $m, n$  we have functions of two variables  $f(x, y), g(x, y)$  and to the sum  $\sum_i m_{il} n_{lk}$  an integral  $\int f(x, \xi) g(\xi, y) d\xi$ .

We are thus led to the consideration of the function  $\int_x^y f(x, \xi) g(\xi, y) d\xi$ , which is denoted by  $\overset{**}{f}g$  and is called the *resultant of the composition* of  $f$  and  $g$ . Composition so defined obeys the associative and distributive laws, but not in general the commutative law. Two functions  $f$  and  $g$  are said to be *permutable* if  $\overset{**}{f}g = \overset{**}{g}f$ . It is then clear that if we confine ourselves to permutable functions there is a complete formal analogy between composition and arithmetical multiplication. Given a set of functions permutable among themselves, it is easy to see that all the functions which can be deduced from them by composition are permutable; in particular we obtain  $\overset{*}{f}^n$ , the  $n$ th power of composition of  $f$ . Thus we pass to *polynomials of composition*, but here we must note that in the first place such polynomials must not have constant terms, the polynomials  $a + f(x, y)$  and  $b + g(x, y)$  not being in general permutable although  $f$  and  $g$  are. To avoid this case of exception we introduce the zero power of composition of a function  $f$ , defined by the rules

$$\overset{*}{f}\overset{*}{g} = \overset{*}{g}\overset{*}{f} = g(x, y),$$

whatever may be the function  $g$ , permutable or not with  $f$ . The effect of this symbol, which plays the part of a unity, is independent of the function  $f$ ; it is thus natural to denote it by a symbol  $\overset{*}{1}$ . With this convention we may include constant terms of the form  $a\overset{*}{1}$  in our polynomials. The next step is to introduce series of composition, it being proved that if in a power series in the variables  $z_1, z_2, \dots, z_p$ , convergent for sufficiently small values of the moduli, we replace  $z_1, z_2, \dots$  by permutable functions  $f_1, f_2, \dots$  and products and powers of  $z_i$  by compositions of  $f_i$  we get a convergent series, representing a function permutable with the given functions. From this we can get an interpretation of such symbols as  $(\overset{*}{1} - f)^{-1}$ , this being the same as  $\overset{*}{1} + f + \overset{*}{f}^2 + \dots$ ; and we can at once solve for  $\phi$  the integral equation  $\phi(x, y) + \int_x^y \phi(x, \xi) f(\xi, y) d\xi = g(x, y)$ . In fact, this may be written  $\phi(\overset{*}{1} - f) = g$ , whence we get

$$\phi = g(\overset{*}{1} - f)^{-1} = g + \overset{*}{g}f + \overset{*}{g}f^2 + \dots,$$

which is the classical solution.

Thus, step by step, the theory is developed in analogy with ordinary analysis, introducing the inverse of composition, logarithms of composition, and differential and integral calculus of composition. Included is an investigation of the functions which are permutable with a given function; they can be constructed by transformations applied to functions of the difference  $(y - x)$ . M. Pérès has discovered those transformations which conserve composition, and by use thereof he is able to find new results and simplify proofs. A final chapter contains suggestive remarks upon the connection between permutable functions and the summation of divergent series.

2. The lectures which M. Bernstein, of Ukraine, delivered at the Sorbonne in 1923, and which are reproduced with some additions in the second, of these volumes, were devoted to showing how the methods developed by Tchebyscheff can be applied to the study of functions of a real variable. A set of functions

$\phi_0(x), \phi_1(x), \dots, \phi_n(x)$ , bounded and continuous in a given segment, is said to form a Tchebyscheff system if an equation of the form

$$F_n(x) \equiv a_0 \phi_0(x) + \dots + a_n \phi_n(x) = 0,$$

in which the coefficients  $a_0, a_1, \dots, a_n$  are not all zero, has at most  $n$  roots in the given segment. Such expressions  $F_n(x)$  are called *generalised polynomials* of the system. Then, if  $f(x)$  is bounded and continuous on the segment, that one,  $P_n(x)$ , of the generalised polynomials for which the maximum of  $|f(x) - P_n(x)|$  is the least possible is said to be a *generalised polynomial of approximation* of  $f(x)$ , and this maximum  $M$  is called the *best approximation* of  $f(x)$  by the system considered. A fundamental theorem that such a polynomial of approximation exists and is unique for any bounded continuous function and giving a test for  $P_n(x)$  enables one to solve problems such as the following: To determine, among polynomials of degree  $n$ ,  $x^n + p_1 x^{n-1} + \dots + p_n$ , that which differs as little as possible from zero on the segment  $(-1, 1)$ . Similar problems, for rational fractions, for polynomials subject to certain conditions, for integral functions, and for analytic functions with given singularities, are then dealt with in turn. There is also a discussion of the relations between the maximum modulus of a polynomial, or an integral function, and the maximum moduli of its successive derivatives. Finally, in an appendix, real continuous functions are classified according to the order of their best approximation by polynomials of given degrees, analytic functions appearing as those for which the polynomial approximation decreases most rapidly.

3. M. Carleman's book is related to some extent to the preceding, certain classes of quasi-analytic functions being considered by M. Bernstein in the appendix above mentioned. Quasi-analytic functions have in common with analytic functions the fundamental property that they are completely determined in the whole of their domain of definition by their values in a portion of the domain however small. Examples are (i) the solutions of a linear differential equation with continuous coefficients, (ii) all polynomials, (iii) all functions analytic in a segment  $(a, b)$ . The question then arises as to the existence of a class of quasi-analytic functions which includes as a sub-group every other aggregate of this kind. The answer is, however, in the negative.

Classes of functions wider than that of analytic functions also arise in the theory of partial differential equations, namely, functions which have derivatives of any order, with limitations of the form  $|f^{(\nu)}(x)| < k^\nu A_\nu$ , where  $A_0, A_1, \dots$  is a given sequence of positive numbers and  $k$  is a constant which depends on the choice of the function  $f(x)$ . Such a class of functions is quasi-analytic if two functions of the class which are equal, together with all their derivatives, at a point of  $(a, b)$ , necessarily coincide everywhere in this interval. Denjoy showed that if  $\sum 1/\sqrt[n]{A_n}$  diverges, the corresponding class is quasi-analytic. M. Carleman investigates necessary and sufficient conditions to be satisfied by the sequence  $A_n$ , and then deals with the actual calculation of a quasi-analytic function in terms of its derivatives at a point. He also applies the theory to the problem of moments and the continued fractions of Stieltjes.

F. P. W.

**Fourfold Geometry, being the Elementary Geometry of the Four-dimensional World.** By D. B. MATH. [Pp. viii + 183.] (London: Methuen & Co., Ltd., 1926. Price 8s. 6d. net.)

Nor very much need be said of this book. It would seem to be a confused treatment of a Euclidean (*not* Euclidian) space of four dimensions with which time and gravitation have somehow or other got mixed up. The locus:

$x^3 - x^2 - y^2 - z^2 = k$  of course comes in ; it is stated to resemble " an hour-glass as nearly as a three-dimensional locus can resemble a two-dimensional one."

**Traité du calcul des probabilités et de ses applications.** Tome II, fasc. 1 : **Applications à l'arithmétique et à la théorie des fonctions.** Par ÉMILE BOREL. [Pp. 102.] (Price 22 frs.) Tome IV, fasc. 1 : **Applications au tir.** Par J. HAAG. [Pp. vi + 183.] (Price 25 fr.) (Paris : Gauthier-Villars et Cie, 1926.)

THE first of these two new parts of Borel's encyclopædic work on probabilities is slight enough, but it makes pleasant reading. The type of problem is that of determining the frequency of the occurrence of a certain figure, say 3, in the expression of a number as a decimal, or the distribution of the intervals which separate identical figures or groups of figures. What are called " denumerable probabilities " are introduced as a half-way house between discontinuous and continuous probabilities, the number of possible cases or the series of trials or both forming a denumerable aggregate. Similar problems on the quotients of a continued fraction are likewise discussed. The applications to the theory of functions are to questions of the measure of aggregates for which a function possesses a certain property ; the phrase " almost everywhere," with which we are nowadays so familiar, and our old friend the multiplicative axiom are naturally much in evidence.

M. Haag says that his book is primarily intended for artillery officers, and he apologises in advance if the mathematics is too refined and out of all proportion to the precision which can be obtained in practice. But as this is part of a larger work he has felt bound to write " dans la tonalité générale," and, anyhow, the simple results and the numerous tables and diagrams will appeal to the practical man. But no doubt many of our professional mathematicians who, like M. Haag, found themselves dealing with this kind of question during the war will find much to interest them in this account. And who could resist a problem such as the following ?—A sportsman shoots at a hare weighing 3 kilos from a distance of 30 metres with a charge of 32 grammes of lead number 4 in a rifle of calibre 12 with a cylindrical bore. What chance has he of killing his hare ?

F. P. W.

**The Dial Machine, an Apparatus for the Elementary Mathematical Laboratory.** By S. J. C. ELLIOTT. [Pp. 98, with 2 full-page illustrations.] (Peterborough : The Peterborough Press. Price 4s. 6d.)

THIS book gives a detailed description of the construction and use of a machine for the practical teaching of mathematics in schools and colleges. It involves an entirely new approach to mathematics by way of the theory of functionality. Its use will at least repay both teachers and student in the demands it makes on mental elasticity and adaptability.

W. C. B.

**Elements of Mathematics for Students of Economics and Statistics.** By D. CARADOC JONES, M.A., and G. W. DANIELS, M.A., M.Com. [Pp. viii + 240.] (Liverpool : at the University Press ; London : Hodder & Stoughton, 1926. Price 8s. 6d. net.)

THIS textbook has been compiled to meet the requirements of all who have occasion to deal with statistics in the course of their studies or work, and is strongly to be recommended on the one hand to students of economics to enable them to take a sound logical view of their work, and also to mathematically minded men in control of great economic issues. It shows, indeed, the need for such men to be able to view their work from the mathematical standpoint.

W. C. B.

**PHYSICS**

**Practical Physics.** By T. G. BEDFORD, M.A., F.Inst.P. [Pp. x + 425, with 225 diagrams.] (London: Longmans, Green & Co., Ltd., 1926. Price 10s. 6d. net.)

It is a pleasure for one who has been in any way associated with the teaching of practical physics at the Cavendish Laboratory to welcome the publication of Mr. Bedford's book. It is an additional pleasure to read such a balanced and reasoned exposition of practical physics; the very appropriate chapter of introduction may be strongly recommended to all students.

The experiments described in this book are mainly those carried out in the author's laboratory at Cambridge, and form part of the preparation for Part I of the Natural Sciences Tripos. The course is designed to meet the requirements of students at various stages, including those who come from schools which possess well-equipped laboratories, and those having a very slight knowledge of practical physics. The result is that a number of simple experiments, such as those which involve the use of pins in elementary optics, have been included in this book for the benefit of the latter class of students. Again, the course does not include all the experiments which a student must carry out during his training for a degree with honours in physics. For example, the experiments on lenses are confined to thin lenses, and no mention is made of induction experiments, the platinum thermometer, thermo-electric effects, and the use of the quadrant electrometer. This is unfortunate, because the reasonable price at which this book is published places it within the reach of a very large number of students, and it is highly desirable that they should possess a textbook which covers as much of their advanced work as possible. Mr. Bedford's book forms such an excellent foundation for the student that we very much hope that in future editions he will make the few additions which will render the book so much more valuable to a wider range of students.

The book is well printed and illustrated; it is certain to become very popular, and we wish it every success.

L. F. BATES.

**Müller Pouille's Lehrbuch der Physik.** Elfe Auflage.

Zweiter Band; Erste Hälfte: "Lehre von strahlenden Energie." [Pp. xviii + 928, with 624 figures and 7 plates.] (Price R.M.50, geb. R.M.54.)

Dritter Band; Erste Hälfte: "Thermodynamik." [Pp. xviii + 1185, and 575 figures.] (Price R.M.63, geb. R.M.68.) (Braunschweig: Friedr. Vieweg & Sohn, 1926.)

THE first part of the second volume of this great work was largely written by Prof. Otto Lummer, who died in July last whilst his contribution was in the press. The editors have accordingly published his work without making any changes beyond the necessary proof corrections, which were made by Lummer's assistant, Dr. H. Lessheim. The book before us commences with a chapter by Lummer on the nature of light and the conception of the ether, and we have a very interesting survey of the changes which the conception of the ether has undergone in the course of time. This is followed by a discussion of the velocity of light in stationary and moving media, the velocity in moving media being dealt with by F. Jüttner of Breslau. Particular attention is directed to the explanation of the Michelson-Morley experiment, but very little mention is made of Miller's experiments, only the 1922 experiments being recorded. We then have an extensive treatment of the reflection and refraction of light at plane and curved surfaces, followed by chapters on dispersion, which include descriptions of modern methods for the determination of the refractive index. The article on anomalous dispersion,

however, contains very few references to really recent work, the latest reference being to work done by Kent in 1919.

H. Klenkhardt contributes a short section on the influence of various factors such as pressure, temperature, etc., on the index of refraction, and also deals with such cognate subjects as the index of refraction of mixtures. Huyghens' principle and allied principles, together with their applications, are considered in the eighth chapter, which is followed by two chapters on the effects of spherical and chromatic aberration. A very large section, nearly 170 pages long, is devoted to the human eye considered as an optical instrument, and the writer, M. v. Rohr of Jena, includes an account of the Gullstrand ophthalmoscope. E. Schrödiger of Zürich contributes a very complete article on physiological optics.

Optical instruments are very adequately treated by A. König of Jena, whose article includes descriptions of projectors, range finders, and methods of instrument testing. Naturally, considerable portions of the book are devoted to interference and diffraction phenomena, and descriptions of modern interferometers are given. Finally, the book closes with a chapter on the theory of formation of images in a microscope, and a description of the ultramicroscope is given.

*Thermodynamik* is likely to be of more interest to English readers than the book which has just been reviewed. In the first place, it is a companion book to Herzfeld's *Kinetische Theorie der Wärme* which was reviewed in this journal last year, and which was welcomed as an excellent contribution to our sources of information on that subject. In the second place, an attempt is made in this book to give the general reader of physics a conception of the important methods and appliances which have been developed in the technical applications of the theory of heat.

The book opens with an excellent description of the general principles and experimental methods of thermometry and calorimetry, particular attention being given to sources of error and to necessary corrections which occur in the measurement of temperature changes and of quantities of heat. For instance, the correction of the observed rise in temperature obtained from the actual data of a calorimeter experiment, is calculated in three different ways. We then have a chapter on the first law of thermodynamics and its applications to isothermal chemical changes and to the heat of chemical combination, the experimental investigations again being carefully described. An exhaustive chapter follows on the second law of thermodynamics and its applications, and includes a section on the Nernst heat theorem.

A large portion of the book is devoted to the discussion of the thermal behaviour of gases, of liquids, and of solids. In it full descriptions of the measurement of specific heat, thermal expansion, and compressibility are given. It is here that we find an account of van der Waal's theory, together with a short review of the various attempts which have been made to elaborate this theory, particular attention being paid to the work of van Laar, whose book on the equation of state of gases and liquids appeared in 1924. The section on the Joule-Thomson effect is contributed by Dr. Pollitzer.

Under the heading of heterogeneous single component systems we have, firstly, a discussion of the equilibrium between the various states of aggregation, and, secondly, a discussion of the thermodynamic relations concerning latent heat and its experimental determination. Thirdly, we have a discussion of surface phenomena, wherein the methods for the measurement of the surface tensions of liquids are fully described. In the section on the velocity of melting and solidification of crystals, Czochralski's method of making single crystals of a metal is described.

Under the heading of homogeneous systems of many components, there is an introductory section on the properties of mixtures and solutions, which

is followed by a discussion of the partial pressure of mixtures, osmotic pressure (including the osmotic pressure of strong electrolytes), diffusion of gases and liquids, and the law of mass action and its experimental investigation. The phase rule and phase diagrams are dealt with in a very comprehensive section contributed by F. Sauerwald. Special two-phase systems and the phenomena of adsorption are treated in succeeding chapters.

W. Jacob, of Charlottenburg, contributes an excellent chapter on thermal conductivity, and this is followed by a large section devoted to the technical applications of heat. Thus we have chapters, written by Dr. Pollitzer and Dr. Zerkowitz, on the thermodynamics of heat engines and turbines, the mechanical production of cold, the liquefaction of gases, and the separation of mixtures of gases by mechanical means. Finally, the book closes with a chapter on the physiological aspect of heat.

An important feature of the book is the provision of a list of the symbols used throughout and their meanings, which is of assistance to readers who wish to refer quickly to particular sections. A table of the symbols used in the current literature to denote the more important thermodynamic quantities, is also given.

Both books are excellently printed and illustrated, full references are everywhere given, and, particularly the one on thermodynamics, form very valuable books of reference.

L. F. BATES.

**Handbuch der Experimentalphysik.**—Wien-Harms. Band XIV. P. Lenard v. A. Becker, "Kathodenstrahlen." W. Wien "Kanalstrahlen." [Pp. xiv + 786, with 352 illustrations.] (Leipzig: Akademische Verlagsgesellschaft M.B.H. Price R.M.70, geb. R.M.72.)

THE names of Lenard and Wien are so well known in England that this volume is certain to attract a great deal of attention. In this beautifully printed and illustrated work we have a book which gives in accurate detail all the important experimental work which has been carried out on cathode rays and positive rays, and it is only when the work on these rays is summarised by two such distinguished pioneers that one realises the immense amount of knowledge which the study of these rays has given us.

Lenard and Becker open their contribution with a survey of the earlier experiments on cathode rays and their subsequent development. They then deal with the methods of production of cathode rays, including the slow rays produced by photo-electric emission, and describe the methods by which their intensity and velocities are measured. Then follow chapters on the loss of velocity of the rays in passing through matter, limiting thickness, absorption, decrease of intensity, secondary radiation and diffusion. Each such chapter opens with a definition, the necessary conditions for exact experimental work are then set forth, examples of the experimental procedure are given, the experimental results are discussed, and the mathematical representation of the phenomenon under discussion is given. Throughout the book the authors emphasise strongly that absorption of cathode rays is to be recognised as a process quite distinct from diffusion and from decrease of velocity. This contribution closes with a very interesting chapter on the ratio of the charge to the mass of the electron.

Wien's contribution is, of course, confined to positive rays which are produced in discharge tubes, and he gives us an admirable treatise which is profusely illustrated with plates and diagrams.

L. F. BATES.

**Leçons de Cristallographie.** Par GEORGES FRIEDEL. [Pp. xix + 602, with 578 figures.] (Paris: Berger-Levrault, 1926.)

WE like the "Avertissement" of this book best. Its simple candour (we almost said animosity) is such that we know from the outset exactly where we

stand. M. Friedel believes that "the literature" is the bane of the young researcher, but that fortunately our French co-workers are as yet fairly free from delusions ("au moins dans notre pays où l'on cherche avant tout la clarté") with regard to "ce babélisme menaçant." He deplores "les mauvais travaux de l'école moutonnaire," and that "la mode" (of searching, before commencing a new research, through piles of "cette poussière") "en est venue du dehors jusque chez nous." "Une telle méthode tend à nous ramener au bavardage livresque du Moyen Age et à étouffer toute originalité," and he is determined to make a stand against it. *Leçons de Cristallographie* is the result.

But having thus summarily disposed of the literature, it is clear from the size of the book that the author himself has become acquainted with much that is not "poussière"—at least, if it is, it is of a peculiarly bulky kind. And, if he will pardon us for saying so, it is equally clear from the literature that though undoubtedly many of the great founders of crystallography were Frenchmen, most of the recent phenomenal development of the subject is due to other countries (? "l'école moutonnaire"). But let us drop such a "bootless inquisition." Crystallography has, from whatever cause, become an enormous subject, and even M. Friedel's large volume is, as its title suggests, no more than "leçons de cristallographie." For instance, it does not include any discussion at all of the optics of crystals. But the author, chiefly for the benefit of young crystallographers, has touched upon many exceedingly interesting problems, and the book will well repay study. It cannot be denied, however, that a not inappreciable part of the discussion is almost exclusively from the author's own point of view, notably in the section dealing with the diffraction of X-rays by crystals. He has his own treatment of this phenomenon, and it is not the one usually accepted. It is to be feared that M. Friedel belongs to that school of crystallographers which regards X-ray analysis with grave suspicion. He is firmly convinced, as his countryman Mallard was, that quartz as we know it is not really trigonal but a complex submicroscopic twinning of orthorhombic or monoclinic individuals. He considers the X-ray interpretation of its structure as valueless. Fortunately, there are a few X-rays analyses to which he grants his reluctant approval.

M. Friedel is frankly an iconoclast, but there is no doubt that in his *Leçons de Cristallographie* he has produced a refreshing and stimulating work. It is not difficult to write a textbook of crystallography that is dull, but it is by no means an unexciting subject. It is impossible not to admire the way in which M. Friedel deals with it. His intention was to stimulate originality. His readers will gather the impression that there is still a great deal of work to be done before everyone can feel happy. From that point of view, then, he has achieved his purpose.

*Leçons de Cristallographie* is divided into two main sections—(1) *Étude du Cristal*, and (2) *Étude des Édifices Cristallins Complexes et des Transformations*. The first 250 pages of Part I comprise a fairly complete sketch of geometrical crystallography, while the remainder are devoted to a discussion of three important branches of physical crystallography—(a) growth and solution of crystals, (b) X-ray diffraction in crystals, and (c) cohesion and cleavage. To our knowledge of the first of these branches M. Friedel himself has made some important contributions, and, as mentioned above, he has developed an independent theory of the second. The second main part of the book is chiefly occupied with a discussion of twins and polymorphism. Some hundred pages are devoted to the study of twins alone, an essay which the student will find very useful.

Though we cannot agree with all the opinions expressed therein, we can thoroughly recommend this book to the earnest student of crystallography.

W. T. ASTBURY.

**Popular Experiments in Dynamics.** By G. C. SHERRIN. [Pp. viii + 64, and 29 diagrams.] (London: George Philip & Son. Price 2s. net.)

MR. SHERRIN has also issued a textbook to explain the use of mathematical apparatus. From a set of simple materials, a number of practical models can be made to illustrate dynamical laws chiefly as applicable to astronomy. The experiments on the gyroscope are especially interesting.

W. C. B.

**Physics in Industry.** Lectures delivered before the Institute of Physics. By WALTER MAKOWER, D.Sc., and BERNARD A. KEEN, D.Sc. Vol. IV. [Pp. 63.] (London: at the Oxford University Press, 1926. Price 3s. net.)

Lecture VIII. Physics in the Rubber Industry, with special reference to Tyre Manufacture. By Walter Makower, M.A., D.Sc., F.Inst.P., with an introduction by the Hon. Sir Charles Parsons, K.C.B., M.A., LL.D., D.Sc., F.R.S.

Lecture IX. The Physicist in Agriculture, with special reference to Soil Problems. By Bernard A. Keen, D.Sc., F.Inst.P., with an introduction by Sir Daniel Hall, K.C.B., LL.D., F.R.S.

THESE lectures, like their predecessors, will attract a wide circle of readers. The general reader who is neither a physicist nor a tyre manufacturer or farmer cannot fail to find a new interest in many phenomena which previously seemed commonplace, everyday happenings. The bending of the walls of a pneumatic tyre or the tilling of the soil may not seem very entrancing subjects, but it is only necessary to read this little volume to have the curiosity thoroughly awakened, and incidentally get some insight into the reasons why tyres must be kept properly inflated if they are to wear well, or why the farmer must be for ever repeating the tiresome operations of soil tilth.

Those who have the control of industry will find much that is stimulating within these covers. It is greatly to be hoped that the plea for more room for men with scientific training on Boards of Management will not fall on deaf ears. The matter is of first-rate importance, both for the earning capacity of each industry and for the prosperity of the country as a whole.

The physicist who reads these lectures cannot help being moved with both pleasure and admiration. It has been said that brilliance consists less in doing something that no one else could have done, than in doing something which many might have done, but, for lack of imagination, did not. Judged by this standard the work described in this volume must be acclaimed brilliant, since for the most part the physical principles used are those with which quite elementary students are familiar. Nevertheless, the results obtained are likely to be far-reaching.

R. K. SCHOFIELD.

## CHEMISTRY

**The Chemistry of Cellulose and Wood.** By A. W. SCHORGER, Ph.D. [Pp. xiv + 596.] (London: McGraw-Hill Publishing Co., Ltd., 1926. Price 30s.)

A BOOK on the very complicated subject of the chemistry of cellulose and wood from such an experienced worker as Dr. Schorger will be welcomed by teachers, students, and industrial chemists alike. The evidence with regard to the constitution of the lignin complex and its relationship to cellulose in lignified tissue or wood is so conflicting as to leave most readers in a state of complete confusion, and it was high time that the ever-increasing literature on the subject was reviewed by an impartial and experienced observer who could state both sides and present a reasoned summary of the knowledge so far acquired. That the subject is in a state of flux is fully recognised by the

author, and for this reason he has largely avoided categorical statements. The book is divided into sixteen chapters, and it may be fairly said that no aspect of the subject has been shirked. The first chapter on the structure, formation, and physical properties of wood pays attention to the biological aspect of the subject, an aspect which, be it noted, receives attention wherever possible in later chapters also. The next chapter deals with the composition of wood, and then follow chapters on lignin and the colour reactions of wood. Then follow six chapters on hemi-celluloses, the constitution of cellulose, gelatinised cellulose, oxycellulose, the action of acids on cellulose, and the saccharification of cellulose and wood. The remaining chapters are devoted more to wood under the headings of the action of various reagents on wood, pulp processes and wood pulps, the distillation of cellulose and wood, the fermentation of cellulose and wood by bacteria and fungi and their digestion by animals. The last chapter is on analytical methods, an aspect of the subject to which the author has himself contributed much valuable information. The book can be thoroughly recommended as being a most useful contribution to the literature of the subject with which it deals.

P. H.

**Potentiometric Titrations : A Theoretical and Practical Treatise.** By DR. I. M. KOLTHOFF and N. HOWELL FURMAN, Ph.D. [Pp. xii + 245, with 45 figures in the text and Bibliography and Tables in Appendices.] (London: Chapman & Hall, 1926. Price 22s. 6d. net.)

THE book is divided into two parts which deal respectively with the theoretical and practical considerations involved in potentiometric titrations. The book is written primarily for the student of analytical chemistry. Hence the theoretical considerations given in the first part are designed to give the reader a simple working picture which will help him in practical analytical work. They are not such as will stimulate him to reflect upon the inner workings of electrolytic dissociation or electrode potential. This is not the aim of the book.

From the practical standpoint the authors make a very thorough survey. The book will be readily followed by students of analytical chemistry who have a good general grounding, and they should find it useful both as a textbook and a book of reference. Potentiometric titrations are not likely to be undertaken by students taking a general course in physical chemistry, as the apparatus required is elaborate, and the principles involved can be illustrated more easily in other ways. The method is chiefly of use where similar analyses have to be performed continually, as in an analytical or research laboratory. Nevertheless, those engaged in teaching will find much in the second part that will be helpful in the design and construction of laboratory apparatus.

R. K. SCHOFIELD.

**Pyro-sols.**<sup>1</sup> By RICHARD LORENZ, Dr.Phil., Dr.Ing., etc., and WILHELM EITEL, Dr.Phil., etc. Monographs on Colloid Research. Edited by R. ZSIGMONDY, Vol. 4. [Pp. ix + 289, with 20 plates and 64 figures.] (Leipzig: Akademische Verlagsgesellschaft. M.B.H., 1926. Price 18M. in paper cover and 20M. in cloth.)

THE title of the book has been treated by its authors in a broad and comprehensive way. Thus only one of seven sections deals with systems which, strictly speaking, come under the head of pyro-sols—that is, systems in which colloidal metals are dispersed in molten media. Other sections treat of colloidal metals in solid media, and of dispersoids in minerals and slags. The authors have, moreover, linked up these phenomena with those associated with the

<sup>1</sup> The book is written in German, the title being "Pyrosole."

formation of latent photographic images, by considering the formation of metallic colloids by the action of radiation. They also devote a section to a discussion of the properties of subhalides, part of which has been written by Prof. A. Magnus, who discusses these substances in the light of modern atomic theory.

Its comprehensive character adds greatly to the value of the book, especially as Prof. Lorenz can write on all the subjects with the authority of an original investigator. The many references given, both in the historical introduction and throughout the book, further enhances its value. These incidentally testify to the importance of the contribution made, both by the authors and the editor of the series of monographs, to our knowledge of the properties of colloidal metals. Advanced students and those engaged in research on colloids will have reason to be grateful to Prof. Lorenz and his able collaborator for assembling in one volume material which previously could only be found scattered through the periodicals of the last thirty years.

R. K. SCHOFIELD.

**Chemistry of the Proteins.** By DOROTHY JORDAN LLOYD, M.A., D.Sc., F.I.C. Introduction by SIR FREDERICK GOWLAND HOPKINS, M.B., D.Sc., F.R.C.P., F.R.S. [Pp. xii + 279.] (London: J. & A. Churchill, 1926. Price 10s. 6d. net.)

DR. JORDAN LLOYD'S book is a very welcome addition to the literature of this subject. It is divided into two parts; the first part deals with the structural chemistry of the proteins, and the second with their physical chemistry. At the end of each chapter is a very complete bibliography.

In the first part are found chapters giving an account of the units of the protein molecule, together with methods of analysis and identification, while the last two chapters deal with the chemistry of protein foods and problems of food preservation. The second part, dealing with the physical chemistry of the proteins, follows on naturally from the first part; many examples are given here of the uses of proteins in industry.

The book is well illustrated with a number of graphs. The relationship of the subject-matter of this book to biochemistry and physiology is clearly indicated throughout.

J. N. E. D.

**A Textbook of Organic Chemistry.** By DR. JULIUS SCHMIDT. English Edition by H. GORDON RULE, Ph.D., D.Sc. [Pp. xxiv + 798.] (London: Gurney & Jackson, 1926. Price 25s. net.)

THE translator has rendered a service to students of chemistry in making available the contents of this work in English. The book gives a systematic treatment of organic chemistry. The number of different classes of compounds which are dealt with is very large; consequently, while some classes are very adequately dealt with, others have very little space allotted to them. The book is well printed and clearly illustrated with structural formulæ, and is very readable, giving as it does a general treatment without burdening the mind with too much detail.

J. N. E. D.

**A Textbook of Organic Chemistry.** By JOHN READ, M.A., Ph.D., B.Sc. [Pp. xii + 679.] (London: J. Bell & Sons, 1926. Price 12s. 6d. net.)

THE first part of this book is devoted to an historical development of the subject. Then follows an introduction to practical methods of organic chemistry, including the determination of molecular formulæ: the idea of structural formulæ is introduced at an early stage. Parts III and IV deal

respectively with aliphatic and cyclic compounds. There are a number of questions at the end of the chapters.

In a book of this size, covering as it does so much elementary work, it is natural that considerable selection of subjects has taken place; thus the sections dealing with dyes and drugs are short. The author has a very pleasant style, and explains the general principles of organic chemistry very clearly, and with a modern outlook.

J. N. E. D.

**Chemistry and Recent Progress in Medicine.** By JULIUS STIEGLITZ. [Pp. viii + 62.] (London: Baillière, Tindall & Cox. Price 7s. net.)

THIS most entertaining little book comprises the Charles E. Dohme Memorial Lectures delivered at the Johns Hopkins University in May 1923. The aim of Prof. Stieglitz has been to indicate the directions in which the methods and fundamental principles of chemistry have been utilised for the advancement of medicine and to emphasise the necessity for more intensive co-operation to ensure real progress in the biological and medical sciences.

The subject is divided into two main branches, the first dealing with "the service of preparative chemistry" and the second with the study of the functions of the elementary materials of the body organism from a mainly physico-chemical standpoint.

The problems involved in the use of dye-therapy are first discussed. Then follows a necessarily rather summary account of the products of internal secretion of the body. This section is in itself a striking illustration of the rapid progress effected in this field with the aid of chemistry.

Since the writing of the book, the question of the constitution of thyroxine has been settled by the work of Harrington, the mechanism of the action of insulin is more clearly understood, and, with regard to the ovarian hormone, it is apparent, in the light of recent research, that we can no longer implicitly accept the follicle as being the only seat of formation of the hormone.

In the early stages of the book, Prof. Stieglitz claims "a good share" of the recent work as done in America, and he proceeds accordingly throughout the greater part of the book to draw his illustrations from the researches of American workers. This is, perhaps, only natural, remembering that the lectures were delivered to an American audience; but when he summarily dismisses the subject of vitamins in a short section confined to the American researches on the types of "Bios," we might reasonably wish that he had extended his remarks to embrace a little of the important work on vitamins carried out here and on the Continent.

The remainder of the book is concerned with the second branch,—the application of the principles of physical chemistry,—and includes a simple exposition of the Donnan membrane theory with an example showing its significance in physiology, and a very useful survey of the recent work on the all-important subject of biological oxidation.

To those workers in the fields of chemistry and medicine whose attention has hitherto been mainly restricted to their own respective subjects, this little book is gladly recommended as a pleasant introduction to the essential investigations progressing under the combined efforts of the two sciences.

C. E. G.

**Carbohydrate Metabolism and Insulin.** By PROF. J. J. R. MACLEOD. [Pp. xii + 357, with 33 figures and 7 plates.] (London: Longmans, Green & Co. Price 18s. net.)

It is with confident anticipation that the workers in the field of physiology await the publication of each new addition to the excellent series of mono-

graphs edited by Prof. Starling. The inclusion of a book in this series is, in itself, almost a guarantee of its excellence, and Prof. Macleod's book more than justifies its inclusion.

It would have been difficult to find anyone more eminently suited to the task of introducing order into the confused mass of literature on the subject of carbohydrate metabolism that has appeared since the discovery of insulin.

We are inclined to feel, perhaps, that the author has been somewhat too unbiassed in the presentation of the material, and that a little more criticism and expression of opinion would have been of advantage to the student and welcomed by the general reader.

The book opens with a preliminary account of the structure and histology of the islets of Langerhans. A convincing summary of the evidence proving the islets to be the main site of origin and storage of insulin is given, but only brief reference is made to the researches of other workers on the presence of the hormone in the acinar tissue or the tissues of other organs. The early efforts to extract the anti-diabetic principle are described in an interesting résumé leading up to the work of Banting and Best, and a short chapter on the methods of preparation and the properties of insulin is included. The effect of insulin on depancreatized dogs is discussed in some detail.

With regard to the difficult problem of the mechanism of the action of insulin, in spite of the extensive and often unsatisfactory literature, Prof. Macleod, in the chapter in question and the allied chapters on the influence of insulin on glycogen and respiration, presents in as clear a manner as possible the evidence for the various theories relating to the subject. He himself supports the view that the disappearance of the blood-sugar in the normal animal under the influence of insulin is associated with its conversion to some intermediate non-carbohydrate substance. It is to be regretted, however, that the recent elucidating researches of Best and his collaborators were not published in time for inclusion in the book. Using the spinal eviscerated animal, these workers have been able to account satisfactorily for all the sugar lost by increased muscle glycogen and oxidation.

The complicated, but very interesting, inter-relationships obtaining between the other ductless glands and the islets of the pancreas are briefly discussed. The last chapter deals with the standardisation and methods of assay of insulin; this has become an increasingly important point with the widely spreading clinical use of insulin.

The text is adequately illustrated with plates (micro-photographs of pancreas) and graphs, and an extensive bibliography is provided at the end of each chapter. Even within the short time since its publication, the book is fast becoming the standard work on the subject. It is probably rather too detailed for the average student of physiology or medicine to digest in his limited time, but for the advanced worker, and those more particularly interested in the subject, it may be regarded as essential.

C. E. G.

## **BOTANY AND AGRICULTURE**

**Soil and Civilisation: A Modern Concept of the Soil and the Historical Development of Agriculture.** By MILTON WHITNEY. [Pp. x + 278, with 32 figures.] (London: Chapman & Hall, 1926. Price 15s. net.)

THE old idea of the soil as static, as dead, inert and simple, is being replaced by a newer conception of it as dynamic, with functional activities that interact and affect its productive power in respect of plant growth. Consequently a fundamental change is occurring in the way the possibility of this continued productivity of the soil is regarded, and it seems probable that a regenerative power exists that admits of indefinite crop production, provided that correct methods of working are adopted. Soils of different types are adapted to particular crops, and this is illustrated by reference

to the more important soils of the United States. In order to get the best results from any soil various methods are applicable, in addition to manuring devised merely to return the elements of plant food abstracted by crops. Rotation, adaptation of crops, cultivation methods, irrigation, drainage, and appropriate balancing of organic and chemical fertilisers, are all recognised to be agents that play an important part in determining the fertility of the soil. The soil, the crops, and the fertilisers used are all correlated and interdependent, and the great advance in our knowledge of the use of chemical manures has not lifted the burden of responsibility from the cultivator, but if used intelligently has supplied him with an effective means of maintaining agriculture in an economic position.

Most civilised countries, at one time or another, have developed well-organised agricultural systems, which have died out in many cases with the decline in the prosperity of the nation or with a change in the character of the people. When sedentary races were replaced by nomadic tribes, as in Asia Minor and Mesopotamia, the agriculture of the country naturally deteriorated. Another cause of decline was the failure to maintain the engineering and irrigation works that are essential in some cases, as in Spain. Where a long consecutive agricultural history can be traced, no radical change in the political history has occurred. In China and Japan the land still responds in productivity to the centuries of hard work expended upon it, while in Egypt the flooding of the Nile provides natural and unfailing irrigation, which maintains the fertility of the surrounding country.

It is noteworthy that modern agricultural practice is still similar in essentials to that of the ancients, though easier and more effective methods are available, owing to the great advance in our knowledge of such agents as chemical fertilisers, steam, and electricity during the past century. We are only at the beginning of the application of these discoveries, and it remains to be seen how far agricultural progress will be hastened by their aid. The possibilities of research have barely been tapped, and great improvement may perhaps be expected in such staple crops as wheat, cotton, corn and oats as scientific principles of breeding and cultivation are more extensively applied.

W. E. B.

**Aims and Methods in the Study of Vegetation.** Edited by A. G. TANSLEY and T. F. CHIPP. [Pp. xvi + 384, with 62 figures, including many photographic plates.] (Published by the British Empire Vegetation Committee and the Crown Agents for Colonies. Price within the British Empire, 2s. 6d.; in the United States of America, \$4.)

ONE result of the Imperial Botanical Conference held in London in 1924 was the setting up of a British Empire Vegetation Committee to encourage and promote the survey and study of the vegetation of the Empire. This work is the first-fruit of the activity of this Committee. It was realised that the British Empire, so far as the great asset of natural vegetation is concerned, was in the position of a stores manager carrying on the management of a large general store without an adequate knowledge of the stock at his disposal. In the work of recording and investigating the vegetation of different parts of the Empire, it is not sufficient merely to compile an inventory of its distribution and quantity. If it is to be utilised to the best advantage, it is necessary also to study its behaviour and potentialities before it is exploited. And this applies not only to those areas of forest and grassland which are directly exploited by man; in regions where the natural vegetation is destined to be replaced by man's crops, a study of the natural vegetation is of great value in forming an accurate judgment of the agricultural possibilities of the land. Much of the actual survey work has already been done by botanists, foresters, grazing and agricultural officers and amateurs resident in different parts of the Empire. A great deal yet remains, and the object of this book

is to put into the hands of potential workers an account of methods of investigating natural vegetation, with indications of the practical utility as well as the scientific interest of the study. In the present state of the subject it was impossible to produce a standardised and exhaustive handbook of methods. What has been done is to provide a statement of what is meant by the study of vegetation, together with some account of the methods which successful workers have used. This has entailed the division of the book into three parts. Part I gives an outline account of the best way to analyse vegetation into units which can be conveniently dealt with, and describes the methods which may be adopted in the actual study of particular examples. Methods of study of the ecological factors, climate, rainfall, physiography, soil, and the effects of human activity on vegetation are dealt with in the most practical manner, and then there are articles by specialists on certain groups of lower plants which call for special methods of study. Part II consists of a number of chapters, contributed by expert workers, on the aspects of vegetation survey in geographical regions which call for special treatment, the tropics receiving the largest attention; while Part III deals similarly with particular types of vegetation such as forest, grassland, and sand-dune country. This method of treatment has of necessity involved a certain amount of repetition and some difference of opinion. Having regard to the variable nature of the work, this is not to be regretted, serving, as it does, to emphasise the fact that different problems call for different methods of attack. Without regarding it as final, but rather for the sake of practical convenience, the editors have adopted the scheme of Dr. F. E. Clements for the nomenclature and classification of plant communities.

The book is well illustrated with photographic plates, maps, and figures. With the object of supplying it at as low a price as possible, it is being distributed through voluntary distributors in different parts of the Empire instead of through booksellers in the ordinary way.

B. J. RENDLE.

**General Botany, with Special Reference to its Economic Aspects.** By C. STUART GAGER, Ph.D., Sc.D., Pd.D. With three chapters on Heredity and Variation in Plants by ORLAND E. WHITE, Sc.D. [Pp. xvi + 1056, with 689 illustrations.] (Philadelphia: P. Blakiston Son & Co., 1926. Price \$4.00.

THE first part of this book is written very much on the same lines as the author's much smaller stimulating work, *The Fundamentals of Botany*, but the emphasis here is placed on the economic aspects of this subject. It is a welcome innovation to find, in a work of this size and scope, so much space given to the historic aspects of the subject. This, while helping to give a proper perspective to the present content of botanical knowledge, increases the interest of the reader. The statistical presentation of facts is also to be commended, especially those given in the sections bearing on physiology. Another useful point is that footnote references are made to new work, especially where the text is unable to give a detailed account of the matter in hand. This feature should prove useful to students and teacher alike; but in the opinion of the reviewers it has led in some cases to the inclusion of facts too advanced to be adequately understood by a student at this stage of study, and in others to a rather one-sided view of the possibilities, as is the case on p. 181, where the steps in starch-formation are dealt with.

The treatment of anatomical structure is inadequate. The descriptions of the structural elements is meagre, and it is difficult to believe that a student could understand the primary structure of stem and root, far less the difficult matter of secondary thickening, from the descriptions and figures given. The descriptions of Figs. 160 and 163 suggest by implication that phloem and cork have definite annual rings.

The second portion of the book dealing with the systematics of economic plants is well done, and as fully and beautifully illustrated as the rest of the book; historic sources such as Dodoens' *Cruydeboeck*, Gerard's *Herball*, and Baillon often being made use of. It would be necessary to supplement the descriptions of the characters of the orders and families from other sources.

A few adverse criticisms have been made, but the fact remains that we have here a work which should interest and stimulate the ordinary reader as well as the student of Botany.

E. M. C.

**A Practical Introduction to the Study of Botany** (specially intended for the use of Indian students). By J. BRETLAND FARMER, M.A., D.Sc., LL.D., F.R.S., and HARAPRASAD CHANDHURI, M.Sc., Ph.B., D.I.C. New Edition. [Pp. viii + 300, with 155 figures.] (London: Longmans, Green & Co., Ltd., 1926. Price Rs. 3.)

THE changes in this new edition consist of substituted matter (Indian plants, for example, and common Indian orders), additional chapters on the lower groups, and a revision of the old chapters. This last does not seem to have been carefully done. The price of a compound microscope with 1-inch and  $\frac{1}{2}$ -in. objectives and binoculars is still stated to be about £6 10s. The chapters on Ecology and Heredity are entirely inadequate. The chapters on the lower groups are unsatisfactory, e.g. the drawing of *Spirogyra* is misleading, those of *Selaginella* do not show the typical heterophylly and the ligule. The new drawings as a whole are much below the standard of the older ones, e.g. the drawing of the bundle of the vegetable marrow stem and a similar impressionistic drawing of a sunflower bundle. On p. 69, by some strange oversight, the reproduction of a microphotograph of a root of *Ranunculus repens* from the old edition masquerades in this edition as that of a root of gram (*Cicer arietinum*).

On p. 242 the typical iridaceous floral diagram is given instead of the liliaceous one intended, and next to this is a drawing of a flower of *Gloriosa superba* unprovided with a stigma.

E. M. C.

## ANTHROPOLOGY.

**Our Early Ancestors. An Introductory Study of Mesolithic, Neolithic, and Copper-Age Cultures in Europe and Adjacent Religions.** By M. C. BURKITT, M.A., F.S.A., F.G.S. [Pp. xii + 243.] (Cambridge: at the University Press, 1926. Price 7s. 6d. net.)

IN the past few years many books have been published giving general surveys of our present knowledge of the Palæolithic period of man's development. The later prehistoric periods—the Neolithic and Metal Ages—have also been dealt with in a manner adapted to the requirements of students of cognate sciences and the general reader. Until the publication of this book, however, there was no adequate account in English of the transitional periods, and in providing one the well-known author of *Prehistory* has rendered another important service to anthropological science. Until quite recently it was generally supposed that the change from true Palæolithic to Neolithic civilisation was of the nature of a mutation, but there can now be no doubt that it was, in reality, a slow and continuous one. The fact that the Mesolithic cultures show unmistakable signs of an Aurignacian parentage and that they are the precursors of the true Neolithic culture can be fully appreciated by the uninitiated reader from the numerous examples of the changing forms of artifacts that are given.

In one way this is a curiously unbalanced book, as by far the greater part of it deals with Western Europe only. As the evidence accumulates

it is becoming more and more evident that the most important problems which the student of the transitional periods has to solve are those connected with the inter-action and synchronisation of the Eastern European and Mediterranean cultures on the one hand, and the cultures of Western Europe on the other. It is at least probable that the eastern area was of greater importance than the western in determining the course of what was then the most advanced civilisation in the world. But the reader of this manual, coming to the subject for the first time, might be led to believe that our half of the continent was no less pre-eminent in those days than it is to-day. For the benefit of such a one, it might have been well to substitute "Western Europe" for "Europe and adjacent regions" in the sub-title. The single chapter devoted to the Mediterranean area could still be included if that change were made. The simple style in which the story is told will make this book of interest to many readers. The addition of a glossary of technical terms would be a help to all.

β.

## MEDICINE.

**Comparative Physiology.** By LANCELOT T. HOGGEN, M.A., D.Sc. [Pp. ix + 219, with 44 illustrations and charts.] (London: Sidgwick and Jackson, 1926. Price 7s. 6d. net.)

DURING the last few decades physiology has, for many, come to mean human physiology, a conception due in part no doubt to the fact that the chief centres of physiological teaching and investigation to-day are the physiological laboratories of the medical schools. The crowded curriculum of the medical student has inevitably led to a greater and greater stress being laid, in the earlier scientific studies, upon the teaching connected with his ultimate preoccupation, the knowledge and care of the human body. Consequently in these centres comparative physiology is chiefly considered in the light of the knowledge it throws upon human physiology. This being the case, there is every reason to welcome the appearance of books which are devoted to a setting out of some of the findings of those who are primarily concerned with physiology from its more general aspect. In the book under review the author extends the contribution to this subject that he and Winton have already made in their *Introduction to Comparative Physiology* reviewed in the October issue of last year. The author's aim here is, as he himself puts it, "to consider what are the characteristic properties of animate systems and inquire how far it is possible to interpret each in terms of known physico-chemical laws." In doing this he hopes "to help the student of zoology to appreciate what is achieved by the application of physiological methods to the study of lower animals, and to widen the horizon of the student of physiology who has not been brought into touch with the diversity of problems which are suggested by a consideration of function in a wider range of animals than those with which he has been accustomed to deal in his medical studies."

*Comparative Physiology* forms the first of a series of Textbooks of Animal Biology edited by Prof. Julian Huxley, and is an excellent introduction to the subject. In these days of preoccupation with human physiology it is undoubtedly enlightening to be made to consider for a time results obtained in connection with other forms of activity than those exhibited in man. There are sections devoted to the consideration, from this wider aspect, of studies of movement, secretion, respiration, nutrition, circulation, endocrine co-ordination, nervous conduction and excitation, behaviour in animals, reproduction, inheritance and development. The author also gives a good bibliography of the subject for the last fifteen years, bearing in mind the encyclopædic nature of Winterstein's *Handbuch der Vergleichende Physiologie*, which gives a very complete account of the subject up to the year 1912. At

the same time he gives sufficient account of the earlier work to secure the idea of continuity and to inform those who may not be familiar with it. Like its predecessor, the Introduction, it is a very readable and interesting book.

W. CULLIS.

**Annals of the Pickett-Thompson Research Laboratory.** Vol. II. [Pp. iii + 203.] (London: Baillière, Tindall & Cox; Baltimore, U.S.A. The Williams and Wilkins Company. Price, 2 guineas per annum.)

THE first volume of this very handsome publication was mentioned in SCIENCE PROGRESS, Vol. XX, p. 509, Jan. 1926, and the second volume does more than keep up the reputation of the work. The principal feature of it is a series of admirable photographs of various bacteria arranged on fifty plates, each containing six photographs—constituting a bacterial atlas which will be of great service in medical bacteriology. The type is large and fine. There are seven articles, chiefly by D. Thomson, giving a very detailed examination of various bacteria, chiefly of the diphtheritic groups. The researches on measles will be specially welcome. The subscription for these Annals is two guineas post free, and ten dollars for America. Each volume consists of two or more parts, and individual parts are not sold separately. Volumes I and II can still be obtained for two guineas plus postage.

**Animal Parasites and Human Disease.** By A. C. CHANDLER, M.S., Ph.D. Third Edition, revised. [Pp. xiii + 573, with 254 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1926. Price 22s. 6d. net.)

THERE is now a constant flow of new books connected with animal parasites, and some of them are by no means as good as they might be. The one under review aims at presenting "the important facts of parasitology, as related to human disease, in such a manner as to make it readable and useful *not* primarily to the parasitologist, but to the public health and immigration service officers; to the physicians who are concerned with something more than their local practice; to teachers of hygiene . . . ; to college and high school students; to the traveller; and to the farmer or merchant who is interested in the progress of science and civilisation." In other words, the volume is not a formal treatise on parasitology, but an educational one. The principal chapters deal with parasites in general, spirochætes, Leishman bodies, trypanosomes, intestinal flagellates and ciliates, amœbæ, malaria, and other protozoal parasites; and there are seventeen more chapters on numerous other parasitic organisms, blood-sucking flies, etc. There are 253 figures in the text, a list of "sources of information," and a good index of forty pages. What is important, the historical details, though brief, are as correct as brevity will allow. The fact that Castellani first actually discovered trypanosomes in declared cases of sleeping sickness, and especially in the spinal fluid where they had never been discovered before, does not seem to be stated; but the author has conscientiously honoured most of the real workers. The zoological and medical details are as full as the scheme of the book justifies; and we can safely say that, unlike many of the works now appearing, this one will be a very useful addition to the small libraries of medical men in the tropics. The style is very clear and good, and the details are generally up to date.

R. R.

**The Wonders of the Human Body: a Health Reader for Schools.** By MARGARET A. SHUTTLEWORTH. [Pp. xi + 190, with 38 figures.] (London: University of London Press, 1926. Price 2s. 6d.)

THIS little textbook is a very satisfactory attempt to provide a course of biological hygiene for schools. It is more especially suited for girls' schools, and gives a very fair survey of the essentials to healthy living, though some of the generalisations are open to criticism.

W. C. BROWN.

**The Importance of Diet in Relation to Health.** The People's League of Health Lectures. [Pp. xii + 130.] (London: George Routledge & Sons, 1926. Price 3s. 6d. net.)

THIS book is a reprint of six popular lectures given by distinguished medical men and leaders of scientific thought. They deal with both the historical and the scientific aspect of feeding; with vitamins and diseases; infant feeding; the feeding of cattle; and food preservation and adulteration. They deserve to be widely read.

W. C. BROWN.

**A Manual of the Parasitic Protozoa of Man.** By CHARLES F. CRAIG, M.D., M.A. (Hon.), Lieutenant-Colonel Medical Corps, U.S. Army, D.S.M. [Pp. v + 569, with 95 figures.] (London: J. B. Lippincott Company, 1926. Price 35s.)

IN his work on the protozoa Craig practically confines himself to those species parasitic in man, and, accordingly, the result has been a volume of the greatest use to medical practitioners, especially those engaged in tropical countries. The text is clear and well written, and each organism is treated in a most methodical manner. His teaching is in accordance with current belief, and where authorities have been found to differ, he has avoided taking sides as far as possible. It is noteworthy that he includes in the section on the intestinal flagellates his own species *Craigia hominis*, and, further, under malignant tertian malaria, two sub-species, viz.: *Plasmodium falciparum* and *P. falciparum quotidianum*. Concerning these the majority of protozoologists are still in doubt.

Most of the illustrations are reproduced from the works of well-known authorities, and the selection has been carefully carried out. A large number of photomicrographs are used to illustrate the various parasites, and, while some of these are excellent, many, especially those of the intestinal amoebæ of man, are of little use, and do not convey much to the reader. Teachers of medical protozoology will find this compact volume a useful textbook for their students, and medical men a handy work of reference. To all such we recommend it highly.

J. G. T.

## MISCELLANEOUS

**Economic Geography of South America.** By R. H. WHITBECK. [Pp. vii + 430, with 197 figures.] (London: McGraw-Hill Publishing Co., 1926.) Price 17s. 6d. net.)

As the only serious textbook on the subject this work is valuable. It is well written and interesting, and on the purely economic side is full and suggestive, but it contains comparatively little geography. This defect is seen, for instance, in the scanty and sometimes exceedingly elementary treatment of physical factors, e.g. there is no attempt to explain *why* two-thirds of the coast of Chile lacks harbours, in spite of a whole page being devoted to pointing out this drawback (pp. 164-5). The defect is specially noticeable in connection with the industries. A great wealth of detail is given, which is very good in its way, but in many cases it is almost wholly technical; and the geographical causes, if given at all, are mentioned in a casual and unemphasised sentence, of the account of the nitrate industry of Chile (pp. 168-77), or the quebracho industry of Argentina and Paraguay (pp. 218-19). Even in the description of the copper production of northern Chile (pp. 177-83), where the author explicitly states that the geographical factor is one of *place*, he omits to give the reason, i.e. that the ore is a soluble oxide and could only have accumulated in large quantities in a region with an arid climate. This omission is all the more curious since his list of refer-

ences includes Miller and Singewald's *Mineral Deposits of South America*, in which the geographical explanation is fully given.

The above example also illustrates another weakness of the book, the little use that is made of reference material at the author's disposal. For instance, the *Commercial and Industrial Handbook* on Colombia, which the author actually quotes, gives an explanation for the deterioration of coffee in transit which Prof. Whitbeck ignores, in spite of the fact that the explanation is thoroughly geographical, more so indeed than the passage which he reproduces.

The book contains, however, much that is very good. If it were all as good as the account of the Pampa (pp. 222-6), there would be little room for adverse criticism; but even in the sections on the Argentine, which are among the best in the book, there are some startling omissions, e.g. a complete absence of any explanation of the location of the maize belt or of the flax area of Santa Fé. University students will find the book useful, provided they make full use of the wealth of reference material, from which they can fill in the geographical background.

M. SHACKLETON.

**Les Soies Artificielles.** By A. CHAPLET. Second Edition. [Pp. 256.] (Paris: Gauthier-Villars et Cie, 1926. Price 40 frs.)

OF modern industries, the manufacture of artificial silk is one whose development during the last few years has been among the most remarkable. The constant introduction of new and improved methods has rendered the literature of the subject published before the war practically obsolete, and the second edition of *Les Soies Artificielles* has therefore been completely rewritten and quadrupled in size. The volume aims at being a *vade-mecum* for specialists of the subject, and includes in the text a comprehensive bibliography. A detailed account is given of the preparation of artificial silk from the various raw materials and bases, as cellulose, nitrocellulose, acetocellulose, protein substances, etc., a separate chapter with illustrations being devoted to the forms of apparatus used for the different processes of manufacture. Bleaching, dyeing, and dressing are dealt with, and a description given of the properties of different types of artificial silk and the methods whereby they may be differentiated. Artificial silk of one type or another can be used for all purposes for which real silk is suitable, not only for textile manufacture but for such objects as electric lamp filaments and incandescent gas-mantles. The statistics of the rise of the industry provide interesting reading, the most marked increase being from a production of 10 million kilograms in 1913 to 50 million kilograms in 1923, England, France, and the United States being the leading producers.

W. E. BRENCHEY.

**Hertha Ayrton. 1854-1923. A Memoir.** By EVELYN SHARP. [Pp. xiv + 304, with illustrations. (London: Edward Arnold, 1926. Price 15s. net.)

THE memoir of a distinguished woman written by a friend, herself a writer of distinction. The writer wonders whether she, as a woman without special scientific knowledge, could properly record the life and work of one whose name in the future will chiefly be remembered for the scientific work she did. No reader of this memoir will feel any such doubt. Mrs. Ayrton's work—work which made her an acknowledged authority on the electric arc, brought to light remarkable new phenomena in wave-motion, and won her one of the Royal Society's medals, is rightfully given the greater space in the book—rightfully since it is certainly as a physicist that Mrs. Ayrton would wish the general public to remember her. At the same time the writer shows

with great sympathy and charm the many other sides of this brilliant, generous, and affectionate woman, with her passion for justice, her power to awaken and retain great friendships, and her capacity for giving, which secured her so much happiness in her personal relationships, whether as friend, sister, daughter, mother, or wife.

Like so many women who have taken up scientific work, she found it difficult to give to the work, which meant so much to her, the undivided attention which would have enabled her to solve the many problems she set herself and which were well within her powers. As Miss Sharpe says, "she was physicist, suffragist, democrat, humanitarian, and very human woman, but never any of these things in water-tight compartments." She was indeed fortunate in that her husband Prof. Ayrton, the well-known physicist, sympathised deeply with her in her work, was eager that she should carry on with it, and was so anxious that she should be recognised for her own work that he refused to carry on collaborative work until his own illness made it necessary for the completion of investigations he had undertaken for the Government's authorities as to ways of improving search-lights, and in which Mrs. Ayrton's own special field was involved. But even with this great asset, to read the story of this life is to be made to realise the limitations imposed on a capacity that could have brought even more valuable contributions to scientific knowledge than it did, not by the fact that as an individual she had certain duties and responsibilities, since these or similar claims must be recognised as inevitable in the case of any human being, but because she was a woman. How passionately she longed for freedom from outside distractions to carry on her work just as a physicist, and yet how inevitably she was driven by her keen sense of justice into the fight to secure for women the rights, responsibilities and opportunities already given to the other half of humanity.

During her life she was brought into touch with many outstanding personalities, some of whom, such as George Eliot, J. R. Green, William de Morgan, William Morris, and the early pioneers of advanced education for women, Barbara Bodichon, Emily Davies, Madame Belloc (all of whom helped her to become one of the early students of Girton College), take us back to an era very different from that of our own days and from those of the suffragist movement in which she was a prominent figure. The dedication of this book to Madame Curie recalls another close friendship.

Her scientific work had always a practical bent, and her latest invention, the "Ayrton Fan," which saved many thousands of soldiers from gas-poisoning, seems to have possibilities in the direction of rapidly changing the air in a chamber which might be applied to the ventilation of such closed spaces as sewers, mines, and such public buildings as theatres and cinemas, where ordinarily the changing of the air seems difficult to accomplish and where it is so essential for life and comfort. Had she lived and been able to carry on her work it seems probable she would have been the first woman to receive the proud honour of Fellowship of the Royal Society—an honour not open to women in 1902 when her candidature was suggested, but now opened by the passing of the Sex Disqualification (Removal) Act.

The memoir is worth reading, not only as the record of the life of a very remarkable woman, but as giving an interesting light on some phases of social development in our country during the last half of the last century and the first quarter of this.

W. CULLIS.

**A French Reader for Science Students.** By JETHRO BITHELL, M.A. [Pp. 144.] (London: Methuen & Co. Price 3s. 6d. net.)

AN excellent collection of extracts from contemporary French science has been compiled by Mr. Bithell. The book is much more than its title suggests,

a mere Reader in French with the purpose of creating a scientific vocabulary. It really achieves what the compiler has set out to do, namely, to give a picture of contemporary French science. The selections are divided into six sections, namely, Chemistry, Physics, Mathematics, Geology, Botany, and Zoology, and are all remarkably well chosen.

W. C. BROWN.

**Experimental Science for Schools.** Parts II to III. By DAVID B. DUNCANSON, M.Sc. [Pp. 256, tables, index, and 52 illustrations.] (London: G. G. Harrap & Co. Price 3s. 6d. net.)

MR. DUNCANSON has compiled a course in Elementary Physics and Chemistry of a thoroughly practical character, so arranged as to create a habit of scientific inquiry and thought. The chapter on Surface Tension is especially good, though teachers may prefer to postpone the subject to a later stage. Questions are constantly inserted as to matters of daily experience and observation, and they are very stimulating in character. In the effort to be "chatty" some rather loose expressions are used, even in the titles, as for instance, "Have all crystals got water of crystallisation?" Nevertheless, it is an excellent book, on right lines.

W. C. BROWN.

**Science, Religion, and Reality.** Edited by JOSEPH NEEDHAM. [Pp. 396.] (London: The Sheldon Press, 1926. 3rd impression. Price 12s. 6d. net.)

In this book a number of writers discuss the relations of Religion with Science and with modern thought in general, each one dealing with a special aspect on which he is able to speak with some authority. Three essays treat the matter historically; the others are concerned with the philosophical aspects. Finally, Dr. Inge provides a summary and conclusion. The essays are all learned, judicious, and readable. They show a degree of co-operation and unity of purpose which is not always seen in compilations of this type, and for which the Editor deserves our thanks. Without prejudice to the other writers, it may be suggested that most readers will find the greatest interest in Prof. Eddington's witty contribution on "The Domain of Physical Science" and in Dr. Inge's weighty and characteristic concluding essay.

To the present reviewer the article of greatest interest is Dr. Singer's account of the historical process which led in the seventeenth century to the clash between organised religion in defence of the ancient cosmology and the new physical science in defence of the Copernican theory and its developments. This opposition, as Dr. Inge points out, is more fundamental than any dispute over evolution, and the points at issue are to this day not always squarely faced by theologians intent on preserving traditional views.

There is no inherent reason for a conflict between religion and science any more than between eating and drinking. Both are natural human activities, the offspring of the natural impulses of reverence and of curiosity. The two activities are harmoniously combined in some men, though perhaps in few. Conflict comes about because the religious and the scientific tend to philosophise or to generalise from their particular sphere; and the philosophies may be antagonistic. Even so, they are not necessarily antagonistic, as is emphasised by several writers in this book. The chief actual cause of conflict has not been purely philosophical. The religious life has always externalised and embodied itself in institutions. The institutions tend to be intensely conservative, honouring tradition more than truth. Scientific men, on the other hand, are bound to be revolutionary; to scrap ruthlessly whatever theory is found to contradict the facts, and adopt a

new one. The general attitude of the scientific investigator, as such, is opposed to the general attitude of the religious institution, as such; so that in course of time, grounds of conflict are sure to appear. In the past, religious institutions have been politically powerful and scientific men weak, so that the former have been the persecutors. Circumstances may change and our descendants may witness a tribunal of the Royal Society condemning religious enthusiasts for denying the Quantum Theory. But if the discussion is approached in the spirit of the writers of this book, diverse as they are in training and outlook, we may consider the possibility to be remote.

A. D. RITCHIE.

**Tierpsychologie, vom Standpunkte des Biologen.** Von Dr. F. HEMPELMANN. [Pp. viii + 676, with 134 figures and 1 table.] (Leipzig: Akademische Verlagsgesellschaft, M.B.H., 1926. Price geb. Mk. 36.

GREAT BRITAIN has strong claims to the title of the birthplace of comparative psychology. We see the first awakening of interest in it in the words of Locke and Hume. In the nineteenth century a splendid start was given to the young science, and, at a later period, two more names, those of Prof. Lloyd Morgan and L. T. Hobhouse, stand out. But in spite of these auspicious beginnings animal psychology has not continued to flourish on our soil. Even at the present day, when so much interest in experimental biology is awakening in this country, the study of the behaviour of organisms has so far claimed little attention. Dr. Hempelmann has now furnished us with a comprehensive treatise on the subject which provides a guide to the enormous amount of work already done in Germany and America and will perhaps do something towards renewing interest among British biologists. Dr. Hempelmann's aim has been to provide a much-needed general view of the whole field which will at the same time not omit anything essential involving related subjects which have already received detailed treatment elsewhere—such as oecology and the physiology of the sense-organs.

The book is divided into two parts: a special part describing the results of investigations on representatives of the principal classes of the animal kingdom; and a general part devoted to a discussion of the following questions: (1) "besondere Fähigkeiten," such as vision, orientation and homing, play, etc.; (2) the *Gestalt* principle; (3) the emotional life of animals; (4) sleep and hypnosis; (5) the psyche and the nervous system; and (6) the history and method of animal psychology. This general part occupies about one-third of the whole book and consequently the topics with which it deals are not discussed in great detail. But it is impossible to discuss such a question as the nature of the psyche and its relation to the nervous system solely from the standpoint of animal psychology. It involves at least some clearing of the ground from the point of view of what Prof. Hempelmann's countrymen call *Erkenntnislehre*, as he himself recognises when he says (p. 618): "Ein Vergleich hinkt, und das um so mehr, wenn wir jetzt zur Veranschaulichung des Begriffes Epiphänomen zwei völlig heterogene Gebiete vergleichen müssen, während wir in den Vergleich in dem einen Gebiete bleiben." The chief value of such discussions in a book of this kind would seem to be to make clear the pitfalls of such problems, and to delimit the field of animal psychology as a science. In reference to this, Dr. Hempelmann remarks in his preface: "Die Lösung der Frage nach die Ursachen und Gründen des teleologische und regulatorische Geschehens dagegen ist nicht, wie wohl mitunter angenommen wird, eine Aufgabe der Tierpsychologie, ebensowenig wie sie die Aufgabe der menschlichen oder der vergleichenden Psychologie ist." The book is beautifully illustrated and contains a good bibliography. An English translation would be useful in view of the absence of such a recent general work of the kind in English.

J. H. W.

**The English Brass and Copper Industries to 1800.** By HENRY HAMILTON, M.A., D.Litt. With an Introduction by SIR WILLIAM ASHLEY. [Pp. xxvi + 388, with 8 plates.] (London: Longmans, Green & Co., 1926. Price 18s. net.)

THIS history deals in adequate and scholarly fashion—stressing perhaps a little heavily the share of Birmingham—with the development of the brass and copper industry in England from Elizabethan times to the end of the eighteenth century. For purposes of national defence Cecil, Elizabeth's great adviser, was anxious to foster the brass and copper industries. The encouragement given by him to foreign capital and skill; the establishment in Keswick, Cornwall, Devon, and elsewhere of mining and smelting industries run by German capitalists and workmen; the old tale of the growth and abuse of monopoly; the breaking down of such abuses by the men of Birmingham in the eighteenth century,—all this story is told by Dr. Hamilton, and its economic implications worked out, with a skill and a mastery of the original sources which cannot but command admiration.

For Dr. Hamilton, while mindful that his story is essentially an economic one, by no means omits the human and the scientific side. The industrial organisation of the sixteenth, seventeenth, and eighteenth centuries, the ebb and flow of wages and of employment, the problems of transport, the type of goods manufactured, and even the trade methods employed are discussed in full detail, which detail, however, is never permitted to accumulate to the detriment of the main interest.

There is a curiously modern "capital and labour" flavour about the whole story—a flavour the absence of which one tacitly assumes in industrial relations prior to the Industrial Revolution. But we do not travel *per saltum* from mediæval to modern times. Professor Lamb has somewhere remarked that, with increasing delicacy of the apparatus used, there at times goes an increasing indefiniteness in our sense of the precision of the physical magnitude or concept involved. It was easy to show that the earth was spherical in the early days of geodesy; but as geodetic methods become more and more precise, we go from sphere to spheroid, from spheroid to ellipsoid, and finally, almost as a counsel of despair, to geoid.

So here in human life and industry we form concepts of an Industrial Revolution, a Renaissance, an Augustan Age, each serving to delimit sharply two regions of human activity. But increasing knowledge shows that Augustans lived before Augustus, and that Renaissance influences were at work long before the magic year 1453. And the development of these industries in Elizabethan days reveals financial, mechanical, and transport operations carried out on a scale that demanded large supplies of capital for funds and for wages. The Industrial Revolution was surely evolving through the seventeenth and eighteenth centuries.

The volume is illustrated by an interesting series of plates showing mainly sixteenth-century smelting and foundry work. Of these the frontispiece, taken from the *Fodina Regales* of Sir John Pettus, exhibits the arms of the Society of the Mineral and Battery Works (1568). The blazoning is calculated to set on edge the teeth of a devotee of the "science for fools with long memories." As here transcribed, it reads sheer nonsense. Whether the errors (which are mainly due to faulty punctuation) lie with Sir John or with the transcriber, matters little, but it would be well either to indicate the source of, or to amend the errors in, the new edition which we trust will soon be demanded.

A work such as this is eminently fitted to to-day's needs, and it will be a matter for congratulation when the development of our great textile and iron industries is treated with such a philosophic outlook and with similar industry and ability.

A. F.

**Imhotep.** By JAMIESON B. HURRY, M.A., M.D. [Pp. xvi + 118, with 17 illustrations.] (London: Oxford University Press, 1926. Price 7s. 6d. net.)

IMHOTEP, "the first figure of a Physician to stand out clearly from the mists of antiquity," as Sir William Osler has described him, emerges in Egyptian history in the reign of King Zozer of the 3rd Dynasty. He enjoys the rare distinction of having been deified after his death although not of royal blood, and he reappears at intervals in the records of ancient Egypt first as a demi-god and eventually as the God of Medicine during a period of some 3,000 years. The known facts regarding this remarkable man who made such an impression upon his countrymen by his labours as a physician as to merit such attention in the succeeding centuries must be of special interest both to archaeologists and students of the history of medicine, and these facts are admirably presented to us by Dr. Jamieson B. Hurry in his monograph *Imhotep, the Vizier and Physician of King Zozer*.

The name Imhotep, meaning "he who cometh in peace," is appropriate to his profession, but his achievements were not confined to medicine; he seems to have been a man of profound learning and varied attainments. Probably he designed the remarkable step-pyramid of Sakkarah, near Memphis; his other activities included those of sage, magician, priest, astronomer, and Grand Vizier.

The evidence set out in this interesting monograph seems to establish beyond doubt the author's contention that Imhotep should be recognised as the patronal deity of medicine. Centuries before the Greek conquest of Egypt by Alexander, Imhotep had become generally recognised as the deity of medicine, and the Greeks, who called him "Imouthes," merged him in their own mythical god Æsculapius. The cult of the latter legendary deity goes no further back than the time of Homer, when Asklepios was still supposed to be a practising physician; yet his figure adorns the medical diplomas and the insignia of countless medical societies throughout the world. Most readers of Dr. Hurry's book will agree with him that "surely the time has come to do justice to the venerable figure described in the above pages and to elevate Imhotep to the place of honour which is his due."

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## BOOKS RECEIVED

(Publishers are requested to notify prices)

**The Evolution and Development of the Quantum Theory.** By N. M. Bligh, A.R.C.Sc., A.I.C. With a Foreword by Prof. Max Planck. London: Edward Arnold & Co., 1926. (Pp. 112.) Price 9s. net.

**An Introductory Course of Mathematical Analysis.** By Charles Walmesley, M.A., Assistant Lecturer in the University of Manchester. With a Preface by W. H. Young, Sc.D., F.R.S. Cambridge: at the University Press, 1926. (Pp. x + 293.) Price 15s. net.

**La Série de Taylor et son Prolongement Analytique.** Deuxième édition et mise au courant des progrès récents. Par M. Hadamard et M. Mandelbrojt. *Scientia*, No. 41, Paris: Gauthiers-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 106.) Price 20 frs. net.

**The Absolute Differential Calculus (Calculus of Tensors).** By Tullio Levi-Civita, Professor of Rational Mechanics in the University of Rome. Edited by Dr. Enrico Persico. Authorised translation by Miss M. Long. London and Glasgow: Blackie & Son, 1927. (Pp. xvi + 450.) Price 21s. net.

- Differential Geometry of Three Dimensions. By C. E. Weatherburn, M.A., D.Sc., Professor of Mathematics at Canterbury College, University of New Zealand. Cambridge: at the University Press, 1927. (Pp. xii + 268.) Price 12s. 6d. net.
- Principia Mathematica. By Alfred North Whitehead, Sc.D., F.R.S., and Bertrand Russell, M.A., F.R.S. Vol. II, Second Edition. Cambridge: at the University Press, 1927. (Pp. xxxi + 742.) Price 45s. net.
- Plane Analytic Geometry. By I. A. Barnett, Ph.D., the University of Cincinnati. New York: John Wiley & Sons; London: Chapman & Hall, 1926. (Pp. ix + 269.) Price 10s. net.
- The Internal Constitution of the Stars. By A. S. Eddington, M.A., LL.D., D.Sc., F.R.S., Plumian Professor of Astronomy in the University of Cambridge. Cambridge: at the University Press, 1926. (Pp. viii + 407.) Price 25s. net.
- Modern Astronomy: its Rise and Progress. By Hector Macpherson, M.A., Ph.D., F.R.S.E., F.R.A.S. London: Oxford University Press, 1926. (Pp. vi + 196 with 25 illustrations.) Price 6s. net.
- La Théorie de la Lune. Par H. Andoyer, Membre de l'Institut. *Scientia*, No. 17. Paris: Gauthiers-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 90.) Price 18 frs. net.
- General Physics for the Laboratory. By Lloyd W. Taylor, Professor of Physics, Oberlin College, William W. Watson, Instructor of Physics, University of Chicago, and Carl E. Howe, Assistant Professor of Physics, Oberlin College. London: Ginn & Co. (Pp. vi + 247, with 145 figures.) Price 10s. 6d. net.
- Explosive Reactions in Gaseous Media. A General Discussion held by the Faraday Society, June 1926. (Pp. 252-375.) Price 10s. net.
- Space and Time. By Émile Borel, Professor of the Faculté des Sciences of Paris. London and Glasgow: Blackie & Son, 1926. (Pp. xiv + 234.) Price 7s. 6d. net.
- High Vacua. By G. W. C. Kaye, O.B.E., M.A., D.Sc., F.Inst.P., Superintendent of the Physics Department, The National Physical Laboratory. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1927. (Pp. xii + 175, with 4 plates.) Price 10s. 6d. net.
- Données Numériques D'Electricité Magnetisme et Électrochimie. Rédigées par Docteur A. Buffat, Docteur G.-L. Higson, et K. Gordon, M. Malapert. Extrait du Vol. V. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 753-1270.) Price 56 frs.
- Données Numériques de Radioactivité Atomistique, Électronique et Ionisation. Rédigées par Mlle I. Curie et J. Saphores. Extrait du Vol. V. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 805-843.) Price 28 frs. net.
- Données Numériques de Spectroscopie, Spectres d'Émission, Phénomène de Zeeman, Spectres d'Absorption. Par L. Brünighaus, Préface du Prof. Dr. P. Zeeman. Extrait du Vol. V. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 367-718.) Price 154 frs.
- Données Numériques de Cristallographie et de Minéralogie, par M. L.-J. Spencer, Préface de Prof. Dr. W. C. Brogger. Extrait du Vol. V. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 1285-1403.) Price 49 frs. net.
- The Structure of the Atom. By E. N. da C. Andrade, D.Sc., Ph.D., Professor of Physics in the Artillery College, Woolwich. Third Edition, revised and enlarged. London: G. Bell & Sons, 1927. (pp. xviii + 727.) Price 30s. net.

- Treatise on Thermodynamics.** By Dr. Max Planck, Professor of Theoretical Physics in the University of Berlin. Translated with the Author's Sanction by Alexander Ogg, B.Sc., Ph.D., Professor of Physics, University of Capetown, South Africa. Third Edition. Translated from the seventh German Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1927. (Pp. xiv + 297.) Price 15s. net.
- X-Rays and Electrons: An Outline of Recent X-ray Theory.** By Arthur H. Compton, Ph.D., Professor of Physics in the University of Chicago. London: Macmillan & Co., St. Martin's Street, 1927. (Pp. xv + 403.) Price 25s. net.
- Lectures on Theoretical Physics delivered at the University of Leiden.** By H. A. Lorentz. Authorised Translation by L. Silberstein and A. P. Trivelli. Vol. I. Aether Theories and Aether Models. Edited by Bremekamp, Ph.D. Kinetic Problems. Edited by E. D. Bruins, Ph.D., and J. Reudler, Ph.D. London: Macmillan & Co., St. Martin's Street, 1927. (Pp. ix + 195.) Price 12s. 6d. net.
- Theory of Vibrating Systems and Sound.** By Irving B. Crandall, Ph.D. London: Macmillan & Co., 1927. (Pp. x + 272.) Price 20s. net.
- Introduction to Contemporary Physics.** By Karl K. Darrow, Ph.D. London: Macmillan & Co., St. Martin's Street, 1927. (Pp. xi + 453.) Price 25s. net.
- Recent Developments in Atomic Theory, being the Twenty-eighth Robert Boyle Lecture.** Delivered before the Junior Scientific Club of the University of Oxford on June 4, 1926. By Prof. C. G. Darwin, Tait Professor of Natural Philosophy, Edinburgh University. London: Oxford University Press, 1927. (Pp. 15.) Price 1s.
- Makers of Science: Electricity and Magnetism.** By D. M. Turner, M.A., B.Sc., with an Introduction by Charles Singer. London: Oxford University Press, 1927. (Pp. xv + 184.) Price 7s. 6d. net.
- A Handbook of Organic Analysis, Qualitative and Quantitative.** By Hans Thacker Clarke, D.Sc., F.I.C. With an Introduction by J. Norman Collie, Ph.D., LL.D., F.R.S. Fourth Edition. London: Edward Arnold & Co., 1926. (Pp. xii + 363, with 22 figs.) Price 8s. 6d. net.
- The Anatomy of Science.** By Gilbert N. Lewis, Professor of Chemistry, University of California. New Haven: Yale University Press; London: Oxford University Press, 1926. (Pp. ix + 221.) Price 14s. net.
- Casein: the Preparation, Chemistry, and Technical Utilisation.** By E. L. Tague, Ph.D., Associate Professor of Chemistry, Kansas State, Agricultural College. London: Constable & Co., 1926. (Pp. v + 218.) Price 17s. net.
- Catalysis in Theory and Practice.** By Eric K. Rideal and Hugh S. Taylor. London: Macmillan & Co., St. Martin's Street, 1926. (Pp. xv + 516.) Price 20s. net.
- Hydrogen Ion Concentration: the Significance in the Biological Sciences and Methods for its Determinations.** By Leonor Michaelis, M.D., Professor in the University of Berlin. Vol. I. Principles of the Theory. Authorised Translation from the Second Revised and Enlarged German Edition. By William A. Perlzweig, M.A., Ph.D., Associate in Medicine and Chemist to the Medical Clinic in the Johns Hopkins University and Hospital. London: Baillière, Tindall & Cox; Baltimore: The Williams & Wilkins Co., 1926. (Pp. xiv + 299, with 32 figures.) Price 22s. 6d. net.
- Photosynthesis.** By H. A. Spoehr, Laboratory for Plant Physiology, Carnegie Institution of Washington. American Chemical Society Monograph Series. New York: The Chemical Catalog Company, 19 East 24th Street, 1926. (Pp. 393.) Price \$6.50.

- Données Numériques sur les Colloïdes. Par G. Rebière (Tables Annuelles de Constantes et Données Numériques). Extrait du Vol. V. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 1262-1288.) Price 28 frs.
- Applied X-rays. By George L. Clark, Ph.D., Assistant Professor of Applied Chemical Research, Massachusetts Institute of Technology. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xiii + 255, with 99 figures.) Price 20s. net.
- First Principles of Chemistry. By F. W. Dootson, M.A., Sc.D., F.I.C., and A. J. Berry, M.A. Cambridge: at the University Press, 1927. (Pp. vii + 339.) Price 6s. net.
- A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc. Vol. VII. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1927. (Pp. x + 977.) Price 63s. net.
- Lecithin and Allied Substances: the Lipins. By Hugh Maclean, M.D., D.Sc., and Ida Smedley Maclean, D.Sc., F.I.C. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1927. (Pp. vii + 220.) Price 10s. 6d. net.
- British Chemicals, their Manufacturers and Uses. Being the Official Directory of the Association of British Chemicals Manufacturers, containing a Full List of Members with a Classified List of British Chemicals and a Note of their Applications. London: Ernest Benn, 166 Piccadilly, W.1, 1927. (Pp. 285.) Price 10s. 6d. net.
- Où en est la Chimie Colloïdale. Par Paul Bary. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1927. (Pp. 323.) Price 25 frs.
- The Corrosion of Metals. By Ulick R. Evans, M.A., King's College, Cambridge. Second Edition. London: Edward Arnold & Co., 1926. (Pp. xvi + 259.) Price 15s. net.
- The Igneous Rocks of the Mountsorrel District: their Relationship to each other and to the Charnwood Forest Area. By E. E. Lowe, B.Sc., Ph.D., Director City Museum and Libraries, Leicester. Introduction by Prof. W. W. Watts, Sc.D., LL.D., F.R.S. Leicester: The Leicester Literary and Philosophical Society; London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. 55, with 29 figures and 18 plates.) Price 6s. 6d. net.
- The Geology of South Africa. By Alex. L. du Toit, D.Sc., F.G.S., Geologist to the Union Irrigation Department. Edinburgh: Oliver & Boyd, Tweeddale Court, and London: 33 Paternoster Row, E.C.4, 1926. (Pp. xi + 463, with 39 plates, 64 text figures, and a geological map.) Price 28s. net.
- Metallographic Researches. Based on a Course of Lectures delivered in the United States in 1925. By Carl Benedicks, Fil. Dr. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xi + 307.) Price 20s. net.
- Aluminium: the Metal and its Alloys. By M. G. Corsen, Research Metallurgist Union Carbide and Carbon Research Laboratories, Long Island City. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1926. (Pp. xx + 291.) Price 26s. net.
- Stories in Stone, telling of some of the Wonderlands of Western America and some of the Curious Incidents in the History of Geology. By Willis T. Lee, Geologist, United States Geological Survey, Washington, D.C. London: Chapman & Hall, 11 Henrietta Street, Covent Garden, 1926. (Pp. x + 226, with 32 illustrations.) Price 15s. net.
- Elementary Botany. An Introduction to the Study of Plant Life. By W. Watson, D.Sc., Biology Master, Taunton College. London: Edward Arnold & Co., 1926. (Pp. viii + 368, with 225 figures.) Price 6s. 6d. net.

- Manual of Plant Diseases.** By Frederick Deforest Heald, M.S., Ph.D., Head of the Department of Plant Pathology and Plant Pathologist of the Agricultural Experiment Station, The State College of Washington, Pullman, Washington. London: The McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xiii + 891, with 272 figures.) Price 35s. net.
- Soil Conditions and Plant Growth.** By Edward J. Russell, D.Sc., F.R.S., Director of the Rothamsted Experimental Station, Harpenden. Fifth Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1927. (Pp. vii + 516, with 46 figures and 5 plates.) Price 18s. net.
- Palladin's Plant Physiology.** Authorised Edition in English, based on the German Translation of the Sixth Russian Edition and on the Seventh Russian Edition (1914) of the Textbook of Plant Physiology. By the late Vladimir I. Palladin. Third American Edition, with a Biographic Note, Chapter Summaries, and other Additions by the Editor. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. xxxv + 360, with 174 illustrations.) Price \$4.
- Contributions from the Harvard Institute for Tropical Biology and Medicine.** I. Report on Sugar-cane Borers at Soledad, Cuba, by George Salt, and II. Dry-Season Studies of Can Homoptera at Soledad, Cuba, with a list of Coccids of the District, by J. G. Myers. Cambridge, U.S.A.: Harvard University Press, 1926. (Pp. 110.) Price 11s. net.
- Biology and Human Life.** By Prof. J. S. Huxley, M.A. The Norman Lockyer Lecture, 1926. London: The British Science Guild, 6 John Street, Adelphi, W.C.2. (Pp. 24.) Price 1s. net.
- The Elements of General Zoology.** A Guide to the Study of Animal Biology correlating Function and Structure, with Notes on Practical Exercises. By William J. Dakin, D.Sc., F.Z.S., Professor of Zoology in the University of Liverpool. London: Oxford University Press, 1927. (Pp. xvi + 496, with 252 figures.) Price 12s. 6d. net.
- Données Numériques de Biologie et de Physiologie et Chimie Végétales.** Rédigées par E. F. Terroine et par H. Colin. Préface de Léon Fredericq. Extrait du Vol. V. Tables Annuelles de Constantes et Données Numériques. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1926. (Pp. 1537-1675.) Price 56 frs.
- On Photoperiodism, Reproductive Periodicity, and the Annual Migrations of Birds and Certain Fishes.** By William Rowan. Proceedings of the Boston Society of Natural History. Vol. XXXVIII, No. 6, pp. 147-180. Boston: Printed for the Society, 1926.
- Enzymes: Properties, Distribution, Methods, and Applications.** By Selman A. Waksman, M.S., Ph.D., and Wilburt C. Davison, M.A., M.D. London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1926. (Pp. xii + 364.)
- Wild Birds in City Parks, being Hints on identifying 203 Birds, prepared primarily for the Spring Migration in Lincoln Park, Chicago, but adapted to other localities in North-eastern United States and Canada.** By Herbert Eugene Walter and Alice Hall Walter. Twelfth Edition. Revised. New York: The Macmillan Co., 1926. (Pp. 111.) Price 6s. 6d. net.
- Necturus: a Laboratory Manual.** By L. A. Adams, Department of Zoology, University of Illinois. New York: The Macmillan Co., 1926. (Pp. vii + 72.) Price 4s. 6d. net.
- The Spiny Dogfish, a Laboratory Guide.** By Alvin R. Caton, Ph.D., Instructor in Zoology in the University of Illinois. New York: The Macmillan Co., 1926. (Pp. xii + 94.) Price 5s. net.

- Insects of Western North America.** A Manual and Textbook for Students in Colleges and Universities and a Handbook for County, State, and Federal Entomologists and Agriculturists, as well as for Foresters, Farmers, Gardeners, Travellers, and Lovers of Nature. By E. O. Essig. New York: The Macmillan Co., 1926. (Pp. xi + 1035, with 76 figures.) Price 42s. net.
- How Insects Live.** An Elementary Entomology. By Walter Housley Wellhouse, M.A., Ph.D., Associate Professor of Entomology, Iowa State College. New York: The Macmillan Co., 1926. (Pp. xv + 435, with 295 figures.) Price 21s. net.
- British Ants: their Life-history and Classification.** By H. St. J. K. Donisthorpe, F.Z.S., late Vice-President of the Entomological Society of London. Second Edition, Revised and Enlarged. London: George Routledge & Sons, 68 Carter Lane, E.C., 1927. (Pp. xv + 436, with 18 plates and 93 figures.) Price 25s. net.
- Breeding and Improvement of Farm Animals.** By Victor Arthur Rice, B.Sc., M.Agr., Assistant Professor of Animal Husbandry in the Massachusetts Agricultural College. London: The McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1926. (Pp. xiii + 362, with 112 figures.) Price 17s. 6d. net.
- Health.** A Textbook for Schools. By M. Avery, B.Sc., M.R.San.I. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xx + 213, with 79 illustrations.) Price 6s. net.
- Variations in the Form of the Jaws, with Special Reference to their Etiology and their Relation to the Occlusion of the Dental Arches.** By J. Sim Wallace, M.D., D.Sc., L.D.S., Lecturer on Preventive Dentistry, King's College Hospital. The Cartwright Prize Essay, 1920-1925. London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1927. (Pp. 265, with 84 figures.) Price 17s. 6d. net.
- Anatomy of the Wood Rat.** Comparative Anatomy of the Subgenera of the American Wood Rat (Genus *Neotoma*). By A. Brazier Howell, U.S. Biological Survey. London: Baillière, Tindall & Cox; Baltimore: The Williams & Wilkins Co., 1926. (Pp. x + 225, with 3 plates and 37 figures.) Price 22s. 6d. net.
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